To the editor and Dr. Kern:
We have revised the paper to address the main issues raised by the reviewers, as well as to include minor modifications of the authors. In the following list, we first address the major issues that are of importance in general, then we refer to the issues raised by Dr. Kern. The major revisions of this paper including 4 parts:

(1) We reorganized Section 1 "introduction". We emphasized the significance of retrieving the sea ice thickness distribution from CP SAR both in the ice services and sea ice forecasting. And an overview of the differences between dual-, quad- and compact-polarimetric modes was provided.
(2) In section 3 "simulation", we carefully discuss the effect of snow layer, and we used real scenario roughness parameters to evaluate the rough surface scattering contribution now.
(3) For data process in Section 4.2, the processing chains were described detailed. The procedure of ice drift correction was provided. The method of segmentation level and deformed ice was modified, the new processing chain can exclude deformed ice, and the ice thickness values > 2.2 m has been passed.
(4) The limitations (including snow layer and environment conditions) and outlook of our method was descried and discussed.
Detailed response to the reviewers' comments is below. Reviewers' comments are in italic immediately followed by our response.

General Comments
This paper combines a thermodynamic sea ice model with a model to simulate sea ice radar backscatter to develop a new method to estimate sea ice thickness from spaceborne SAR imagery obtained at C-Band. The method is limited to level first-year ice with snow covers < 20 cm thickness during winter conditions. The method requires polarimetric observations BUT is based on a novel way to obtain the polarimetric information from compact polarization mode SAR imagery. Results of the simulations show convincing evidence that the CP-Ratio is sufficiently sensitive to the sea ice thickness - within certain bounds. Results from contemporary helicopter-borne sea ice thickness surveys and quad-polarization SAR imagery confirms the applicability of the method and illustrates that SIT can be computed from CP SAR imagery also practically.
This is a nice paper. It is easy to read. It is well structured. It touches a hot topic. It contains very interesting information and should definitely be published.
The paper would benefit from - a better introduction which better embeds and motivates the study in the context of research dedicated to sea ice thickness retrieval in general and, in particular, from space-borne (!) SAR. - a more careful treatment of the potential limitations of the method in connection to the model simulations. - an extended interpretation and discussion of the simulation and in particular the experimental results - again in the light of the potential limitations but also in the light of the environmental conditions and the working steps done. The latter seem not to have been described detailed enough at places. - a more thorough discussion /

outlook about how this promising method could be implemented practically.
In this version, we reorganized Section 1 “Introduction”, and we emphasized the importance of retrieving the sea ice thickness distribution from CP SAR in the present demand. Because of the extensive changes we made in the introduction, it is difficult to relate single parts of the modified text to the old text. The new introduction should be read in its entirety. About simulations, we carefully discuss the effect of snow layer. For simulating the rough surface scattering contribution, we now used real scenario roughness parameters. In the experimental results, we included the comparison between simulation and experimental results. Last, a discussion about limitations, outlook of our method was descried.

Specific Comments

P5446, L6: What is meant by "optimal conditions"? Are these environmental conditions?
The "optimal conditions" means optimal sea ice conditions and radar parameters. Thanks, has been changed.

P5446, L7: Please specify what "CTLR" means
Thanks, has been changed to circular transmit, linear receive (CTLR) mode.

P5446, L9: "Sea of Labrador" –> "Labrador Sea" One could add "different" before "empirical".
Done.

P5446, L16-L21: You want to motivate that sea ice thickness is an important parameter to better understand the spatiotemporal development of sea ice and its interaction with the ocean. For this you provide two, relatively old references, which are also not hitting the main point you want to make in your paper: That with spaceborne SAR one would be (perhaps) able to retrieve the sea ice thickness (SIT) distribution at finer spatial scale. I suggest, you try to find references which underline the present-day demand for fine spatial resolution SIT data. Possibly you find these in papers related to regional sea ice prediction for shipping etc.
Done. The introduction has been completely revised. For this part, we have emphasized the importance of retrieving the sea ice thickness distribution at finer spatial scale. And we have added papers related to regional sea ice prediction for marine transportation and offshore operations.

P5446, L22 to P5447, L6: This is an interesting mixture of different types of sensors from various platforms capable to obtain sea ice thickness estimates. I don’t know what drove you to this selection but it is unstructured and not well organized. Again I suggest that you structure the background information given here such that it leads to what you want to sell in this paper. For sure there is not the need to list and explain all SIT estimation sources but the list should make sense. A few comments to these: - Sonars measure draft from which sea ice thickness needs to be estimated via

assumptions about buoyancy, densities and snow load. These exist in the Arctic and Antarctic in moored form and provide excellent information about the temporal development of SIT at one location. In the Arctic they have been also been employed from aboard submarines; the data archive from submarine sonar is actually our one and only data source about SIT in the central Arctic before satellite radar altimeters have been used to retrieve SIT. The one reference used here is not sufficient. - Electromagnetic induction sounders measure the total sea ice + snow thickness. There are sledge-based and fixed-wing aircraft versions of this technique which has been applied by various research groups - predominantly from AWI and in Canada. The two references given here aren't those I would use. In line 26 you switch without further notice to satellite data - leaving out in-situ measurements and air-borne SIT estimations such as provided by the Operation Ice Bridge campaigns. - When writing something about SMOS you need to mention that the SIT retrieval using this sensor is only possible for close to 100% sea ice cover and cold freezing conditions and level sea ice and is limited to SIT below about 30 cm to 50 cm over saline ice and below about 100 cm to 150 cm for brackish and/or MYI. - Then in Line 5 on page 5447 you cite a Kwok et al. paper - being still in the context of the SMOS explanations. This is not the correct reference. - Main sources for SIT retrieval from satellites are: ICESat-1 (this is where Kwok et al. papers fit), Envisat radar altimeter, ERS1/2 radar altimeter, CryoSat-2 and SARAL/AltiKa. Yes, they have problems to allow to retrieve fine spatiotemporal scale SIT ... but these are of different nature than of the SMOS sensor mentioned above. Enough of that. I guess the authors understand that this paragraph of the introduction is far from being complete and does not provide the motivation to the paper it should provide. L25: What is "sufficient accuracy"?
Done. The introduction has been completely revised. All suggestions by the reviewer have been considered. By introducing the limitations of different types of sensors, the motivation of the paper is provided.

P5447, L7: What is "much higher spatial resolution"?
Done. We specified this, range: 1-100 m.

P5447, L8: What is "all day"?
Done. It has been changed to “day-and-night”.

P5447, L9: I guess this "almost all weather conditions" depends on the frequency used, correct? Again: "high spatial resolution" Please specify. Doesn't this come at the expense of the spatial coverage? Or do SAR images cover also swaths of about 1500 km width like satellite passive microwave sensors? You write further down about the limited swath width of quad- and dual-polarized SAR data but it would be nice to have a general info here as well. How about the temporal coverage? Does space-borne SAR allow daily monitoring of sea ice conditions at a certain location? It is important to mention these (current) limitations of SAR in advance. It would also be good to tell the readership what a SAR actually measures.
The information about spatial resolution, swath, temporal coverage and the current

limitations of SAR has been provided.

P5447, L10-18: Yes, a lot of work has been done here. Wouldn't it make sense to be more specific here and list which of the cited studies used L-, C- and/or X-Band SAR data? Which of these are based on satellite data and which on air-borne instruments? Which of these were developed and/or tested on Arctic sea ice? I suggest to spend an "e.g." in both lists of citations, i.e. in L11 and in L13 because there are many more studies. The sensitivity between co-polarization ratio and sea ice thickness for thin ice has been demonstrated e.g. by Onstott 1992 (in Book by Carsey, Microwave sea ice remote sensing). It is the later studies (cited here) which claimed that this is not only valid for thin ice but also for thicker sea ice.
Agreed. We considered these comments and added the required information.

P5447, L13: Do you mind to briefly explain what the co-polarization ratio is? L14: Do you mind to briefly explain what the "alpha angle" is? L19: Would you mind to briefly explain what the "cross-polarized ratio" is?
The co-polarization ratio and cross-polarized ratio are explained, but the alpha angle is not used in the modified text.

P5447, L22-23: Well, this isn't a conclusion really. In order to do so you would need to specify more what all the studies cited did. The benefit of using polarimetric SAR for sea ice classification and SIT retrieval may depend on the frequency and some parameters like the phase difference might be as important as the co- or cross-polarization ratio. I find also this part of the introduction not sufficient. The authors could have worked through the existing literature more carefully to embed their work better into what has been done.
We now specified radar frequencies, ice thickness ranges, and test areas. The major points to relate our work to what has been done before is clear now.

P5447, L24: Would you mind to give the polarizations which are usually used in "quad" and "dual" polarization mode data?
This is now already explained earlier in the introduction.

P5447, L29: I suggest to add an "(see below)" behind "SAR modes" to make clear that it will be explained further down what the CP mode actually is.
Done.

P5448, L10: "... calculated from CP mode SAR data."
Done.

P5448, L23: "first practical radar"? What is this? Is the Chandraayan an Indian satellite?
Yes. We avoid this now and only mention RISAT, ALOS-2 and RCM.

P5448, L26/27: In order to avoid confusion, you could add a sentence that henceforth you will use "CP SAR" when you are referring to "CPLR mode SAR data".
Done.

P5449, Equation (1): You set HV = VH, correct?
Yes. Has been revised.

P5449, L15: Would you mind to explain that the subscript "R" is for right circular polarization?
The description of “R” is added.

P5450, L3: Perhaps "From Eq (3) it then follows that" ... ?
Yes. Done.

P5451, L3/4: One could add the typical RMS height and correlation lengths for this ice type here.
Done.

P5451, L10: One could add examples of the wavelength and SAR spatial resolution used in this paper.
We did.

P5452, Equation (9): What is "Erfc"?
The description of “Erfc” is added.

P5453, L17-L19: These results of the CP-ratio are then in line with the co-polarization ratio of "real" HH and VV-polarized radar backscatter values, aren’t they? One could state this in the text.
Yes. We state this in the text.

P5454, L1: Is there any deeper motivation why the chosen incidence angles in Fig. 1 only vary between 30 and 50? Later, on P5456, L22 you speak of incidence angles between 20 and 60.
We now show results from 20° to 60°.

P5454, L3, How "realistic" is sigma < 0.15? Is this a typical value for level first-year ice?
Sigma is the standard deviation of tan(surface slope angle), i.e. sigma = 0.15 is about 8.5 deg. No data exist about slopes of the facets on the ice. The extended Bragg model is conceptionally used here to provide a theoretical framework – one limitation is that actual measurements of sigma are lacking.

P5454, L5/6: Instead of "in most cases" one could write the conditions under which volume scattering can usually be neglected for first-year ice.

We mention the high salinity of arctic young and first-year ice.

P5454, L11: In the section ending here snow was not mentioned - as well as frost flowers on sea ice which might be incorporated in a snow cover. It would be good if there would be 1-2 sentences at the beginning of this section that it is assumed that the snow is transparent at the frequencies used and that eventual elevated salinities in the basal snow layer due to incorporated frost flowers will not have an influence.
We now explicitly mention these assumptions.

P5454, L24: Here sigma is the "standard deviation of the surface slope variation" while on P5453, L15 it was the "standard deviation of the surface slope"; on P5457 L4 one reads "standard deviation of the slope parameter". Which one is correct? I suggest to use the same description for sigma throughout the paper. Add ", respectively" after "H".
We corrected this ("standard deviation of surface slope") throughout the paper.

P5455, L3-6: You consider frost-flowers as potentially changing the small-scale surface rough- ness but at the same time you don't consider deformation processes. I wonder how this belongs together because frost-flowers grow on thin ice while deformation causing surface roughness components on the order of meters (vertically or horizontally?) are rather a process for thicker ice. Perhaps you could either give a lower SIT above which you carry out your investigations or you could be more specific here and describe how and if frost-flowers and the two deformation processes rafting and ridging are incorporated - if they are.
We now exclude frost flower growth and related surface roughness changes. The reason is that frost flowers require a completely different treatment of the radar backscattering. We explicitly mention this in the text.

P5455, L14: F_C is driven by the upward oceanic heat flux and the temperature at the ice underside. I assume you set it to the freezing point of sea water at which salinity?
The freezing point of the sea water we set as a constant -1.8℃. We explicitly mention this now.

P5456, L4-15: When I read this paragraph I get to know that the sea ice is grown under un- realistically constant conditions (23 days of constant temperature and wind). Does the sea ice grown like this really represent the physical property range that one would encounter in reality? Later in the paragraph you write that rms heights and correlation lengths were set to specific values? I initially hoped that employing the sea ice growth model was mainly motivated by trying to simulate the different (real) surface roughness conditions as a function of varying temperature and wind forcings. Maybe I overread a sentence in which you stated that the sea ice growth model is only used to get one realization of the ice internal parameters?
We note here that the paper demonstrates a first step for retrieving ice thickness using CP. This means that we start with ideal and simple conditions, considering the fact

that we do not have field data of small- and large-scale ice properties available that would allow a very detailed analysis comprising various different scenarios. A question to the reviewer: is there any reference to a study that presents equations of the surface roughness as a function of wind and temperature?? We are not aware of such a study.

P5456, L26-P5457 L2: This statement surely holds but I am wondering whether this couldn't also be related to the discontinuity in the brine volume fraction in Figure 4 c) between day 4 and 5 (which is when H reaches 0.4 m) and the fact that the Cox and Weeks parameterization of the sea ice salinity has a discontinuity at H = 0.4 m.
Yes, we made this clear in the text

L5457, L5/6: "contribution of the ice-surface slope" ... perhaps you could also here stick to the same terminology that is used for sigma because the above phrase reads like it could be one small or large but constant surface slope but what you mean is the variability of the surface slope, am I correct?
Yes, the reviewer is correct. We have corrected this throughout this paper.

L5457, L5-8: I have difficulties to "read" the statement made here from Figure 6 for the entire SIT range shown. For SIT values up to the discontinuity at about 0.4 m indeed the CP-ratio decreases more with increasing SIT at low sigma than at high sigma. But at larger SIT values one can hardly see any difference in the slope of the graphs shown in Figure 6. How would Figure 6 look like for incidence angles 40, 50 and 60?
We also shown the case of 40° incidence angle, and we rephrased the text. The sensitivity of CP-Ratio to ice thickness is less at larger values of sigma. Given the same sigma values, the sensitivity of CP-Ratio is higher at larger than at smaller incidence angles. We mentioned those facts in the text.

L5457, L13: This reads fine but one could have added that the sensitivity is substantially larger for thinner sea ice (< 0.4m) than for thicker ice. Furthermore it becomes increasingly weaker the thicker the ice gets; the slopes in Figure 6 and 7 do not change even though the x-axis is in log scale. The CP-ratio changes between 0.5 m and 0.7 m SIT approximately twice as much as between 1.0 m and 1.2 m.
We mention this explicitly.

L5457, L25/26: I suggest to write "such soundings" when referring to earlier papers than the Prinsenberg et al. one because you are referring to the accuracy of this measurement setting in more general here.
Done.

P5458, L4: You could add that the ground-penetrating radar allows to identify the ice-snow interface - and perhaps also the air-snow interface although the return is possibly weaker and the laser altimetry works better to define this surface - and

therefore allows to retrieve the snow depth on sea ice.
Done.

P5458, L10/11: Where were the SAR images obtained from? In which processing step were they obtained (georeferenced, orthorectificed, noise reduces, calibrated ...?)
They were ordered via MDA. Processing steps are now described.

P5458, L11: Perhaps write "field survey" instead of "measurement"? Does your readership know what a Pauli RGB image is?
Regarding "field survey": done. About "Rauli RGB", we think not all readers will know what in detail a Rauli RGB image is. In this case we expect that the readers check themselves since this term is explained in many textbooks about polarimetry and decomposition.

P5458, L22: "footprint of EM" –> "footprint of the EMS"
Done.

P5459, L1: How were open water regions defined?
The definition of water regions focused on dark areas, and the interpretation was based on the backscattering signature and texture features. We note that areas with a thickness < 0.1 m (measured by the EMS) are anyway not considered in the analysis, which alleviates any potential errors in the detection of open water areas. This is mentioned in the text.

P5459, L5/6: Would you think that it might make sense to share the criteria used to exclude deformed ice from the analysis by using the laser data? I can image that you computed something like standard deviation of the surface slope and correlation length?
The criteria are described in step 4 of the processing chain, section 4.2.

P5459, L7-10: I have few questions here: - How accurate is the GPR-based snow depth retrieval over level sea ice? - Does the history of the snow pack development suggest that it is a "smooth" one without substantial changes in density and/or grain size due to melt-refreeze events and/or depth hoar growth? - Would you mind to share how many data points you needed to exclude due to too high snow load? This last question also applies to the surface types left out from step 3 of your processing chain. I see that you tell at the end of this section that the total length of the useful profiles is 10 km (702) samples. OK, but what was the original number? Where 1, 5, 10 or 50% of the original data discarded? This is an important information for the planning of future surveys.
The information of GPR-based snow depth retrieval (section 4.1) and snow properties (section 4.2) are now provided. The reviewer shouldn't forget that we rely on the information given in the field survey report. We provide information about the percentage of data points that could not be used since they did not meet the criteria

given in the processing chain section 4.2.

P5459, L12/13: How did you decide whether there is just one ice type in these areas?
Using the segmentation procedure, it is rather to segment the radar signature into homogeneous area (in which the radar signatures of all pixel are statistically equivalent). We assume that each segment represents homogeneous ice conditions. We mention this in the text.

P5459, L14: I don't fully understand the reason for this 13 x 13 pixel window. All helicopter- based data are only obtained along the track, right? I recall: Laser every 3-4 m with a footprint of several centimeters; EM every 3-4 m with a footprint of 20 m; GPR every 1-1.5m with unknown footprint. The SAR images have 8 m pixel resolution. Therefore we have 4 different sample spacings and I am wondering to which the 13 x 13 pixel window applies. As you write that this corresponds to 50 m on the ground I assume that you are taking the sampling distance of EM and laser as the basis. But this means that you average over i) 6-7 SAR pixels, ii) 13 completely independent laser samples, iii) 13 highly correlated EMS samples.
We explained this in more detail, see step 5 Section 4.2. The window is used for segmentation of the SAR images.

P5459, L20-21: It would be good (see above) if you can underline that the chosen sea ice patches have a sigma < 0.15.
How? We do not have corresponding field data. We think that the helicopter measurements are too coarse (flight attitude and altitude variations) and the laser accuracy not high enough to get reliable values on sigma (slope).

P5460, L7-9: This one way of seeing it. One can also say: Figure 10 illustrates that with a CP- ratio of 0.4 it is very likely to obtain a sea ice thickness between 5 cm and 20 cm. With a CP-ratio of 0.2, however, SIT values can range between 0.3 m and 3.5 m. ... How much work would it be to derive a plot where you show a histograms of SIT values for CP-ratio bins? I mean, the measured parameter is the CP-ratio and it would be very informative to see down to which CP-ratio value one has a reasonably narrow SIT range and/or a reasonable concentration of the single SIT values around an average SIT value (would also apply to Figure 11 perhaps). There is an inconsistency in SIT ranges shown in Figures 10-11 (start at 0.0 m) and those given in Figures 5-7 (minimum SIT = 0.2 m). This leaves the question open whether the observed further increase of CP-ratio towards smaller SIT values holds also from the theoretical considerations shown in Figures 5-7. Yes, I am aware of the fact that you discarded observed SIT < 0.1 m because of the limitations of the EMS system. This leaves the range between 0.1 m and 0.2 m open for discussion - in particular in the light of the slight discontinuity in the CP-ration SIT relationship at SIT values < 25 cm towards incidence angles of 50 and 60.
We think that Figs. 7-9 offer the opportunity to estimate the range of uncertainty in thickness retrieval because of unknown small-scale roughness and surface slope, and

– if a snow layer is present – because of the unknown incidence angle on the ice surface. We do not present a new figure because we have already many of them…
Please note that the x-axes scales in Figs. 5-7 are logarithmic, and in Fig. 10 the x-axis is in linear scale. We do not understand the point addressing the "slight discontinuity" at SIT values < 25 cm.

P5460, L8-11: You mention the presence of multiyear ice (MYI) here as one of the possible factors for the reduced sensitivity to SIT shown in Figure 10. Do you have evidence that MYI was indeed present in the region of interest? I mean, MYI does sometimes drift along the Western shors of the Labrador Sea southward. But it would be good if you could underline - e.g. by means of Canadian Ice Charts whether this really is the case. If not then your process to discard deformed sea ice described earlier did not work properly. In general, I am quite surprised by the relatively large amount of sea ice thicker than about 1.2 to 1.6 meters which I would assume is the maximum possible thermodynamically grown first-year ice in that region - except perhaps in some of the enclosed bays with fast ice coverage. But also there I doubt that SIT is exceeding 2 meters from pure thermodynamic growth. In addition, in your explanation why MYI causes a reduction in the sensitivity of the CP-ratio to SIT you only comment on the lower and vertically constant salinity but you do not comment on the increased porosity which causes a substantial amount of volume scattering which eventually exceeds the fraction of surface scattering.
Because of the new processing chain and the careful separation of level and deformed ice, we did not obtain ice thickness values > 2.2 m for the profile data that passed the threshold in the processing chain. Hence we can exclude the presence of MY ice along our profiles.

P5460, L17: I suggest to write 0.05 m and 0.78 for RMS error and correlation, respectively. I wouldn't give the RMS error with more decimals than the accuracy of the retrieval could theoretically by on average and I don't think that you are able to retrieve SIT with a better accuracy than 1 cm.
Agreed. We have corrected it.

P5460, L18: I don't see a reason why the "and can hence be ..." part of the sentence needs to be in "( )".
Done.

P5460, L20: I suggest to refer to Table 3 here. Figure 11: What could be the reason that the regression lines have a larger distance for the two higher incidence angle cases (42 and 49) with an incidence angle difference of just 7 than for between the lines for 29 and 42 incidence angles with an almost twice as larger incidence angle difference?
Table 3 has been referred and the figure has been updated.

P5461, L6-14: - I assume that path-3 which is overlapping with both image #1 and *image #2 is the one from which the EMS data are used here. This needs to be written and also referenced to Table 3 and Figure 8. It needs also to be mentioned that you limited the SIT retrieval using the CP-ratio to those 50 m transect pieces in the SAR image which fulfil the criteria mentioned on page 5459. - How many data points to we see in Figure 12? - As the number of data points is relatively small: It would not hurt to give an uncertainty estimate for each of these data points, i.e. an error bar. I mean, the agreement is perfect but would get even higher credibility with error bars. - I would write 8 cm and 20% instead of 8.05 cm and 19.95% for the absolute and relative RMS error, respectively. Does the mentioning of the relative RMS error imply that the absolute RMS error is 0.2 m at a SIT of 1.0 m and 0.02 m at a SIT=0.1m? If so, then I recommend to mention this explicitly.*

Now, we consider all the comments, explain these explicitly, and the figure has been updated.

P5461, L23: If I have understood it correctly, then you evaluated your method with one SAR image (image #2) not with several ones.

Two images (#2 and #3) were used to evaluate our method. We mention this in the text now.

P5461, L25 - P5462, L1: see above with respect to number of decimal digits.

Done.

P5462, L1/2: Yes, I agree, the method is useful but it is essential to mention i) during freezing conditions, ii) for snow depth < 0.2 m, iii) at C-Band frequencies.

Agreed. We have mentioned them.

P5462, L3-7: This paragraph might need to be re-written depending on your answers towards comments towards L8-11 on P5460 and L5-6 on P5459.

This sentence has been removed.

P5462, L8 - P5463, L3 and Figure 13: In the light of the main results of the paper, of the need to extend the introduction / motivation and being a bit more detailed here and there when it comes to the interpretation of the results I suggest to skip this part and figure.

We have skipped this part and figure

P5463, L3-9: You could be more specific here and perhaps mention Sentinel-1A/B? You could also comment on the applicability of your method to data from SAR sensors operating at other frequencies such as L-Band (PALSAR) or X-Band (COSMO-Skymed, TerraSAR-X).

We mention satellites with CP-capabilities in the introduction. Any comments regarding other frequency bands and their potential with CP are speculative at this stage and may be the topic of a future study.

P5463, L9: I am sure more can be said in this section. I am naively giving a few comments / asking a few questions in the following to show you how - at least my brain - would work. - You test your algorithm for a SAR image subset from which you know from auxiliary data where you have level first-year ice and where your snow depth is < 20 cm. What actions would be required to simply take the CP-ratio of an entire SAR image to derive the SIT? This is a question of practical matters. Where would you get the required information from? How would you deal with the incidence angle range encountered across the 350 km wide images? How would be a practical implementation look like? Would one need to classify the image first? Would one carry out a correction for the incidence angle variation before applying your method? Would one need to find an empirical relationship for every SAR image or is there the potential to prepare a look- up table for these? - What are the limitations of your approach with regard to processes changing the snow properties towards being less transparent? What are the limitations of your approach with regard to ocean-sea ice-snow interaction processes where the basal snow layer properties / ice-snow interface properties are changed e.g. by re- freezing slush, wicked up brine, hoar frost development etc.? - What I also miss is more discussion about whether the environmental conditions and or the choice of the model parameters could have had an impact on the results. Examples for this could be the very constant ice growth simulated, and the varying differences between EMS measurements and SAR image acquisitions.

We agree that practical issues need to be solved and discussed in more detail. The reviewer can be sure that our brains work in similar ways. However, our paper is already lengthy, and we are of the opinion that the practical issues have to be addressed in a separate study.

Regarding the effect of environmental conditions, we touched a few details about the influence of snow but note that this issue needs to be addressed also in more detail in future work.

The choice of the models (which may be even more critical than the choice of model parameters) again is worth a separate study. In this case, we acted as “end-users” who need to decide which model is most appropriate in view of the parameters that can be determined by measurements.

P5464: L14 & L19 % L28: "T." –> "Trans.", "Sens." missing –> this occurs on the other pages as well. L18: "olarimetric" –> "polarimetric"

Done.

Figure 1 & 2 & 4: I find the font size of the axes annotations and the legend quite small - in particular in comparison to the size of the figures.

Done.

Figure 6: I suggest to change the first sentence of the caption to: "Sensitivity of the CP-ratio to the standard deviation of the surface slope sigma (x-axis ..."

Done.

Figure 7: - Why is this for 40, 50, and 60 incidence angles? What about 20 and 30 which are used in Figure 6? I suggest to change the first sentence of the caption according to the stype in Figure 6: "Sensitivity of the CP-ratio to small scale surface roughness (x-axis ..." - I find the blue and black lines very difficult to discriminate. Perhaps you could use cyan instead of blue?
We now show results from 20° to 60°, and all the comments are considered.

Figure 8: - You could have chosen a Canadian Ice Service Ice chart as a background or something else other than Google maps. Or a simple sea ice concentration map. Perhaps a more simple map with land, coast and open water without the unnecessary details given in the Google maps about topography on land and under water would do it as well. I find it confusing this way. - If you keep the map as it is I recommend to choose grey frames for the SAR images and then draw the path-8 in white. It will be visible better against the grey texture of the SAR image in the background. - I would also give the day and time of the SAR image acquisition in the figure caption. Or you refer the Table 3.
The figure has been updated, and all the comments are considered.

Figure 9: You could be more specific about the "averaged" in the caption. If I have understood it correctly then every single SIT value is representative of a 50 m long transect piece.
Yes. The figure has been updated.

Figure 10: - You could also use a logarithmic X-axis scale like in Figures 5 to 7 (this applies also to Figure 11). - What is the number of data points shown? 702? - I would only shade the confidence interval and don't show the bordering dashed lines. Caption: I suggest to write: "CP-ratio derived from the SAR images as a function of the sea ice thickness derived from the helicopter-borne field survey. The red line denotes the logarithmic best-fit regression derived from the data. The shaded area corresponds to the 90% confidence interval around the regression."
This figure has been removed.

Figure 11: Here the discrimination between blue and black is easier due to the shading of the confidence intervals (compare my comment to Figure 7) - Why did you use 50% for the confidence interval here and not 90% like in Figure 10? - There is another very narrow, darker shaded area around the regression lines. What is this?
The figure has been updated.

Figure 12: Fonts are clearly too small.
Corrected.

Typos: P5447: L8: "Synthetic aperture radar" –> "Synthetic Aperture Radar" P5448: L4: "making it is well" –> "making it well" P5451: L12: "whereat" –> "where at"

P5457: L13: "underformed" –> "undeformed" P5458: L17: "time difference" –> "time differences"

Done.