To the editor and reviewer 2:
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 reviewer 1. 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 provides, to my knowledge, the first attempt at retrieving sea ice thickness from (simulated) compact polarimetric (CP) synthetic aperture radar (SAR) data. The authors introduce a CP parameter, the CP-Ratio, which shows considerable promise for the retrieval of level first-year sea ice (FYI) thickness from C-band CP SAR data. The authors provide the theoretical framework to demonstrate that the CP-Ratio is sensitive to the dielectric constant and surface roughness of sea ice and to the incidence angle of observation. Through numerical model simulations, it is shown that over level FYI the CP-Ratio is sensitive to the dielectric constant, which is primarily a function of salinity, which in turn is strongly related to the ice thickness, assuming purely thermodynamic growth. Empirical relationships between the CP-Ratio (calculated from quad-pol RADARSAT-2 images) and airborne measurements of FYI thickness (derived from electromagnetic induction sounding) are shown to have a strong correlations using an exponential fit. Using a single RADARSAT-2 image as validation the authors demonstrate that their method can produce accurate sea ice thickness retrievals.
This paper is well suited for the scope of TC, and presents a novel method for inverting FYI thickness from SAR data that may have significant impacts on the sea ice community, particularly for operational sea ice monitoring. I believe it represents significant progress in this field. However, the paper requires some improvements:
1) The authors need to better define the target audience of this work in the introduction and need to outline relevant user requirements (e.g. spatial resolution,

temporal revisit time, swath width etc.) for SAR derived ice thickness products. Ice services, who currently utilize SAR data in an operational context to map ice conditions in near-real time are the obvious stakeholder that would directly benefit from SAR derived ice thickness estimates from CP SAR data. This should be outlined at the very beginning of the introduction. Other possible user groups (e.g. sea ice modelers/forecasters) also need to be identified. The authors repeatedly mention that higher spatial observations of ice thickness are needed, but not all applications (e.g. sea ice climate modeling) require data at the high spatial resolution provided by SAR observations. Revisions are also needed in the conclusions to directly relate the significance of the results to the various possible users of SAR derived ice thickness products.

2) Compact polarimetry is still a relatively novel method of SAR data acquisition (at least for Earth Observation SAR missions) that many readers are likely to be unfamiliar with. It would be beneficial if the authors provided a brief overview of the differences between dual-, quad- and compact-polarimetric beam modes earlier in the introduction to familiarize the readers with the benefits of CP beam modes. In particular, the authors should explain how CP is able to provide increased swath widths and spatial resolution relative to 'standard' quad-pol data.

3) Further discussion on the impact of sea ice drift on the results is required. The authors do acknowledge that ice drift is likely to have an impact on the correlations observed between the CP-Ratio and airborne ice thickness measurements; however, they do not attempt to apply a correction to account for the ice drift between the acquisition times of the SAR data and the airborne electromagnetic induction sounding (EMS) data. Presumably this is due to an absence of ice drift data. However, in the Labrador Sea ice drift can be very fast (up to tens of kilometers per day), as evidenced by buoy data illustrated in Fig 30 of the DFO field campaign report (Prinsenberg et al., 2012). Aside from EMS surveys P-4 and P-5, at least 4 hours and up to 26 hours passed between the acquisition times of the EMS and SAR data sets. In these cases, it is unreasonable to assume that the SAR data and EMS measurement lines observed the same ice floes. I am impressed by the results presented in Section 4, but am skeptical of how such strong results were achieved given that significant ice drift would have occurred between the SAR and EMS data acquisitions. The authors must provide some justification for why they did not attempt to correct for ice drift, or if they did do so, the methods used need to be described in detail.

In this version, we reorganized Section 1 "Introduction". Besides we emphasized the significance of retrieving the sea ice thickness distribution from CP SAR both in the ice services and sea ice forecasting, we provided an overview of the differences between dual-, quad- and compact-polarimetric mode, and explained why the CP mode can increase swath widths. 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. Lastly, we presented the procedure of ice drift correction, and the detail description is given in section 4.2.

Specific Comments

P5445, Title: The title could be modified slightly to ". . . from simulated C-band compact polarimetric SAR images" to indicate that the data used are not true compact polarimetric data, but rather are simulated from quad-pol data.
Done.

P5446, L6-7: What are the "optimal conditions for thickness retrieval"? They should be stated in the abstract as concisely as possible.
The "optimal conditions" means optimal sea ice conditions and radar parameters. Thanks, has been changed.

P5446, L14: I think it would be more appropriate to state the range of ice thicknesses across which the RMSE and correlation coefficients were calculated (0.1 to 1.5 m) rather than the mean; or better yet state both the mean thickness and thickness range.
We considered these comments and explicitly mentioned these in the text.

P5446, L16-21: The first paragraph of the introduction is far too brief. The authors need to elaborate on why knowledge of the ice thickness distribution is of interest to a wider range of user groups and why it is important to other science questions. E.g. the authors do not mention the importance of sea ice thickness for model forecasts of sea ice conditions (both seasonal and long-term climate forecasts) or to support polar operations (marine navigation, resource exploration and extraction etc).
Done. The introduction has been completely revised. For this part, we have emphasized the importance of retrieving the sea ice thickness for model forecasts and polar operations

P5446, L22-24: The authors fail to include several methods that have been used to measure sea ice thickness including: in situ measurement techniques (drill holes, ground based EM surveys); and, more importantly, airborne and satellite altimetry (e.g. ICESat, Ice- Bridge and CryoSat-2), which has been used to provide pan-Arctic estimates of ice thickness.
The introduction has been completely revised. This part has been reorganized, all comments has been included. By introducing the limitations of different types (including drill holes, EMS, and altimetry) of sensors, the motivation of the paper is provided.

P5447, L5: The reference to Kwok et al (2009) is inappropriate. Kwok et al discuss Arctic sea ice thinning and volume loss estimated from ICESat – it does not mention radiometer derived ice thicknesses at any point. At what resolution are radiometer derived sea ice thickness products provided? 25 km? For what users would this resolution be considered coarse? For many applications (e.g. climate modeling) ice thicknesses at these coarse resolutions would be sufficient.
We have revised these and the specific information of radiometer has been added.

P5447, L6-10: Related to the previous comment, what do you mean by higher

resolution? 1 km or 100 m or 10 m? If possible provide the spatial resolution expected for compact- pol SAR beam modes (100 m?).
The information is now provided

P5447, L11-12: It would be useful to also separate the list of references provided here by the radar frequency bands considered in each study. Most of the references cited analyzed L-band SAR data, which should be noted.
The information is now included.

P5447, L24: Define what quad- and dual-polarization modes are.
This is now already explained earlier in the introduction.

P5447, L24-26: Again, you need to define what the user requirements are that you are trying to meet. What is the areal coverage required by your targeted audience/user base? Is a 500 km swath wide enough?
We separate science users and operational mapping. There is now rule-of-thumb for the optimal swath width. It depends on the application. We think it is beyond the scope of this paper to discuss this in detail.

P5447, L26-27: RADARSAT-2 has "wide fine quad-pol" and "wide standard quad-pol" modes that provide 50 km swath widths. See Page 2-8 of the R-2 Product Description document: http://gs.mdacorporation.com/includes/documents/RN-SP-52-1238%20RS-2%20Product%20Description%201-8_15APR2011.pdf
Thanks, we considered this.

P5447, L28: It should be noted that some SARs (RADARSAT-2, Sentinel-1A, and PALSAR-2), can acquire dual-polarized data at ScanSAR beam modes.
We mention this explicitly for RADARSAT-2.

P5447, L28-P5448, L2: Some readers may be unfamiliar with compact polarimetry. It would be beneficial if you could include a sentence or brief paragraph explaining the difference between CP and traditional linear polarimetric systems (e.g. CP transmits a circularly polarized wave, and receives H+V linear backscatter, allowing acquisitions over a wider swath width at higher resolution, relative to traditional quad-pol beam modes, through reduced power consumption and data storage requirements).
We have modified these as suggested.

P5448, L2: You should note that CP modes will provide a reduced quantity of polarimetric information relative to quad-pol modes.
Yes, we clarified this.

P5448, L3: Salberg et al (2014) is not an appropriate reference. Salberg et al do not outline forthcoming SAR missions that support compact-pol modes. Find another

source if possible, or simply to state the names of current/future sensors that will support CP (e.g. RCM, PALSAR-2).
We stated the names as suggested.

P5448, L5: Operational sea ice monitoring, which is the stakeholder that could best take advantage of the results of this work, needs to be mentioned much earlier in the introduction. It should be outlined from the first paragraph that operational sea ice monitoring would benefit greatly from sea ice thickness derived from SAR data.
We did this.

P5449, L18: I don't think it is accurate to state "two orthogonal circular polarizations", I think this should be "two opposite handed circular polarizations".
It is possible to use the term "orthogonal circular polarizations. But we avoided this term in the new version of the paper.

P5449, L19-20: Following the convention in Nord et al (2009) CTLR is an acronym for "Circular Transmit, Linear (horizontal and vertical) Receive". You should use this explicit definition, as the current text leads the reader to wonder why "L" is included in the acronym.
The meaning of CTLR is explained in the introduction and in the abstract

P5449, L23: What do you mean by the "first practical radar"?
The "first practical radar" is the Chandraayan satellite. We avoid this now and mention Risat, ALOS-2 and RCM, see above.

P5451, L4: What are typical values for the RMS height and correlation length of smooth FYI? Include appropriate references.
We provided examples.

P5453, L17: Provide a reference for the value of the dielectric constant of FYI that you have used here.
Done.

P5454, L16-19: Can you explain why you didn't use the surface scattering term from the Nghiem et al model? Why did you use the PTSM model of Iodice et al instead?
Nghiem et al consider SPM as well as physical optics (PO) and geometrical optics (GO). The disadvantage of SPM is explained in our text. The applicability of PO for sea ice was critically discussed in the late 70s, and most simulations are now carried out with the IEM and its extensions (not use by Nghiem). Since the extended IEM is to complex in particular for retrievals of geophysical parameters, we decided to use a more straightforward approach by applying PTSM.

P5454, L20 to P5455L2: The organization of this paragraph could be improved. I recommend introducing all of the permittivity parameters first (air, water, ice and

brine), then introduce the surface roughness parameters. You also need to add a definition for the permeability μ0, which is shown in Fig. 3 but not described in the text.
Thanks. All the comments have been considered.

P5454, L22: Is only the ice surface (top) roughness defined? If the ice bottom roughness is also prescribed in the model, specify that here, and add relevant labels to Fig 3.
We do not consider the scattering contribution from the ice-water interface (because of the high salinity of arctic young and first-year ice), which is now mentioned in the text.

P5455, L4-6: Do you mean you do not consider deformation processes that are dynamic in origin (i.e. rafting/ridging)? Some dynamic processes (e.g. rafting of nilas) can cause deformation on centimeter to decimeter scales.
We do not consider deformation processes. The simulations are for ideal cases (as in most other studies). We mention this in the discussion.

P5455, L12-17: define the symbols used in Eq. (13) in the same order in which those terms are found in the equation so that it is easier for the reader to follow along (i.e. define α first, then F_r, then I_0 etc.)
Done.

P5456, L1-2: What are the valid ranges for s, l and σ within the X-SPM model? It would be helpful to provide these to the reader. Furthermore, references should be provided to demonstrate that the range of values considered for these parameters are representative of FYI surfaces.
References for the X-SPM ranges are given, the selected parameters represent the validity range. FY-Ice roughness: see section 2.2, where typical values for level ice are given based on the Fung reference.

P5456, L6-7: Why were these fixed values of air temperature and wind speed selected? You need to provide justification for these values. Are data available for a weather station near the study region? Check Environment Canada's climate data archive (http://climate.weather.gc.ca/), I believe there is a station at Makkovik.
Data are based on information from the field campaign described in Section 4 – we now mention this here.

P5456, L13-15: Again justification is required for the values of s, l and σ. Provide references to demonstrate that the chosen values are typical for FYI. You also need to provide a definition for the wavenumber symbol k.
The small-scale roughness validity ranges of the Iodice-model are not given explicitly. We use the Bragg-backscattering validity range which should be acceptable for the Iodice model. The used small-scale roughness parameters are based on the data

reported in Onstott, 1992; page 86, table 5-3. we explained what “k” means.

P5456, L24-26: The impact of ice thickness on CP-Ratio was not discussed in Sect. 2.
Thanks. This was an error, we removed the hint to section 2.

P5457, L10: when you state “more or less independent of the incidence angle” do you mean CP-Ratio is not sensitive to incidence angle? It seems clear to me that CP-Ratio is highly dependent on incidence angle.
The reviewer is right, we changed the text accordingly.

P5457, L13: The impact of incidence angle on the CP-Ratio should be acknowledged here. I would change this sentence to read: “. . . has a strong correlation with the thickness of smooth undeformed ice at any given incidence angle.”
The reviewer is right, we changed the text accordingly.

P5457, L18-19: Indicate the data source for these air temperature and wind speed measurements.
Temperature and wind data are now discussed in section 4.2, and the reference is included

P5457, L26: Remove the reference to Simila et al (2010). That paper references Haas et al (2006), which you have already cited. Also remove the Simila citation on P5458 L3. Instead add the following reference, which discusses the calibration and accuracy of the EMS system used by DFO: Prinsenberg, S., S. Hollady, and J. Lee (2012). Measuring ice thickness with EISflow, a fixed-mounted helicopter electromagnetic-laser system. Proc. 12th International Off- shore and Polar Engineering Conference, 1, 737-740.
Done.

P5458, L4-5: Change this sentence to read “By subtracting the GPR snow thickness measurements from the EMS snow plus ice thickness measurements, sea ice thickness can be estimated.”
Done.

P5458, L11: change “the measurement” to “the DFO airborne survey flight lines”
Done.

P5458, L11-12: Define what a Pauli RGB image is.
The definition of a “Pauli RGB” is given in Fig. 8.

P5458, L16-19: As outlined in my general comments, further discussion is required regarding the potential impact of ice drift on your results, as only 1 SAR image was acquired within an hour of a coincident EMS survey. In the DFO field report (Prinsenberg et al., 2012) Fig. 30 shows drift tracks for icebergs and ice floes

acquired by ice beacons during the field campaign. This figure indicates that ice drift was on the order of tens of kilometres per day! To me, this indicates that in all cases you cannot reasonably assume that the SAR pixels located underneath the EMS flight lines observed the same ice floes as were observed during the EMS surveys. Was any ice drift correction applied to (automatically or manually) co-locate the SAR and EMS datasets? If so this must be described in detail in the manuscript. If not you must provide an explanation for how you have any confidence that the SAR and EMS were observing the same ice.
We provided the description about ice drift correction. The procedure is described in section 4.2.

L19 and L26: specify "spatial resolution".
Done.

P5459, L1-19: I think the 7 data processing steps should be described with more detail. I found several steps to be unclear (see following comments). L5-6: How were deformed ice and icebergs identified? Was this done manually/visually? Where any automated procedures applied? What criteria were used to determine if sea ice is deformed? L7-10: Provide more suitable references to support your 20 cm threshold for snow depth. . . Stiles and Ulaby (1980) discuss the impact of snow wetness, not snow depth, on microwave response. Nakamura et al (2009) evaluate X- and L-band SAR backscatter over sea ice, so their results cannot be directly applied to C-band. Both these references should be removed. L11-15: How do you determine that only one ice type is present within an area? Are you manually delineating individual ice floes?... When averaging the airborne data, are they always averaged in 50 m sections? Or does each section simply need to be at least 50 m long (e.g. can one profile segment be 50 m in length and another be 100 m?). The profile segments should have a constant length. L22: If you have 702 samples, each at least 50 m in length, that should total at least 35 km, not 10 km as indicated in the text. Please correct/explain this discrepancy. L24: The maximum ice thickness for a profile segment is reported as 3.3 m. It is not realistic for FYI in this region to grow thicker than 2 m through thermodynamic processes alone. To me, this indicates that at least some of the profile segments include deformed ice. Some discussion of why ice thicknesses greater than 2 m are observed should be provided, or perhaps the masking of deformed ice requires improvement?
We modified the processing change and hence the description of the different steps. We tried to consider all items listed by the reviewer.

P5460, L5, Fig. 10: Are all 702 data points plotted? It doesn't look to me like there are 702 data points shown in Fig. 10. . . additionally, is Eq. (14) derived using all 702 data points? or a subset of the ice thickness and SAR datasets? The SAR images and EMS paths included in these figures/regressions need to be made explicitly clear.
Now 320 data points are left, and we mentioned this in the text

P5460, L8-9: You state that ice thicknesses exceeding 2 m correspond to multi-year ice (MYI); however, Canadian Ice Service (CIS) charts do not indicate the presence of MYI along any of the flight lines. Can you identify MYI with certainty in the quad-pol SAR data? Are you sure these aren't simply areas of deformed ice? CIS charts are available from: http://iceweb1.cis.ec.gc.ca/Archive/?lang=en

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, L9-11: While the dielectric constant of MYI is unlikely to change significantly with ice thickness, backscatter from MYI is dominated by volume scattering, not Bragg scattering, and the surface roughness of MYI is much greater than that of FYI, so the assumptions used when modeling CP-Ratio versus ice thickness cannot be applied to regions of MYI. These considerations should be discussed, assuming MYI is proven to be present.

Now the MY ice has been excluded, see above, and hence it is not necessary to discuss the effect of MY ice.

P5460, L17: provide units for your RMS error (this applies throughout the manuscript). The RMSE and CC are also provided at far higher precision than is justifiable given the uncertainty of the EMS thickness measurements (at best cm precision).

We considered this

P5460, L18-19: While this equation can be applied without knowing incidence angle – is that a wise thing to do? I doubt that this empirical equation would provide useful results if applied to a different scene let alone a different study area or study year.

The equation has been removed.

P5460, L25, Eq. (15): Again are all data points (grouped by incidence angle) used to derive these empirical regressions? Or are only some flight segments/images used?

Now we explain this explicitly.

P5461, L1-2: this statement is too general. You need to specify that this will work for "smooth level first-year ice from C-band radar images, under winter dry snow conditions."

Done

P5461, L13-14: For the validation work presented in this paragraph, your results are limited to thicknesses < 1.5 m. Why have you subset your validation dataset to < 1.5 m while the preceding paragraphs included all data points (up to 3.3 m)? L14: Can you also include the mean bias for the validation dataset.

Now we explain this explicitly.

P5461, L23: Sentence needs to be edited, you state RADARSAT-2 images (plural), were used for validation; however, only one validation image was presented.
Done. Two images (#2 and #3) were used to evaluate our method and we explain this explicitly.

P5462, L2: While the first paragraph of Sect. 5 does a reasonable job of summarizing this work, some important details should be acknowledged. First, it needs to be stressed that the results are derived from a single field campaign with a limited number of SAR images, spanning one study site and only two days. Further validation work is required before the regression equations presented can be applied in an operational context. Second, further work is needed to define thickness inversion equations at other incidence angles. Finally, the valid range for ice roughness (σ, l, and rms height) and snow conditions (< 20 cm, dry snow) in which these results can be considered applicable, should be explicitly restated here.
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.

P5462, L9-11: You should note that the relationship between large-scale surface roughness and ice thickness reported by Toyota et al (2009) and Peterson et al (2008) is for FYI. Neither study included MYI. L15: Define a symbol for the correlation coefficient between ΣH and ΣV - perhaps ρ_{CP}? Then use this symbol in Eq. (17) and Fig. 13.
We have skipped this part and figure.

P5463, L5: specify that "C-band compact polarimetric SAR has great potential for sea ice thickness retrievals over level FYI cover by a thin, dry snowpack."
Done.

P5463, L6: mention what current/planned C-band SAR missions support compact-pol data acquisitions. Also it would be great, if possible, to provide some comments on the possible use of compact pol data at other frequencies (specifically L-band or X-band).
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: what about other applications/user groups aside from operational ice monitoring (e.g. seasonal and climate sea ice forecasting) – could they benefit from this work?
We address this point at the end of Section 5.

Table 3: Number and order the RADARSAT-2 scenes chronologically. Change the caption to read "Specifications of the quad-pol RADARSAT-2 data" and remove the Polarization column from the table. In the column headers fix the following typos: "Data/Time" to "Date/Time" "Resolutions" to "Resolution" "Incident" to "Incidence" The spatial resolution of RADARSAT-2 SLC data products varies with beam mode and is not constant in the range and azimuth directions – have you resampled the data to produce square pixels? If so you need to explain this in text. If not both the range and azimuth resolutions should be listed.
The table has been updated, and all the comments are considered.

Table 4: Replace "EM" with "EMS" throughout the table (caption and table headings) – be consistent with the use of abbreviations/acronyms provided in the text. In the table headings change: "SAR data coincident with EM" to "SAR Scene ID coincident with EMS" "Data/Time" to "Date/Time" As with Table 3, number your EMS flight segments chronologically.
The table has been updated, and all the comments are considered.

Fig 1: Add a line on the plot for incidence angle = 20 degrees (wide swath beam modes for Earth Observing SAR sensors typically range from 20 to 50 degrees).
For consistent the incidence angles in all our figures, we now show results from 20° to 60°.

Fig 2: It would be helpful to mention in the text whether or not the results for other incidence angles follow similar trends as those shown for 30 degrees. Also why does the x-axis cover such a large range? The dielectric constant of FYI should cover a much smaller range of values near 3.
We gave the results only the value of dielectric constant near 3.

Fig. 4: The font size of the axis labels and tick marks is far too small to read. I would also recommend that you try combining these three plots into a single plot with multiple y- axes and three different colored lines instead of three separate graphs. Adding grid lines to the plots would also be helpful (this goes for all graphs in the manuscript).
The figure has been updated, and all the comments are considered.

Fig. 5: Why does your x-axis end at 1.4 m ice thickness? Your inversion is valid up to 1.5m thickness, so the x-axis should extent to at least 1.5 m. The same applies for Figs. 6 and 7. Why do you model incidence angles up to 60 degrees? As far as I am aware

the standard "accessible" swath for all current SAR missions is 20 to 50 degrees. I suppose it doesn't hurt to include 60 degrees as well though, but be consistent in the incidence angles you include in all your figures.
The x-axis end at 1.5 m, and we now show results from 20° to 60° in Figure 1.

Fig. 6: Why have you shown only 20 and 30 degrees incidence? To save space? If you are only going to show 2 incidence angles then you should select two incidence angles hat show a wider range of the standard accessible swath (e.g. 20 and 40 degrees), not two steep incidence angles as you have shown here.
Agreed. We now show results from 20° and 40°, and all the comments are considered.

Fig. 8: This figure needs to be remade. A screenshot of Google earth is not acceptable. A graticule indicating latitude and longitude, as well as a scale bar are required on a map. It would also be helpful to include an inset map indicating the study site location along Canada's east coast (i.e. include a zoomed out map with more reference features so that readers can see where Labrador is located). In the caption change "measurement site" to "study site"; change "with four Pauli RGB decompositions of Radarsat-2..." to "with Pauli RGB decompositions of the RADARSAT-2. . ."; change "induction sounder" to "EMS". NOTE: As required by MDA's RADARSAT-2 End User License Agreement "the follow- ing copyright notice must be conspicuously displayed alongside the product": "RADARSAT-2 Data and Products © MacDONALD, DETTWILER AND ASSOCIATES LTD. (2011) – All Rights Reserved" and "RADARSAT is an official mark of the Canadian Space Agency" must appear as a credit.
The figure has been updated, and all the comments are considered.

Fig. 11: Why 50% confidence intervals? In Fig. 10, 90% confidence intervals were used. If possible add modeled CP-Ratio vs thickness curves - it would be very interesting to see how well the model vs empirical fits agree/disagree.
Now, 90% confidence intervals were used, and the comparison between model and empirical results was presented in the text.

Fig. 12: Again the axis labels and tick labels are too small to read. Make the plot area of part (b) square to better demonstrate the strong 1:1 agreement.
The figure has been updated, and all the comments are considered.

Technical corrections
P5446, L2-7: Define all acronyms included in the abstract. Add the acronym for compact polarimetry ("CP") on L2, add the definition for SAR on L2, and add the definition for CTLR on L7.
Done.

P5446, L9: Change "in the region of the Sea of Labrador" to "in the Labrador Sea" L17: Change "of the Arctic" to "of Arctic" L22: change "can be" to "has been"

Done.

P5447, L1: Provide the sensor name in full followed by the acronym. L2-3: change "on the ice thickness" to "on ice thickness" L20: Change "Arctic Sea" to "Arctic Ocean" L21: change "older ice" to "MYI" L22: change "In conclusion," to "Based on the existing literature,"
Done.

P5448, L13: change "directly" to "direct"
Done.

P5449, L9, Eq. (1): The bottom left element of the scattering matrix should be S_{VH}. Also note in the text that reciprocity (SHV =SVH) is assumed. L10: Include a definition of the subscripts of the scattering matrix provided in Eq. (1). (i.e. "where Spq denotes the p transmit and q received linear polarization."). L11: change "coherence matrix" to "coherency matrix". L18, Eq. (3): provide a reference to Nord et al (2009) for this equation.
Done.

P5450, L5: Ensure you are consistent with upper/lower case for the subscript characters in your equations - Σh should be ΣH on this line. L6: Replace Σh with ΣH (on first line in Eq. 6) L17: Change "coherence matrix" to "coherency matrix" L18: Replace Σh with ΣH (in Eq. 8)
Done.

P5451, L13 and L22: change "incident angle" to "incidence angle".
Done.

P5452, L1: change "incident angle" to "incidence angle". . . Check for this typo throughout the manuscript. L12: Add reference to Iodice et al. (2011) for Eq. (10).
Done.

P5453, L4: change "..we propose the. . ." to ". . .we propose to exploit the. . ." L4-5: change "(here called CP-Ratio)" to "(here denoted as the CP-Ratio)" L6, Eq. (11): change "Ratio =" to "CP-Ratio =" L14-15: change "...dielectric constant of surface..." to "...dielectric constant of the surface. . ."
Done.

P5454, L3: change ". . . insensitive to the surface slope variations. . ." to ". . . insensitive to surface slope variations. . ." L24: Symbols are in wrong order, change "The thickness and surface temperature . . . are T0 and H." to "The thickness and surface temperature ... are H and T0, respectively."
Done.

P5455, L24: add symbols for the ice-water interface temperature (Tb) and ice surface temperature (T0) L25: Fix units for density, should be kg m^{-3}*.*
Done.

P5456, L11-12: Be consistent with the symbols used for ice thickness and brine volume fraction. In Tables 1 and 2 you used H and f_{vb}*, yet here in the text you have used h and fv. L19: change ". . . due to the desalination process" to ". . . due to desalination processes" L22: change "C band" to "C-band"*
Done.

P5457, L1: change ". . .ice thickness < 0.4 m" to ". . . ice thickness is < 0.4 m" L2: add a reference to Cox and Weeks (1983) here. L8-9: change " . . .changes in particular. . ." to ". . .has an effect on. . ." L11: change ". . .is less reduced than. . ." to ". . .is greater than. . ." L17: add the acronym "DFO" for the department of Fisheries and Ocean Canada.
Done.

P5458, L1: change ". . .deformed ice; the maximum. . ." to ". . .deformed ice, where the maximum. . ." L2: remove "in the worst cases". L10: Change "Radarsat-2" to RADARSAT-2" (fix this throughout the manuscript). L27: Change "in situ" to "airborne".
Done.

P5459, L5: Change "Deformed ice, ridge, and iceberg areas" to "Regions of deformed sea ice and icebergs were removed. . ." L7: Delete "The" from the start of 4. L16: Typo "GRP" should be "GPR", add "EMS" before snow-plus-ice thickness. L18: I think this refers to the wrong equation, should refer to Eq. (11)? L20: Remove comma after ". . . ensures that". L25: change final sentence to ". . . and the modal thickness (peak), representing the ice thickness most frequently encountered, was 0.50 m."
Done.

P5460, L4: change ". . .with corresponding values of CP-Ratio. . ." to ". . .against the corresponding values of the CP-Ratio. . ." L21: change ". . .the level of CP-Ratio decreases. . ." to ". . . the level of the CP-Ratio increases. . ." L24: define the acronym for correlation coefficient (CC), prior to using it in Eq. (15).
Done.

P5462, L21: change "and independent" to "and is independent"
Done.

P5463, L3: replace "matching" with "coincident"
Done.

REFERENCES: Please review your references list carefully. A few typos/errors I noticed are listed below. P5464 L18: "olarimetric" should be "polarimetric" P5465 L11: The first author "Monaco" should be "del Monaco" P5465 L19: Article title is missing "synthetic" from synthetic aperture radar.
Done.

Table 1: For the incident shortwave radiation term: Equations column, second row: Should "cosH" at the end of the equation be "cosHa"? Parameters column, third row: Should "C in the range 0 to 10" be "C in the range 0 to 1"? Parameters column, fifth row: what does d represent? I don't think it has been defined. Comments column, final line: change "Universal Time Coordinated" to "Coordinated Universal Time" For the long wave radiation term: Parameters column, bottom row: should "e is the water vapour pressure at Ta (unit: Hpa)" be moved to the comments column? Also e is not used in the equation – is this a typo? Should e replace d in the equation for the emissivity of the atmosphere? For the upward conductive heat flux term: Parameters column, equation for k: the term V_b is undefined, I assume this is a typo and should be f_{vb}?
The table has been updated, and all the comments are considered.

Table 2: Remove "parameters" from the end of the caption. For the sea ice density and brine volume fraction equations the terms F1 and F2 are undefined. I would also suggest using Ti instead of T for the ice temperature. For the sea ice salinity equations the unit m in the thickness conditions should not be italicized.
The table has been updated, and all the comments are considered.

Fig. 1: Include the σ symbol in the x-axis label.
Done.

Fig. 2: Include the ε symbol in the x-axis label. Change "constants" to "constant" (singular).
Done.

Fig. 3: Change "Structure" to "Structural" in figure caption. Ensure all your subscripts are correct in the figure (e.g. the subscripts for the permittivities of ice and brine need to be reversed).
The figure has been updated, and all the comments are considered.

Fig. 6: Typo in caption ". . .for Ratio. . ." should be ". . . for CP-Ratio. . .".
Done.

Fig. 7: Typo in caption ". . .for Ratio. . ." should be ". . . for CP-Ratio. . .".
Done.

Fig. 10: Typo, both regression and line should be singular in the caption.

Done.

Fig. 11: Typos in caption, "incident" to "incidence"; "angle", "fit", "interval" and "color" all need to be pluralized.
Done.

Fig. 13: Include the σ symbol in the x-axis label. Change "correlation" to "correlation coefficient" in the caption.
Done.