The Cryosphere Discuss., 9, C2282–C2299, 2015
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The Cryosphere Discussions

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Interactive comment on "Retrieval of the thickness of undeformed sea ice from C-band compact polarimetric SAR images" *by* X. Zhang et al.

Anonymous Referee #2

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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

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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 kilometres 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.

Specific comments

P 5445:
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.

P 5446:
L6-7: What are the "optimal conditions for thickness retrieval"? They should be stated in the abstract as concisely as possible.
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.

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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).
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, IceBridge and CryoSat-2), which has been used to provide pan-Arctic estimates of ice thickness.

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.
L5-6: 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.
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?).
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.
L24: Define what quad- and dual-polarization modes are.
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?

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

P5448:
L2: You should note that CP modes will provide a reduced quantity of polarimetric information relative to quad-pol modes.
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).
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.

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”
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 defini-

tion, as the current text leads the reader to wonder why “L” is included in the acronym.
L23: What do you mean by the “first practical radar”?

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

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

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?
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 μ_0, which is shown in Fig. 3 but not described in the text.
L22: Is only the ice surface (top) roughness defined? If the ice bottom roughness is also prescibed in the model, specify that here, and add relevant labels to Fig 3.

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.
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.)

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.
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.
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.
L24-26: The impact of ice thickness on CP-Ratio was not discussed in Sect. 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.
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*."
L18-19: Indicate the data source for these air temperature and wind speed measurements.
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 Offshore and Polar Engineering Conference, 1, 737-740.

P5458:
L4-5: Change this sentence to read "By subtracting the GPR snow thickness measure-

ments from the EMS snow plus ice thickness measurements, sea ice thickness can be estimated."

L11: change "the measurement" to "the DFO airborne survey flight lines"

L11-12: Define what a Pauli RGB image is.

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.

L19 and L26: specify "spatial resolution"

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?

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.
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
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 consideration should be discussed, assuming MYI is proven to be present.
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).

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.
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?

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.”
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.
L23: Sentence needs to be edited, you state RADARSAT-2 images (plural), were used for validation; however, only one validation image was presented.

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.
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 Σ_H and Σ_V - perhaps

ρ_{CP}? Then use this symbol in Eq. (17) and Fig. 13.

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*."
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).
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?

TABLES:
P5469, 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.

P5470, 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.

FIGURES:

P5471, 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).

P5472, 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.

P5474, 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 coloured lines instead of three separate graphs.

Adding grid lines to the plots would also be helpful (this goes for all graphs in the manuscript).

P5475, 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.

P5476: 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

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that 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.

P5478, 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 following 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.

P5481, 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.

P5482, 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.

Technical corrections

P 5446:
L2-7: Define all acryonyms 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.
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”

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,”

P5448:
L13: change “directly” to “direct”

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

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

P5451:
L13 and L22: change “incident angle” to “incidence angle”.

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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).

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...”

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 T_0 and H.” to “The thickness and surface temperature ... are H and T_0, respectively.”

P5455:
L24: add symbols for the ice-water interface temperature (T_b) and ice surface temperature (T_0)
L25: Fix units for density, should be kg m^{-3}.

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 f_v.
L19: change “... due to the desalination process” to “... due to desalination processes”
L22: change “C band” to “C-band”

P5457:

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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.

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"

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."

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).

P5462:

L21: change “and independent” to “and is independent”

P5463:
L3: replace “matching” with “coincident”

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.

TABLES:
P5467, Table 1:
For the incident shortwave radiation term:
Equations column, second row: Should “$cosH$” at the end of the equation be “$cosH_a$”?
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 T_a (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}?

P5468, Table 2:

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Remove “parameters” from the end of the caption.
For the sea ice density and brine volume fraction equations the terms F_1 and F_2 are undefined. I would also suggest using T_i instead of T for the ice temperature.
For the sea ice salinity equations the unit m in the thickness conditions should not be italicized.

FIGURES:
P5471, Fig. 1: Include the σ symbol in the x-axis label.

P5472, Fig. 2: Include the ε symbol in the x-axis label. Change “constants” to “constant” (singular).

P5473, 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).
Add σ to the list of ice surface roughness parameters shown on the figure.

P5476, Fig. 6: Typo in caption “. . .for Ratio. . .” should be “. . . for CP-Ratio. . .”.

P5477, Fig. 7: Typo in caption “. . .for Ratio. . .” should be “. . . for CP-Ratio. . .”.

P5480, Fig. 10: Typo, both regression and line should be singular in the caption.

P5481, Fig. 11: Typos in caption, “incident” to “incidence”; “angle”, “fit”, “interval” and “color” all need to be pluralized.

P5483, Fig. 13: Include the σ symbol in the x-axis label. Change “correlation” to “correlation coefficient” in the caption.

Interactive comment on The Cryosphere Discuss., 9, 5445, 2015.