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Interactive comment on "The Arctic Ocean Observation Operator for 6.9 GHz (ARC3O) – Part 1: How to obtain sea-ice brightness temperatures at 6.9 GHz from climate model output" *by* Clara Burgard et al.

Clara Burgard et al.

clara.burgard@hzg.de

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RC: Reviewer comment, AR: Author response

Reviewer Summary: The manuscript addresses the simulation of brightness temperature at 6.9 GHz from sea ice (with no consideration of snow cover) using ice property profile (temperature and salinity) resulting from an advanced 1D thermodynamic model and other simplified models. The brightness temperature is simulated using 1D microwave emission model. The main purpose of the study is to examine the sensitivity

of the calculated brightness temperature to assumptions of the ice property profiles. With that, the study reached conclusions about the factors that affect the brightness temperature the most, such as the sub-surface salinity of first-year ice and the use of a salinity profile that changes with depth compared to assumption of constant salinity or linear temperature. With this information, it is possible to develop an observation operator that can be applied to sea ice simulation by a climate model. While the study offers information towards this purpose it does not actually provide a conclusive answer. Yet, this does not take from the credibility of the study as I see it a pioneering attempt to handle the sensitivity issue using a novel approach of testing the effects of different profile shapes (e.g. constant salinity introduced very large uncertainties in brightness temperature and the two-step linear temperature assumption in snow-covered sea ice does not introduce large uncertainties, etc.). The manuscript is well written and the subject is timely as the issue of sensitivity of ice parameter estimation (from satellite observations or modeling) has been identified as urgent, a conclusion from a workshop on same subject in Hamburg in October 2017. I would recommend publication subject to revision that takes into consideration the following comments.

AR: Thank you very much for the positive feedback, and for your detailed, constructive comments on how to further improve our paper. We plan to address all your comments as described in the following.

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RC: Major comments First comment: The writing in some parts is confusing. I had to read the same part several times to understand and connect what the authors want to say. Please modify to make the presentation more coherent, especially in the parts that describe the tested profiles (reference and simplified), sections and sub-section titles that do not reflect the contents, etc.

AR: We will clarify the structure, better separating the methods and the results. We also plan to split some of the figures into several figures to make it easier for the reader to follow.

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RC: Second comment: The use of some terminology is confusing such as “water liquid volume fraction”.

AR: In the process of working on this study, we have moved from using the term "liquid water fraction" to using the term "brine volume fraction". We started with "liquid water fraction" in opposition to "solid ice fraction" but decided to move on with "brine volume fraction" to avoid the confusion you are mentioning. Therefore, there should not have been mentions of "liquid water fraction" left in the manuscript. We apologize for the confusion and will replace "liquid water" by "brine" in the relevant occurrences.

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RC: Third comment: Some presented aspects of sea ice physics are not precise. I am suggesting corrections.

AR: Thank you for the suggestions given below. We will take them into account in the revision of the manuscript.

—

RC: Minor comments (All these issues are explained in the following comments. I call it minor though they are many and some may exceed the definition of “minor”.) Abstract The last sentence “As periods of melting snow with intermediate moisture content typically last for less than a month,...” needs modification. Snow may become wet during transition seasons (fall and spring) and that leads to anomalous brightness temperature (please see Shokr et al. Rem Sensing of Env, 123,(2013), and Ye et al., IEEE TGRS, 54(5) (2016)).

AR: We will clarify based on your suggestion.

—

RC: P3 L9: suggest using “loss” instead of “permittivity”

AR: Done.

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RC: P3 L11: This paragraph is about the emissivity (emitted radiation) from snow-covered sea ice. It needs modifications as I find confusion between using the terms emissivity and permittivity. Here is some information that might be useful in rephrasing the sentences. (1) permittivity determines the reflection/absorption at a surface of dielectric mismatch but emissivity determines the emitted radiation (in TIR or MW bands). (2) While there is relation between the emissivity and reflectivity, there is no relation between emissivity and permittivity. (3) The sentence “This means that water is a stronger absorber than pure ice in the microwave range” is not correct because water has high permittivity as you mentioned in the first sentence, therefore it is high reflector in the MW bands (but not in the TIR). (4) When the snow becomes wet or the ice surface is flooded, the emissivity increases due to the more absorption of solar radiation by water contents (nothing to do with the permittivity). So, to conclude this point, the authors can just focus on the emissivity in this paragraph and remove all connections to the permittivity. Emissivity and permittivity are used in modeling MW emission when layers are assumed (water/ice/snow/air) but this is not the subject of the paragraph.

AR: We apologize for the confusion due to using "permittivity" instead of "emissivity". Thank you for the clarification. We will change the terminology according to your suggestion.

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RC: P3 L16: the sentence “In snow, liquid water is mainly present during melting periods” needs correction. Please see my comment about the Abstract.

AR: We will clarify following your suggestion above.

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RC: P3 L21: the opening of this paragraph "The scattering of the microwave radiation in sea ice is a function of..." Once again, the theme here should be the emitted radiation, hence the focus should be placed on the two forms of extinction, the absorption and the scattering. Also, since you include the atmosphere, it is better to mention "the satellite observation of microwave radiation from sea ice" in the first sentence.

AR: Thank you for pointing out that this sentence was not precise. We will clarify following your suggestion.

—

RC: P3 L26: you can add "air bubbles in MYI" and mention something about the MW wavelength in relation to the typical size of brine pockets, snow grain, air bubbles and atmospheric droplets.

AR: We will add the air bubbles and typical sizes of the scattering bodies into the text.

—

RC: P4 L21 This last sentence in the paragraph is clumsy. Please clarify and simplify.

AR: Thank you for pointing that out. We will reformulate this sentence.

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RC: P4 L13 Make it "our reference profiles".

AR: Done.

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RC: P5 L1-5: Any reason why you did not use ERA5?

AR: Most of the analysis presented here was conducted and finished before the release of ERA5. However, we do not expect the choice of reanalysis data to substantially affect

the results of the study in any case, as the analysis focuses on conceptual findings, not tied to the exact timing and location of the forcing.

—

RC: P5 L11. You provide example to show that simulated sea-ice evolution is not necessarily representative for the real sea-ice evolution at location 75°N, 00°W. You can mention another example at 90°N as this location may not have MYI in all years. Please see maps of MYI in Fig. 8 in Ye et al. (2016) (mentioned above). The maps were generated from a retrieval method using satellite microwave observations.

AR: As mentioned in the manuscript, we do not claim to simulate the sea-ice evolution at the given location and time realistically. This is because SAMSIM always assumes a seasonal cycle for the oceanic heat flux to the bottom of the ice following the oceanic heat flux measured during the SHEBA campaign north of Alaska. Under the combination of ERA-Interim atmospheric forcing and this SHEBA oceanic forcing, sea ice can form at 75N00W and the ice at the North Pole survives the summer melt. This also means that locations which usually have MYI as pointed out in the reference you give might not have MYI in our simulations. As suggested by reviewer #2, we will explain the principle and location more conceptually. This will highlight that the locations for which the ERA-Interim forcing was chosen cannot be compared to these locations in reality.

—

RC: P6 Fig.2 the difference between the black and grey lines is not obvious although it is easy to understand what each color indicates. The peak of the ice thickness in June is NOT a "comfortable" result. Equally "uncomfortable" is the rate of MYI thickness increase. My expectation is that MYI thickness increase should take place at a slower rate.

AR: As mentioned in the caption, the peak of ice thickness in June is a model artifact.

As they represent only a very small fraction of data points, we do not expect this to have an effect on our results. However, to avoid confusion, we will mask these points out for the study. To our knowledge, the MYI thickness increase is not anomalous. Following your remark, we will check with literature again.

—

RC: P7 L6: Do you mean "incoming longwave radiation" instead "microwave radiation"?

AR: We mean microwave radiation. This is the radiation normally referred to as the downwelling microwave radiation. This represents all microwave radiation reaching the ground from the atmosphere. Contributors to this radiation are background space radiation, clouds and water vapour in the atmosphere, and oxygen. However, we set it to 0 K in our setup because we are mainly interested in the effects of sea-ice physical properties on the brightness temperature. We will clarify this in the manuscript.

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RC: P7 L7: Table 1, not Tab. 1

AR: Changed.

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RC: P7 L10: just to complete the physics picture, you may add the loss and scattering (extinction) caused by snow wetness, brine wicking, and snow metamorphism. Then you can state that you ignored these effects (the 6.9 GHz is not affected by the grain metamorphism as mentioned before in the text)

AR: We will complete the sentence.

—

RC: P7 L15: it is good to mention this limitation on the application of your study. Just want to remind you, once again, of the possibility of the wet snow during the transition

seasons as indicated above.

AR: We will clarify the handling of dry and wet snow throughout the text.

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RC: P7 Table1: did you mention the source of these data? If not please do.

AR: We will provide the sources for these constants in the caption.

—

RC: P7 last 3 lines (no line numbers in the manuscript): this is the first time you mention "brine pocket form". I am not familiar with MEMLS but does it need the geometry of brine pocket? This parameter is not mentioned in Table 1.

AR: MEMLS assumes either random needles or spherical pockets. We mention later in the manuscript that we use the spherical pockets assumption. Following your suggestion, we will include this information earlier.

—

RC: The influence of vertical sea ice properties Should this section be called "Results"??

AR: This will be part of restructuring the manuscript.

—

RC: P8 second paragraph.... Here are a few observations that might be used to improve the text. First, the salinity profile is always of C-shape as long as the cold temperature prevails. There is a physical explanation. It changes when the temperature rises in the spring. You can refer to the book of Weeks (2015) "On Sea Ice" or the book you already quoted by Shokr and Sinha. Second, the shape of brine pockets does not depend on age but, as rightly stated, on the initial formation process of sea ice. The assumption of spherical pockets may be valid for frazil ice. This is common in the subsurface layer of Antarctic ice and it exists in the Arctic when ice is formed under turbulent oceanic conditions.

AR: Thank you, this is useful information. We will clarify following your input.

—

RC: P8 Section 4 and section 4.1. The titles do not reflect the contents. For example, Section 4 "The influence of vertical sea ice properties" include Fig. 3, which is about effect of sub-surface salinity (not vertical profile). Also, Section 4.1 "Brine volume fraction" has information about the temperature profile at the end. Please re-organize the information to make improve the flow of the information.

AR: Thank you for your input. This will be part of restructuring the manuscript.

—

RC: P8 in Section 4.1, the authors kept mentioning "ice surface brine volume" while they mean sub-surface. Please replace "surface" with "sub-surface" and define the subsurface depth, at least roughly.

AR: We apologize for the confusion. In this case our subsurface is the upper 1 centimeter. We will clarify by using "near-surface brine volume".

—

RC: P9 L1: the sentence "Especially above an ice surface brine volume fraction of 0.2,..." is awkward. You may say "when ice surface brine volume fraction is higher than 0.2 ...". Also, it is not right to say "brightness temperature at the ice surface". Just say "brightness temperature from the ice cover". Then, in the following sentence you can say that the radiation is mainly coming from the surface.

AR: We will change the sentence following your suggestion.

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RC: P9 L4: in the sentence "brightness temperature transitions roughly linearly .." you may change the word "transitions" to "varies".

AR: Done.

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RC: P9 L8: the sentence "In our SAMSIM profiles, these high surface brine volume fractions fractions occur predominantly in summer, i.e. from April to September" is correct although the word "fractions" is repeated. I would like to draw the authors' attention to an estimation of brine volume fraction which we performed (experimentally) on Arctic sea ice and found that the fraction in the sub-surface layer (top 5 cm) exceeds 0.2 only when the average temperature exceeds -3°C. It is possible that the temperature of this layer reaches this value in the beginning of the freezing season. But this note does not affect the work in your study.

AR: Thank you for pointing this repetition out. We think that your observations are in line with our findings. Thanks for sharing these!

—

RC: P9: Figure 3 and the conclusions from this figure are interesting.

AR: We agree, thank you.

—

RC: P9 last paragraph (no line number)... you talk about "surface liquid water fraction" and "ice surface brine volume fraction". It is a bit confusing. On P8 L28 you mention "liquid water in the form of brine", which is a bit ambiguous. Brine is brine! And the dissolved salt (not water) is the material that causes loss of MW signal. I would suggest avoiding liquid water and just keep "brine". The liquid water fraction is relevant only to the snow at the onset of melt. Related to this point, you mentioned "For surface liquid water fractions below 0.2, occurring in both winter and summer..." But Fig.3 shows surface brine volume fraction, NOT liquid fraction. Also, you said "For these low ice surface brine volume fractions,...". What are those low fractions? Please fix this issue of liquid water versus brine volume fraction. It is only brine. Not liquid water.

AR: Again, we apologize for the confusion. As mentioned in an answer to a previous comment, in the process of working on this study, we have moved from using the term "liquid water fraction" to using the term "brine volume fraction". We started with "liquid water fraction" in opposition to "solid ice fraction" but decided to move on with "brine volume fraction" to avoid the confusion you are mentioning. Therefore, there should not have been mentions of "liquid water fraction" left in the manuscript. We apologize for the confusion and will replace "liquid water" by "brine" in the relevant occurrences.

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RC: P10 L1-5: it is mentioned that brightness temperature of thin MYI in summer drops to about 180K and that is attributed to the saline layer at the bottom of the ice. It is true that MYI thickens (grows) when winter returns and there is a layer of saline FYI at the bottom. But why do you say the emitted radiation mainly comes from this layer? The entire volume of the ice radiates. And the radiation from the bottom layer may be completely scattered by the bubbles, which concentrate at the to 20 cm or so.

AR: The influence of the bottom salinity on the MYI brightness temperature was inferred from investigating the different profiles one by one. These low MYI brightness temperatures were only found in a few September and October occurrences. In these cases, the ice is not thicker than 20 cm and the only property that can explain this difference when looking at the data is the gradient in salinity in the bottom layer. We therefore assume that the penetration depth reaches the ocean below the ice in these cases. As the ice thickens again during the freezing period, this effect vanishes rapidly. AS such thin MYI is not very common in the Arctic, we do not expect this issue to be relevant when inferring brightness temperatures from actual climate model output.

—

RC: P10 L6 "Unfortunately for the higher brightness temperatures around 260 K at low ice surface brine volume fractions, we could not infer...". Are you going back to the FYI here? You are in the middle of discussing MYI.

AR: The structure will be revisited following the general change in structure.

—

RC: P10 L9: Again, you mention "liquid water fraction profile". You probably mean brine fraction. Saline FYI ice has slid ice, brine, air and sometimes solid salt if temperature drops below the precipitation point of the salt. MYI has only solid ice and air. The term liquid water fraction is confusing for me.

AR: Again, we apologize for the confusion. This will be corrected.

—

RC: P10 L1: brightness temperature from MYI is around 180K in winter (low value because of the scattering from air bubbles) and it increases in summer due to surface flooding. That is why you found higher values of 260K. Please correct this information.

AR: This is not the case here. Our high values around 250 K are what is expected at 6.9 GHz. Typical tie-points values for winter MYI lie near 250 K (e.g. Ivanova et al. 2015, TC Vol9(5) use 246K). Low brightness temperatures for MYI are only occurring in our simulation in rare occasions during September and October when the MYI is at minimum thickness, but the surface is not wet anymore. As explained in a previous comment, these low brightness temperatures are anomalies tied to thin MYI, which does not occur often in the Arctic.

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RC: P10 L10-14: The information in this paragraph should be combined with information in the first paragraph in section 4.2 (Fig.4). The current text is confusing. What is the simplified profile? Constant for salinity and linear for temperature? Then why do you include a non-linear salinity in Fig. 4 and call it also "simplified"? Also, MPI-ESM uses the constant salinity and temperature profile. True? Is that the reason you tested the effect of constant salinity on brightness temperature? This is the most confusing part for me. Please re-write to make the information more organized and coherent.

AR: Again, we will work on a new structure to clarify.

—

RC: P10 L21: The title of 4.2 does not express the contents. We find data from Reference salinity, Reference temperature and Salinity as function of depth. Also, I would suggest presenting all these options in a table that shows the values, the functions (if any) and the method for each option. That will make it easier for the reader to follow the text and interpret the figure better.

AR: Again, we will work on a new structure to clarify.

—

RC: P10 L22: "as would be given..." or better be "as would be used..."?

AR: Replaced.

—

RC: P12 and P13: in the captions of Fig.5 and Fig.6 you should mention the season of the data (Oct.-March) and (April-Sept.), respectively.

AR: Thank you for pointing that out. We will add the clarification.

—

RC: P14 L8-9: This is the first time the explanation of the non-linear profile in Fig. 4 is explained. That is what I mean by re-organizing the information. I was wondered about this curve while reading, until I reached the explanation here.

AR: Again, this will be part of the restructuring of the manuscript.

—

RC: P14 Section 4.3: This section highlights the contribution from this study. Would be it useful to compile the statistics of absolute difference in one table to help the reader to explore the impact of each assumption at a glance? The numbers in the text should remain. I am not sure if this suggestion is reasonable but the authors might consider it. The results from using salinity as a function of depth in the case of MYI in summer (Fig. 6) is not the best, contrary to the conclusion in P14 L20.

AR: Yes, the salinity as a function of depth leads to the best result for MYI in warm conditions (10.6+/-21.7 K compared to 43.0+/-45.7 K for constant salinity). We will try your suggestion of using a table. This might be an important way to conclude these results, especially if we split the figures.

—

RC: P14 L28: model or module?

AR: We mean "model" here. We do not plan to integrate the emission model as a module into the climate model but rather to apply it on already produced climate model output.

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RC: P16 L2: "relationship only depends on the snow thickness". Why depend on snow thickness? You present the decrease of brightness temperature per unit depth (cm)?

AR: We plan on looking into the data again and clarify.

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RC: P17 L21: "In summer, we cannot reproduce realistic sea-ice surface brightness temperatures due to the very high sensitivity of the liquid water fraction to small changes in salinity near 0°C." Something is wrong here. Brine volume fraction is sensitive to salinity, but liquid water fraction?

AR: Again, we apologize for the confusion. We mean "brine volume fraction" and will replace it.

—

RC: P17 L25: the sensitivity of brightness temperature in summer is high because it is related to two parameters which we have no accurate information about; the areal ratio of melt pond and the wetness of the snow or even ice surface as you indicated later. In the next paragraph you mention snow grain as a possible contributor to the brightness temperature in summer. But this influence virtually does not exist at that time.

AR: We agree, this is unclear. We will clarify this.

—

RC: P18: The Outlook section is well composed. It is true that there is lack of comprehensive data on snow property profiles. However, there are many measurements conducted in scattered areas over the past few decades to characterize snow over ice under different atmospheric temperatures. It would be useful if someone compiles this information in one review paper and conclude some gross features that can be used in GCM models.

AR: Yes, we strongly agree that such a compilation of observations would be a very valuable resource for similar studies in the future.

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RC: P18 In the Conclusion section there is no mention about the good use of "salinity as a function of depth".

AR: We have mentioned the salinity as a function of depth in the point about "cold conditions". We will work on highlighting this better.

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