Response to Reviewer Comments on TC paper

Reviewers' comments

Responses to the comments

Responses to Reviewer 1

General comments

The paper is interesting and shows a theoretical explanation of passive microwave time series collected over aquifers in Greenland and Antarctica. The text is well written and easy to understand. It opens by introducing the scientific problem, the test sites and relative geophysical parameters, the electromagnetic models used for the analysis. Then it keep on with a suitable description of the results along with a fair discussion about these achievements and the uncertainties in the process. The workflow is fine although I found some major issues in the many assumptions made due to the lack of ground data. Assumption that often are unreferred and somewhat strong. In my opinion this point must be stressed clearly both in the abstract and in the introduction, in order to provide the reader with a clear overview of what will follow. Provided this, the paper is not a breakthrough but a first attempt to understand the relationship between aquifers evolution and microwave signatures although the many assumptions made can weak the reach of the work.

Specific comments

- The main issue of the paper is the lack of ground measurements to be used both in the modelling phase and in the verification of results. This lack has led to many assumptions and has weakened the work. For instance, density and temperature profiles at site 2 are not known and assumed as site FA-13 (line 59), temperature profile and liquid water content are not known for the Antarctic test site (line 64). The aquifer's liquid water content is let range from 5% to 25% but no reference is provided (line 188, it seems that these values comes from other papers or simulations, not from ground measurements). Also, temperature profile at Wilkins Ice Shelf test site is derived from a model but no references are given (lines 213-215 and 219-223).

Thank you for the comments. In this paper, we want to provide a forward model to simulate the SMAP brightness temperature observed over the aquifer region. We have made use all of the in situ measurements that are now available over Greenland and Antarctica. For the properties in Site 2, it

is not far away from the FA-13 site, thus we assume the same properties. The liquid water content values are from the measurement values from Dr. Lora Koenig's paper: Initial in situ measurements of perennial meltwater storage in the Greenland firn aquifer, GEOPHYSICAL RESEARCH LETTERS, VOL. 41, 81–85, doi:10.1002/2013GL058083, 2014. The temperature profiles and the liquid water content are derived from the paper's measurement data. These are ground measurements. For Wilkins Ice shelf, we use the surface temperature from Modis and relate the surface temperature and aquifer temperature with a exponential like function.

We agree that the temperature profiles used from this paper lack measurements. We tried to use the temperature profiles from geophysical models but the modeling results showed that it has a major difference between the in situ measurements from the in situ measurements we have. Temperature profile above the firn aquifer is indeed an unknown parameter. In the later responses, we would like to show the brightness temperature comparison between different physical temperature profiles.

The depletion trend of the aquifers is just assumed, and no ground truth is available but the one on April (lines 258-259 and 297) for Greenland and December for Antarctica. Given the work found that the water table level is one of the main drivers of Tb timeseries trend, the water table level must be derived in a more robust way. Maybe from a geophysical model.

We strongly agree with the reviewer that the change of water table need a better way to characterize. However, to our knowledge, such a model is not available at present. This is also why a modeling work from physical perspective at this stage would provide scientific value.

The snow temperature profile changes in time due to changes in water level and thermal forcing from above, no details are provided about its modelling (at line 261 is cited just a "squeezed"). For FA-13 the firn permittivity is set to a fixed value corresponding to a given liquid water content (line 279), however no references are provided to justify this geophysical value.

Thank you for your comments.

For the surface temperature swing, If we use a lower temperature in December than the temperature value used in April, this means that for the same brightness temperature observed, the aquifer water table is at a higher position than using the April surface temperature. For example, if we let the surface temperature in December be 5K lower than the current case, we can still reproduce the brightness

temperature observed by SMAP by moving the water table 0.5m higher as shown in the table below. This does not change the conclusion that water table position change is the main reason for the TB decrease over the months. For more rapid temperature changes, those can be explained by the high frequency fluctuations in the brightness temperature time series.

	V	*H*
Use April surface temperature	*238.2*	*212*
Use 5K lower surface temperature and move water table 0.5m higher	*237.8*	*211.7*

The liquid water content values are from the measurement values from Dr. Lora Koenig's paper: Initial in situ measurements of perennial meltwater storage in the Greenland firn aquifer, GEOPHYSICAL RESEARCH LETTERS, VOL. 41, 81–85, doi:10.1002/2013GL058083, 2014. The temperature profiles and the liquid water content are derived from the paper.

For the second Greenland site, the assumptions are similar to FA-13 but in this case the liquid water content of the aquifer is set to 10% (line 303). No justification is provided for this value.

I want to apologize for the typo in table 2. FA-13 is using 20% of LWC rather than 10%. The permittivity values are the same if you look at table 2 and table 3. So the 2 are using the same liquid water content.

For the Antarctic test site, the water table level is "adjusted as shown in figure 10" (lines 315-316).

Overall, it seems that the work relies on too many assumptions, in several cases without proper reference.

- In Fig.4 and 5 it is possible to see two temperature profiles, but nothing is said about the day of the year on which they were collected. Considering that the paper analyze a time series of 4 months, the temperature swing in the snow/firn is appreciable and not considered.

  Thank you for the comments. The temperature profile is collect in April 2013 in FA-13. According to the measurements shown in Dr. Lora Koenig's paper: Initial in situ measurements of perennial meltwater

storage in the Greenland firn aquifer, GEOPHYSICAL RESEARCH LETTERS, VOL. 41, 81–85, doi:10.1002/2013GL058083, 2014.

The effects of surface temperature swing is discussed in the previous point.

- Ice permittivity models from Mätzler 1996 and Tiuri 1984 are appreciably different (line 191). More details should be provided about the use of these models.

  We use Matzler's model for the real part and Tiuri's model for the imaginary part.

- In the simulations it is not cited the inclusion of the temporal temperature swing in the upper layers that, given the shallow thickness of the snow (about 10m) can have an impact on the Tb. And maybe contribute to the "cooling" trend of SMAP Tb.

  The swing effect was discussed previously. The surface temperature in December is lower than April which means the surface temperature cannot be the driving force of the cooling trend since surface temperature in increasing in the general trend from December to April, it is not the same trend as in the brightness temperature.

- Having the model parameterized in section 3.1, a sensitivity analysis is provided in section 3.2. However, given the many assumptions often not referenced, the representativeness of the trends found seems at least questionable.

  As we responded in the previous points, the seasonal swing is discussed above and the liquid water content is provided from previous research works.

  At line 397 the paper says "This model eliminates the ambiguities.....where different parameters ae needed to explain ....". Actually, given the number of assumptions made, this sentence sounds too optimistic.

  We want to clarify this point. The parameter difference is about the different parameter used for density variation properties and firn aquifer permittivity value. In previous work of Bringer 2016, the V and H pol brightness temperature time series show major differences of 40K using a layered medium model while the SMAP brightness

temperature time series have difference around 20K. Temperature profile would not affect the difference between the V and H pol data. The layered medium model could not match the V and H pol time series data with a same set of density and liquid water content parameters. Using a single set of parameters to explain the brightness temperature difference is one of the achievements in this paper. The statement of "

This model eliminates the ambiguity in the previous 1D random layering structure model, where different parameters are needed to explain the different polarized brightness temperature data" is changed to "

This model eliminates the ambiguity in the previous 1D random layering structure model, where different density and aquifer permittivity values are needed to explain the different polarized brightness temperature data"

Minor points

- line 26, there is a red "s" in aquifers.
- line 56, here the second test site is 5km far from FA-13 while at line 298 is 6km. It is better to use a single value for coherency.
- What is the Tb reduction? It is not a common parameter and should be described in the text, not in a table (line 289).
- section 2.1. This section is redundant and the formulation can be found in many books and papers. I strongly suggest removing this section that adds nothing to the discussion and leave just some proper references.
- Figure 3 must be improved, for instance by using inset with large-scale maps, a clearer (lat, lon) grid, etc.
- section 3.1.2 seems misplaced and should be moved close to the model description.
- in line 332 the first "H pol" should be "V pol"
- the panels in figures 10 and 11 should be represented in a unique figure each to ease the comparison of the curves.

- In figure 12, the different limits of y-axes makes the comparison of the two different cases difficult. Better to merge the two panels into one.
- What “the increased water content contributes constructively to the decreased water table depth” means at line 367?
- lines 388-394 are redundant and can be shortened or deleted at all.

Thank you, we will address these issues when revising the manuscript