Response to reviewer #1

The reviewer's comments are in black and our answers are in red.
Modifications of the manuscript are reported in bold and italic.
The pages and lines reported here correspond to the original pdf.

The main aim of this paper is to evaluate the performance of the FIRR instrument under field campaign conditions. This is done successfully with overall performance shown to agree with laboratory performance within limitations imposed by the operational and environment conditions. Improving our understanding of the distribution and radiative effects of cirrus clouds in Arctic climates is highly important and TICFIRE a very worthwhile endeavour. Testing and improving the underlying technology for TICFIRE through the FIRR instrument is therefore crucial and this paper highly relevant.

Of the four main objectives mentioned at the end of the introduction I would suggest that the measurements described are not strictly a radiative closure experiment, the atmospheric state is not sufficiently well known to allow this. Similarly for the verification of the spectral signatures of cloud radiance. The work does assess the FIRR radiometric performance and demonstrates the sensitivity of FIRR measurements to atmospheric characteristics. The inclusion of the section on atmospheric cooling rates is not helpful for the objectives of the paper a fact emphasised by the lack of zenith view data. This can be omitted without impact on the paper. I would like more detailed information on the in-flight variability of the stabilitary data set, such as local humidity and ambient temperature, particularly at fixed flight levels. Please see additional text below.

We believe that the temperature and humidity measurements are sufficient to perform the radiative closure experiment in clear-sky conditions, given that many similar studies have called "radiative closure experiments" comparisons of radiance measurements to simulations fed by radiosoundings data, which is what is presented here. On the contrary, we fully agree that in cloudy conditions we do not have the necessary information to close the radiative experiment. We lack substantial information about cloud properties and the encountered clouds were too heterogeneous. This was already highlighted but is now stated more clearly.

We changed the introduction so that there is less confusion possible between the objectives of the campaign (which include radiative closure in all sky conditions) and those actually achieved. We also explicitly say that the radiative closure is completed for clear-sky conditions, while it is not for cloudy conditions.

p3 l.15 : "In the context of TICFIRE, ***there were four main reasons of*** flying the FIRR in the Arctic: "

p12 l.7 : "FIR simulations provide strong validation of the radiative transfer model, ***resulting in a satisfactory radiative closure for clear-sky conditions.***"

p22 l.13 : "further campaigns in the Arctic winter remain necessary, ***in particular to complete a radiative closure in cloudy conditions, which was not possible here due to lack of quantitative information about clouds properties.***"

p24 l.3 : "and their high heterogeineity. ***As a consequence, measured ice clouds spectral signature could not be compared to simulations with sufficiently well-constrained cloud properties. Such airborne*** campaigns"

As recommended by the reviewer, we removed the section of the discussion dedicated to the cooling rates, because it is mostly based on simulations contrary to the other results. Part of this section has been moved to the introduction to broaden the context of far-infrared radiation in the atmosphere and introduce the notion of efficient atmospheric cooling through LW emission of ice clouds.

Regarding the inflight variability of ancillary data, this is discussed in more details below.

Suggested changes to text:

Replace F-IR with FIR throughout text

done

Page2
line4: “host includes the strongly absorbing pure rotation band of water vapor” and coincides with a maximum in the water vapour continuum strength.

done

Line 8: “The emission maximum of Planck’s function...”

done

Line 11: Reference to the Mars climate sounder is not relevant.

This reference has been removed from the introduction to be mentioned only in the presentation of the FIRR instrument, because of the similitude between both FIR filter radiometers. The same is true for the Diviner Lunar Radiometer.

P4 l.17: “***In this sense it is very similar to the Mars Climate Sounder (McCleese et al., 2007) and the Diviner Lunar Radiometer Experiment (Paige et al., 2010)***”

Line 32-33: This is a little confused, the wording may be clearer. “uttermost in Arctic regions because as discussed proportionately more energy is emitted from these colder surfaces at FIR wavelengths while the same time lower water vapour column increases atmospheric transmission.

done, with slight modifications.

Page 3
Line 22: vignetting by the chimney edges? I assume

Actually the edges of the chimney are not in the field of view. The vignetting simply corresponds to standard vignetting, that is the fact pixels on the edge of the illuminated area receive a bit less signal than those in the center.

“to avoid the ***small vignetting on the edges of the illuminated area.***”

Line 28: “One spectral measurement thus corresponds to a 0.8 s...”

done

Page 4:
There needs to be specific reference to the fact that the measurements are comprised from the average of all pixels in the 15 pixel diameter area illuminated by the scene footprint. The authors highlight the advantages of fast scanning and the high radiometric accuracy of their instrument but in the operational configuration described individual spectral band measurements are, if I understand the text correctly, off-set temporally and hence spatially. This should be made clear at this stage and placed in context to the along track averaging. The sequence described indicates 0.8 s averaging per band, 9 bands per filter wheel rotation totalling 7.2 s observation time for all bands. Given 3 scene views and 2 calibration scans per cycle that equates to 36 s. The Authors indicate that one complete sequence last 210 s, there is therefore some considerable time unaccounted for, can the Authors expand on this and explain the implications, if any, for high variability scenes such as that observed in the cirrus observations.

To insist on the fact that measurments correspond to spatial averages over the whole illuminated area, we slightly modified the text that was already quite explicit about this:

“In this study, the FIRR is not used as an imager, hence ***the data presented here correspond to averages over*** the selected area of 193 pixels”.

This is true, the acquisition of all channels is not simultaneous, hence consecutive measurements do not exactly correspond to the same scene. This is clearly stated now.

“for higher signal levels. ***Note, though, that measurements in successive spectral bands are offset temporally, hence spatially, which has to be borne in mind at the stage of data interpretation, especially when significant scene variations occur in less than 20 s.*** ”

As for the total duration, it can be roughly decomposed as follows:
- 0.8 * 10 * 5 = 40 s taking measurements on 10 filter wheel positions (a blank measurement is also taken)
- 1.5 * 17 * 5 = 126 s rotating the filter wheel (1.5 s to move of one position, total of 17 positions on the filter wheel)
- 3*15 s = 45 s rotating the pointing mirror

This means that a lot of time is lost rotating the filter wheel and the pointing mirror, which is one of the major issues that we should work on in the future.

This is now detailed:

“One FIRR measurement sequence lasts 210 s, ***during which approximately 40 s are used to actually take measurements and 170 s are spent rotating the filter wheel and the pointing mirror***. A sequence consists of [...]“.

“that measures all 9 filters ***in approximately 20 s***”

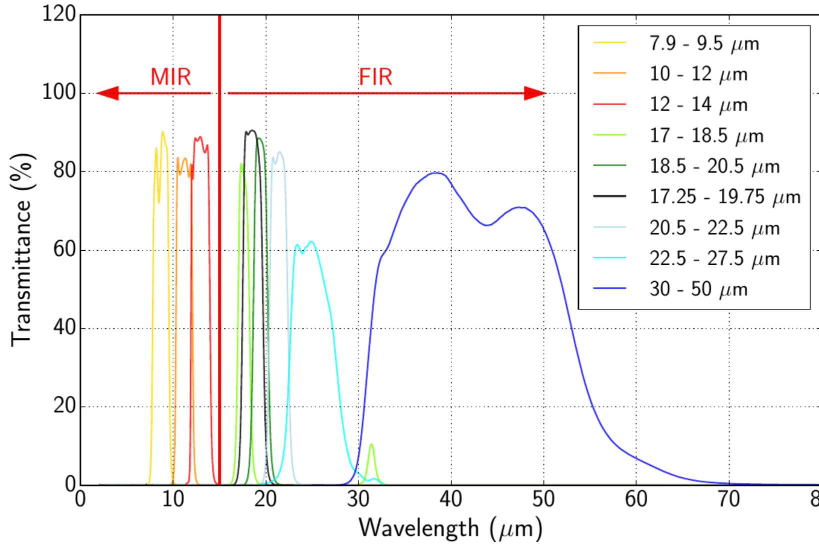
The impact of this temporal offset is already discussed in Fig. 4 that shows apparent spikes in brightness temperatures.

The impact in the case of hight variability scenes is now detailed in the last section of the discussion dedicated to the recommendations for future operations:

p23 l.7 : "***It would also ensure that measurements in all channels are taken on the same target, which was not always the case during the campaign above leads or through highly heterogeneous ice clouds.*** Such ***technical*** developments"

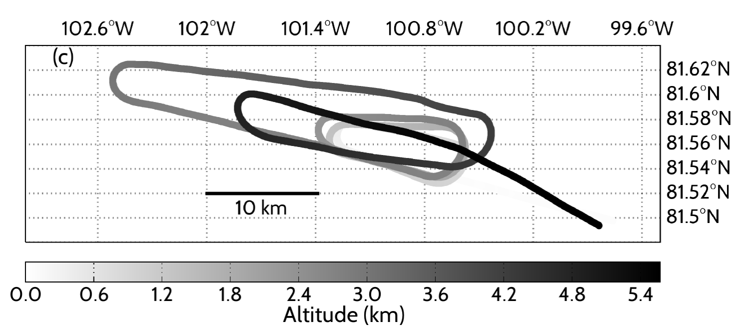
Page 5:
Figure 1 does not add a lot to the text and can be omitted Table 1 would be more informative replaced with a spectral plot showing the filter transmission, similar to that of figure 2a in the Author's earlier paper, "A microbolometer-based far infrared radiometer to study thin ice clouds in the arctic".

We removed Figure 1 but moved the picture 1b to the paragraph describing the issue we had with the input of air inside the instrument, the latter being difficult to understand without the support of such a picture of the hatch. As suggested, we replaced Table 1 by the filters transmittance, indicating the band pass in the legend of the figure.



Page 6/7:
The description of the flight paths for the aircraft lacks detail, the longitudes indicated on figure 2 (left panel) are wrong (75/60/45 degrees being 15 degrees out). Choose one flight and expand to show detail of the profile track more clearly. Alternatively a more detailed figure of the flight path could be included with the case details.

The longitudes were updated because they were indeed 15 degrees off.
A detailed flight path for the 11 April flight has been added to Figure 2. It shows the size of the spirals and the trajectory typical for a vertical profile at constant speed. The color indicates the altitude.



Page 8:
Line 6: Is the KT19 spectral response known and has this been applied derive surface temperature with the assumption of a spectrally flat surface emissivity of 0.995, be more explicit.

It was assumed that the KT19 measures the radiation in the range 9.6-11.5 µm (square response) and that in this range surface emissivty is flat at 0.995. This is now detailed.

P8 l.6 : "from the KT19 observations assuming a ***uniform spectral response of the instrument and a spectrally flat*** surface emissivity of 0.995 ***in the range 9.6-11.5 µm***."

Line 25-30:
How was the trend in ice temperature over the 30 minutes established, was this correlated against the KT19 data set for validation or was the KT19 data used to establish the trend?

This experiment was performed on snow when the aircraft was on the ground, so that only the FIRR was operating. The KT19 was not. Here we're interested in the resolution of the measurement, so that we removed the monotonic temporal trend attributed to snow temperature variation. This is now stated more clearly.

P8 l.28 : "for each spectral band. ***For all bands, the radiance increased continuously throughout the experiment, which was attributed to an increase of snow temperature. To remove this effect and focus on the resolution of the measurement only, the radiance series were first detrended,*** and the standard deviation of the residual was then computed."

Page 9:
Line3: "To further investigate the reduced thermal resolution observed..."

done

Line 21/22:
"the KT19 was -32.6C while a maximum of -24 C was observed in the atmospheric temperature profile between 1 and 2 km..."

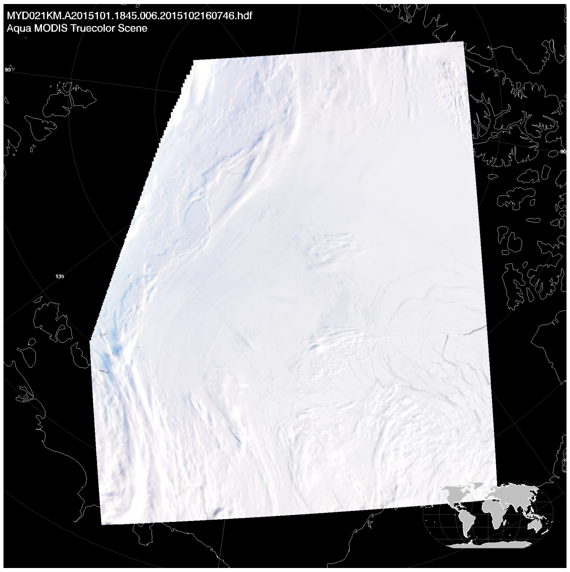
done

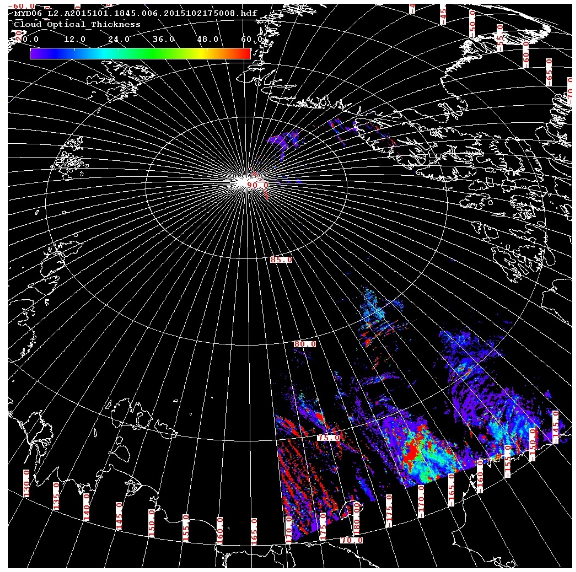
Line 23: I do not believe you can justify suggesting no cloud above the aircraft from CALIPSO measurements made 3 hours previously, are there MODIS cloud cover products that are nearer in time that you can use.

This is true. An Aqua MODIS image was taken above the flight area at 6:45 PM (see images below), while the spiral ascent took place between 7:00 and 7:55 PM. This picture and the corresponding cloud products show a very large clear sky area around the flight area. The text was changing accordingly.

p7 l.2 : "***Images taken by the MODIS and the associated cloud products are also used to investigate cloud conditions above the aircraft***."

p9 l.23 : "and the ***Aqua MODIS image taken at 18:45 UTC shows*** that no clouds"

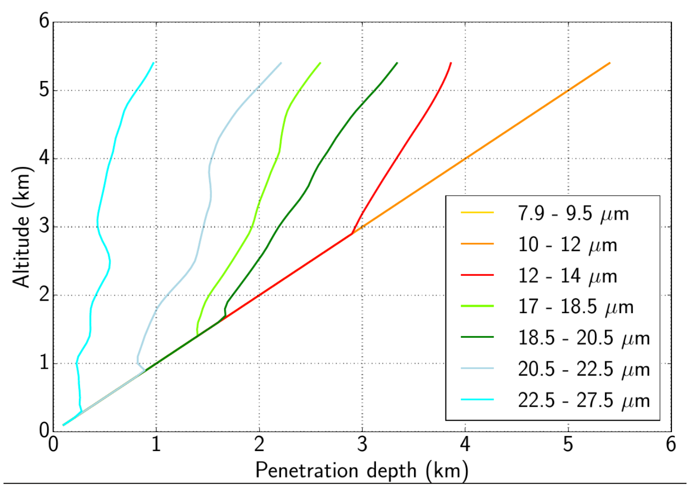




True color image (left) and cloud optical depth(right) from Aqua MODIS at 6:45 PM on 11 April

Line 25: A plot of the atmospheric transmittance vs altitude for each channel may help interpretation.

Following this suggestion the following figure was added. It shows the distance from the aircraft such that the atmospheric transmittance reaches 75%. It gives an idea of the distance to which each channel penetrates, which helps to interpret the radiance profiles shown in Fig. 4.



Some text was added accordingly:

p9 l.25 : "***To further illustrate this differential sensitivity to the temperature profile, Fig. 6 shows the penetration depth of each channel as a function of altitude. The channels that penetrate the least are sensitive to the conditions closest below the aircraft***."

Page 11:
Figure 4: 4c should indicate how the irradiance measurements were obtained.

“Vertical profiles of (a) temperature and relative humidity ***measured by in situ probes***, (b) FIRR brightness temperatures and (c) upwelling broadband LW irradiance ***measured by the CGR-4 pyrgeometer*** for 11 April flight.”

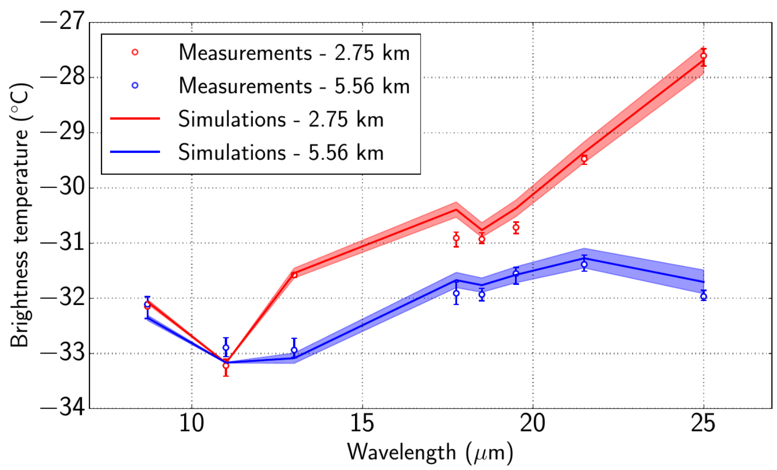
Page 12:
Line 1: Be more specific about what feature you are referring to.

We clarified this:

p12 l.1 : “measurements show an unexpected ***peaked minimum***. Although the origin of ***this peak*** is not ***fully*** understood”

Fig 5. Can the Authors include error bars on the simulations using realistic uncertainties applied to the atmospheric data set used in the radiative transfer model.

Complementary simulations were performed for the 11 April flight, namely one with humidity increased by 2.5% and temperature increased by 0.3 K, the other with humidity decreased by 2.5% and temperature decreased by 0.3 K. These uncertainties correspond to the uncertainties of the temperature and humidy measurements. These simulations were used to estimate error bars in Fig. 5 (see below).

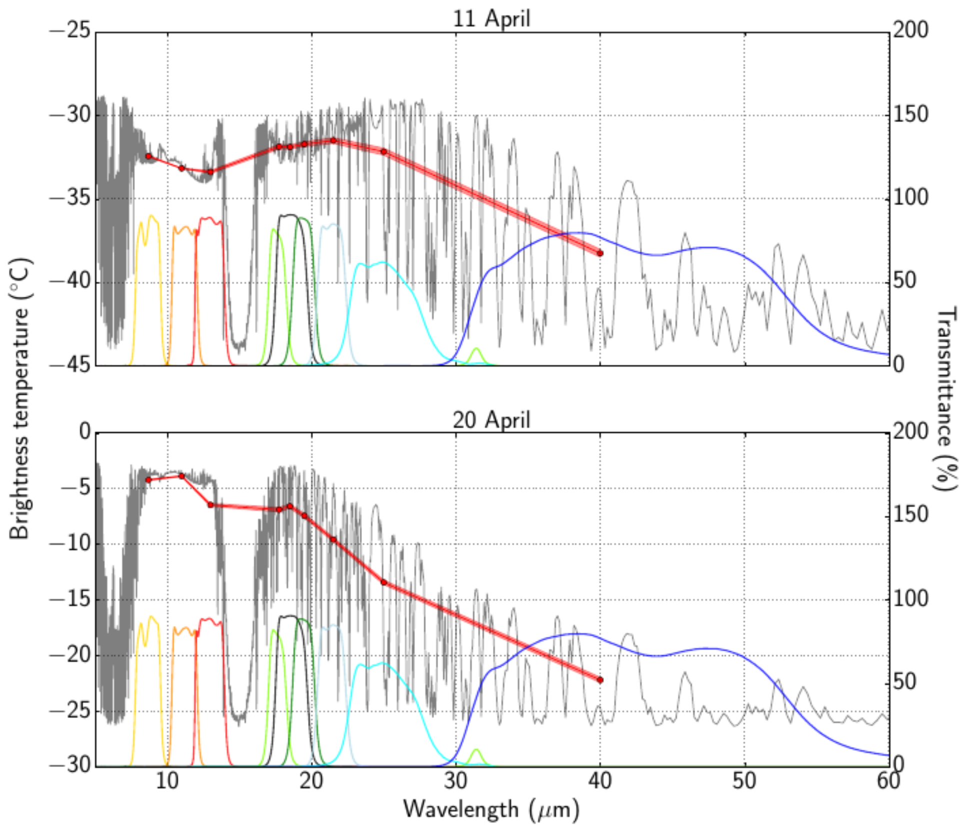


The text was also modified as follows:

p12 l.11 : “***In addition, most deviations between observations and simulations are within the range of uncertainties due to uncertainties of the temperature and relative humidity measurements.***”

Page 13:
Lines 19-34: It would be informative to see the spectrally resolved MODTRAN radiance output plotted as brightness temperature with the filter responses superposed, for the 11th, 20th and 21st April at the maximum aircraft altitude. Again uncertainties on the simulation BT’s would be informative for figure 6.

The following figure was added to show the simulated high resolution brightness temperatures. The 21 April case is not shown because it is somehow redundant with the 20. The text was updated accordingly.

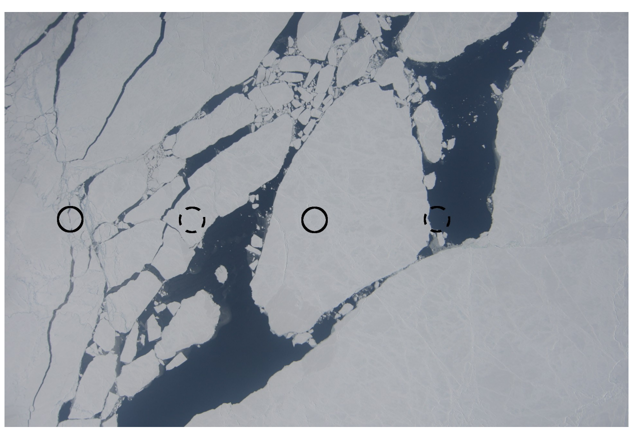


p13 l. 15 : "***The difference between the conditions encountered on 11 and 20 April is further illustrated in Fig. 9. It shows the high spectral resolution brightness temperature simulated by MODTRAN at 6 km altitude for both flights, and the corresponding simulated FIRR spectral signatures. This highlights the greater transparency of the atmosphere in the FIR for the 11 April***."

Page 15:

Figure 7 shows a 2-D image footprint for a 0.8 s scan, can the Authors include the relative positions for all 9 band observations along track for a single filter wheel rotation and indicate the position offsets between filter wheel cycles

We added some circles on the image to indicate when the first (plain line) and last (dashed) filters of a sequence are mesaured.

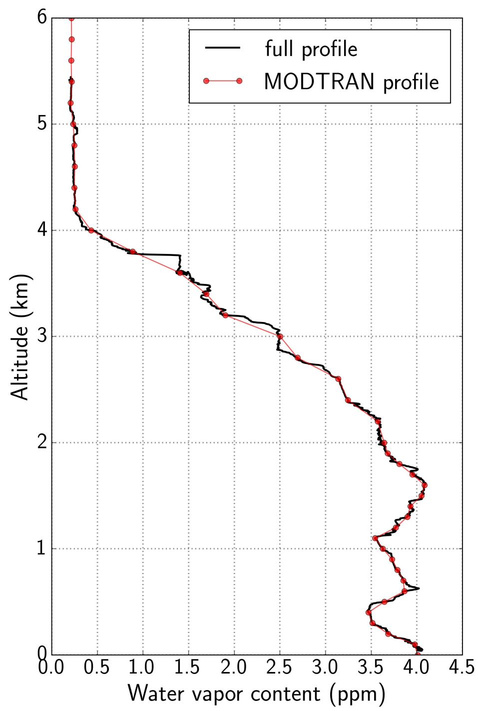


Line 3: “This question is left to future work...”

done

Line 5-6: You have no uncertainties placed on the MODTRAN simulations so stating the deficiencies here is not justified, for instance what is the along track variation in the measured humidity.

We show below the high temporal resolution measurements of water vapor, along with the “average” profile used for the MODTRAN simulation of 20 April. No significant variations of the water vapor are observed along track. The figure is not shown in the manuscript but this potential source of error is ruled out.



p13 l.32 : “***In addition, water vapor measurements along track did not show significant variability, so that spatial variability of water vapor can be ruled out. Only the incursion of a wet air mass below the aircraft before the end of the ascent could explain such a discrepancy between observations and simulations. In such case the water vapor profile used in the simulation would not correspond to the actual profile at the time of the measurement, but this is unlikely given that it was observed on two different flights***.”

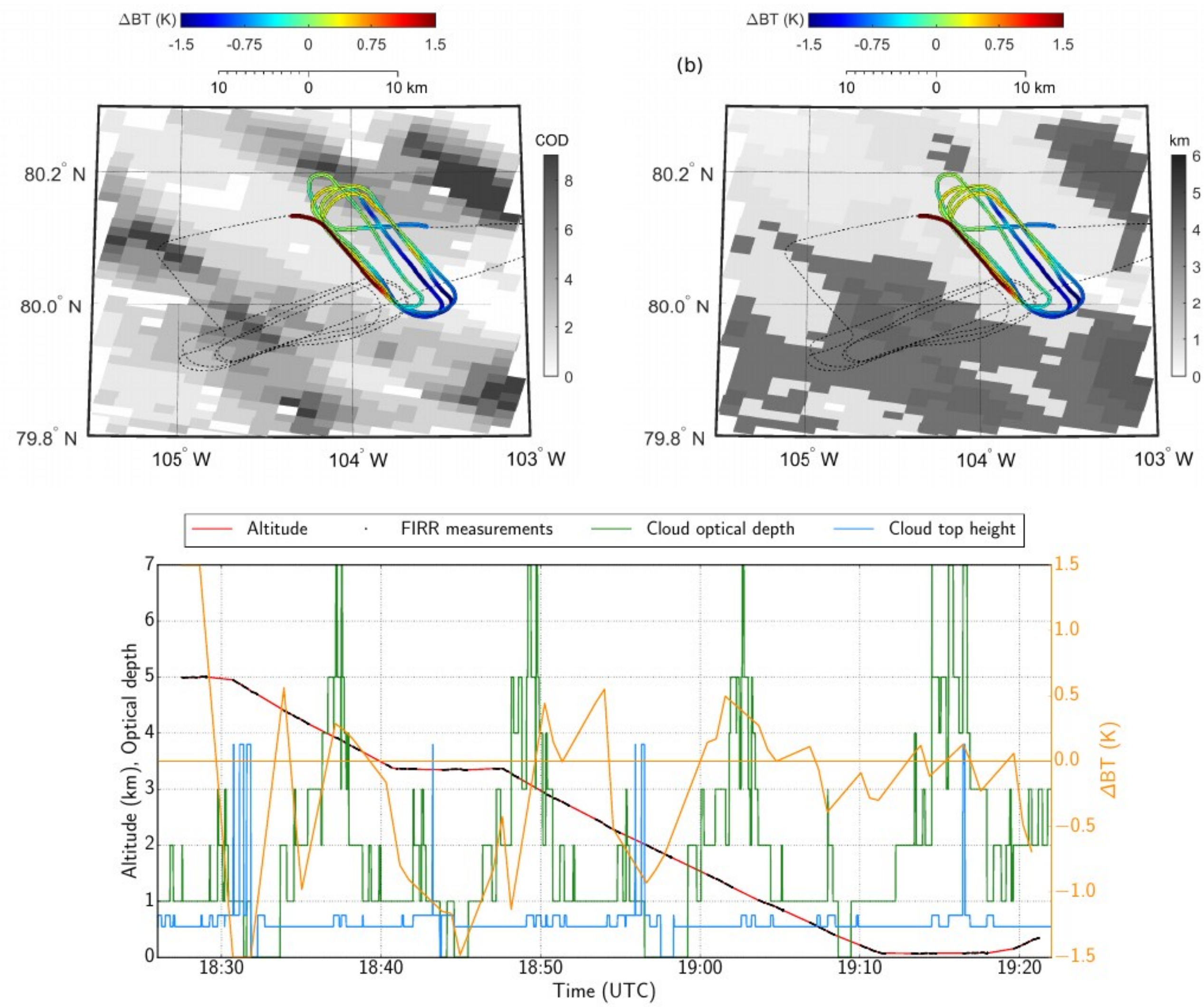
Page 16:
Line 6: “, consistently with relatively large particles seen consistently by the 2D-C probe”

done

Page 18:
Line 15:
Inferences made from reference to figure 10 would be enhanced with inclusion of a linear plot of relevant data sets as a function of aircraft altitude vs time (location). Colocated MODIS cloud optical depth/height can be superposed for reference.

The Figure 10 has been redrawn because the data shown were erroneously spatially interpolated. The new MODIS maps have been shifted by a few pixels, such that the interpretation is a bit changed in the manuscript.

Also, we added the timeseries of the brightness temperature difference, along with altitude and the time series of cloud optical depth and cloud top height corresponding to the maps shown in Fig. 10.



p18 l.14 : “In fact, the difference between the temperature measured by the 10- 12 µm channel and the simulation with $\tau = 2$ (indicated by the color of the trajectory in Fig. 13) is minimum ***near the area corresponding to the high altitude cloud***, ***which suggests that the cloud there has an optical depth larger than 2***. It is higher elsewhere, meaning that FIRR senses warmer temperatures corresponding to either a thinner or lower cloud. ***The variations of the brightness temperature difference are more evident in Fig. 13c, that shows the time series of the difference along with the MODIS estimates of cloud characteristics.***”

Page 19:
Line 4: “making them somehow somewhat redundant.....”

done

Page 20.
Atmospheric cooling rates:
Mlynczak et al 2011, The INFLAME design is such that the net flux is measured directly thus allowing instantaneous cooling rates to be established. It is my understanding that FIRR would require combinations of sequential measurements of zenith and nadir views, similar spectrally resolved measurements of atmospheric cooling rates in the far-infrared have in fact been measured, Harries 2008.

The section of the discussion dedicated to the cooling rates has been removed. Part of this has been moved to the introduction and recommendations for future operations.

Line 10: “The net flux was computed from broadband sensors”. What sensors are these?
The inclusion of this section on cooling rates does not benefit the overall interpretation of the FIRR instrument performance. In itself it is not new nor does it expand on existing work. The “measured” broadband cooling rates are not detailed and the lack of FIRR zenith data is a hindrance. In my opinion this section should be omitted entirely.

The section has been omitted as suggested.

Page 22:
Line 1: “field of fiew view....”

done

Line 16: “instrument resolution” What aspect of instrument resolution are you referring to, spectral, spatial, thermal.

We added “***radiometric***”