Reply to Reviewer #2

We thank Wilfred Theakstone for the insightful review of our paper.

Wilfred Theakstone

In this paper, Yde et al. report an attempt "to attain knowledge on the diversity of spatio-temporal δ18O variations in glacier rivers" by studies at three glacierized catchments in Greenland. The observations at Mittivakkat supplement studies undertaken there since the mid-1990s and are a useful addition to knowledge of the glacier. Most of the data from this site was collected during annual studies between 2003 and 2009. Kuannersuit Glacier is of interest because of its recent surge history: it has been in a quiescent phase since 1998/99. Data were obtained annually from 2000 to 2005, during which the nature of the glacier tongue underwent major changes. The Watson River drains a sector of the Greenland ice sheet. Sampling glacier river water for oxygen isotope analysis was more sporadic there than at the two other sites and it is only for 2008, when 42 samples were collected in a 45 day period, that the studies can be described as detailed.

1) The paper cites a large number of papers. It is useful to have these included in one place, but the citations hinder easy reading. Thus, partway through the paragraph beginning at line 18, page 5845, 17 papers are cited. It is not possible to check these citations in the References section without losing track of the text around them. Are all the cited papers relevant to the reported studies or are they included in order to provide a comprehensive list of papers dealing with oxygen isotopes?

AUTHORS: We think that it is important to show that end-member isotope-mixing studies are timely and widely used in glacierized catchments. Thus it seems relevant to refer to both pioneering studies (by Behrens, Fairchild, Theakstone, Mark), studies from different regions (the Andes, Himalaya, Scandinavia, the Arctic) and the many recent studies (seven studies published in 2014). As the reviewer mentions, it is also convenient to have citations to these studies together in one place. We hope that readers will see this as a resource rather than a hindering in easy reading. We have kept the citations, but if the editor wants us to reduce the number of citations, we are of course willing to do so.

2) The structure of the paper could be improved. I would have preferred to see separate 'Results' and 'Discussion' sections.

AUTHORS: We have restructured the manuscript as suggested. It now contains separate Results and Discussion sections.

3) The results do not always emerge clearly. For example, the authors start section 4.1 by stating that “information on δ18O is valuable for validating the proportional contributions of snowmelt and ice melt to dynamic glacier models” without further elaboration, and follow this immediately by reference to three snow pits excavated at Mittivakkat Glacier in 1999. Glacier ice data then are given, followed by speculation about the “reasons for an absence of a δ18O lapse rate”. The authors suggest (line 18 page 5851) that “it is evident that end-member snowmelt has a relatively low δ18O compared to end-member ice melt and that these two water course components can be separated.” It is difficult to find the data on which this conclusion is based. The data from the three snow pits at different altitudes are not provided – only a mean of -16.5±0.6‰ is given. Did the pits reveal isotopic stratification related to variations of winter storm activity? If so, how far did individual samples deviate from the mean value? How representative of all the samples is the mean?

AUTHORS: We recognize that the results were not clearly presented. We have now separated the Results from the Discussion and amended the text. The data from the three snow pits belong to Dissing (2000), but we do have access to the data. Information about altitudes of snow pits, number of samples, sampling frequency and range of individual samples have been added to the text. The pits show some isotopic stratification, but the variations have not been linked to air mass trajectories or storm activity as the Reviewer did at Tustervatn (Theakstone, 2008).

4) Sampling glacier ice at 10 m increments along profiles totalling 2.95 km in length is summarised by a range (-15.0 to -13.3‰ and a mean value (14.1‰. Did the sample δ18O values have a normal distribution around the arithmetic mean?

AUTHORS: Unfortunately, we do not have access to the data sets collected by Boye (1999), so we are unable to test whether the data has a normal distribution around the arithmetic mean. Boye (1999) did not test this.

5) The authors state (line 23 page 5851) that “the mean annual δ18O value was -14.68±0.18‰´’ and that “the uncertainty of δ18O is given by the standard deviation”. A better indication of the homogeneity/heterogeneity of the sample values would be provided by the Coefficient of Variation (standard deviation divided by the mean): two groups of samples, one more homogeneous than the other, may have different mean values but identical standard deviations.

AUTHORS: The reviewer suggests that we apply coefficient of variation rather than standard deviation to express uncertainties. However, coefficient of variation is only meaningful on a ratio scale (e.g., such as length, mass etc.). As $\delta^{18}O$ is given by an interval scale (having an arbitrary zero value), it is meaningless to apply coefficient of variation on $\delta^{18}O$ data. Thus uncertainties are given by the standard deviation.

6) The suggestion (line 3 page 5852) that δ18O values ranging from -15.16 to -14.35‰ in late May and mid-June respectively indicate that ice melt had started before sampling was undertaken requires elaboration. It is not clear why an increase of 0.04‰ per day is equal to an increase of 1.7 in the snow melt: ice melt ratio.

AUTHORS: We have rewritten these sentences to clarify that the similarity in $\delta^{18}O$ between the early melt season and peak flow period indicates that ice melt had started before sampling in May 2005 commenced. We agree that the sentence about the trend in $\delta^{18}O$ was not clear and we have decided to remove it.

7) What are the assumed "end-member δ18O compositions of snow melt and ice melt"? (In the introduction, it is noted (line 29 page 4845) that it may be necessary to divide ice melt into several components.)

AUTHORS: This is now clarified in the sub-section 4.1 "$\delta^{18}O$ end-member components".

8) Is the assumption of a standard value for snow melt justified? Does the composition of the water leaving the melting snow pack change as the melt season proceeds? This should be considered in relation to the hydrograph shown in Fig. 5.

AUTHORS: We take into consideration the Reviewer's point that a seasonal change in the isotopic composition of the bulk water leaving the melting snowpack will influence the value of the end-member snowmelt component. We do not have data to estimate a seasonal effect on the water leaving the melting snowpack. A study by the Reviewer (Raben and Theakstone, 1998) showed that the isotopic composition in snow pits on Austre Okstindbreen, Norway, remained unchanged in the early melt season but increased between May/June and August. However, a seasonal effect on the isotopic composition of the water leaving the melting snow pack was not estimated. In order to answer this question in detail, a combination of field sampling of snowmelt, local meteorological data and ablation modelling is required and this is beyond the scope of our study. We have added a short discussion of the uncertainties involved in using end-member estimates in hydrograph separation models (section 5.3).

9) At Kuannersuit Glacier, longitudinal and transverse sampling at the post-surge glacier

surface revealed large δ18O fluctuations. On the transverse transect, relatively high values were observed at the glacier margins. The authors suggest (line 11 p 5855) that there are no comparable studies of transverse variations. In fact, Hambrey (1974 Geogr.Ann. 56 147-158) studied such variations on a small Norwegian glacier and suggested that marginal ice there was older and originated at a higher level than ice in the centre of the glacier. The contrast might be worth exploring.

AUTHORS: Thank you for making us aware of the study by Hambrey (1974). We have now made a separate sub-section 4.4 “Longitudinal and transverse $\delta^{18}O$ transects”, where we present the results of our transects and compare them with the findings from Charles Rabots Bre by Hambrey (1974) and Saskatchewan Glacier by Epstein and Sharp (1959).

10) 180 samples of glacier river water were collected at Kuannersuit Glacier during six summer periods. A mean value of -19.58‰ is noted (line 18 page 5854), but this is the mean of the five individual yearly means of Table 4. If an overall mean is needed (it probably is not), it should be calculated from weighted annual values, as the number of samples ranged from 2 (2005) to 109 (2001).

AUTHORS: We now apply a sample-weighted mean annual $\delta^{18}O$ as recommended.

11) After a discussion of glacier ice sampling, the paper continues with an examination of glacier river water sampled on one day in each of four successive summers. This reveals a marked difference in the last year (Fig. 7). (It is hard to discern the ‘tendency’ in 2002 (line 13 page 5856). Indicating the individual values would be better than the line plot.). However, one day’s sampling surely is insufficient to define a “trend in diurnal variability” or to indicate that, in 2003, “the glacier runoff was not well-mixed” (line 23 page 5856) or to indicate “the presence of a well-mixed drainage network” (line 2 page 5857).

AUTHORS: The reviewer is correct. We have moderated or deleted the interpretations. We tested the reviewer’s suggestion to plot the data in Figure 7 as individual values but it did not improve the clarity – in our opinion the line plot makes the best visual presentation of the diurnal $\delta^{18}O$ variations. We think that it is important to show a figure of the diurnal variability of these four July days without rainfall, in combination with the long time-series from 2001, in order to visualize the lack of diurnal oscillations in the years following the surge event.

12) Section 4.2 is somewhat confusing; results and discussion should have been separated.

AUTHORS: The Results and Discussion sections are now separated as suggested.

13) The Watson River sampling programme was sporadic, rather than systematic. A reasonable body of bulk water data was obtained only in 2008 (Table 3). It is difficult to identify the basis for the conclusion (line 6 page 5860) that “the dominating meltwater provenance was near-marginal melting of basal ice”. Samples taken at different times of day on four days in 2005, one day in 2007, 5 days in 2008 and 2 days in 2009 (Table 5) or along the river on a single day in 2007 and 2009 (Table 6) are hardly a strong basis for a discussion of spatiotemporal variability of oxygen isotope composition in the Watson River catchment.

AUTHORS: We have taken the Reviewer’s point into consideration and decided to remove all Watson River data from the manuscript.

14) Study of this section of the paper (4.3) is hindered by the poor quality of Figure 2.

AUTHORS: We are sorry about the poor quality of the former Figure 2. It was intended as a high quality figure covering two pages, but it came out wrong in the preprint.

15) In summary, I consider that the oxygen isotope data from the Watson River catchment is not adequate for either a stand-alone paper or a comparative one. The Mittivakkat and Kuannersuit Glacier studies are of interest, the former as part of long-term observations, the latter because there is no body of oxygen isotope data from a recently-surged glacier.

AUTHORS: The manuscript now focuses on the two sites of interest: Mittivakkat Gletscher River and Kuannersuit Glacier River.

16) Any revised paper(s) should have more clearly presented data, separate from a discussion of the results. Concentration on a two-component mixing model (ice melt/snow melt) should be avoided unless a discrete value for each component can be identified.

AUTHORS: We have taken the Reviewer’s recommendations on board and separated the Results and Discussion. We believe that this has improved the clarity of the presentation of data. We have focused the paper on the isotopic differences between a surging glacier and non-surging glacier river catchment peripheral to the GrIS. We acknowledge the limitations of end-member hydrograph separation models (isotopic or hydrochemical), well knowing that a discrete value for each component cannot be identified and possibly does not exist over time. However, by using best-estimates of each component as end-member values we get information about the relative proportion of the components that is otherwise difficult to obtain. We believe that the use of isotopic hydrograph separation is legitimate in glacierized catchments, where the estimated values of each component are sufficiently different from each other (such as in Mittivakkat Gletscher River), but should be avoided in other glacierized catchments, where each component has not been identified within the catchment area (such as in Kuannersuit Glacier River). To address the limitations of the hydrograph separation technique, we have added a new sub-section (section 5.3) on uncertainties in $\delta^{18}O$ hydrograph separation models.