Hydrol. Earth Syst. Sci. Discuss., 12, C2270–C2273, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C2270/2015/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Spatiotemporal variability of oxygen isotope compositions in three contrasting glacier river catchments in Greenland" *by* J. C. Yde et al.

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Received and published: 28 June 2015

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. 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? The structure of the paper could be improved. I would have preferred to see separate 'Results' and 'Discussion' sections. 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$\pm$0.6‰ is given. Did the pits reveal isotopic stratification related to variations of winter storm activity? If so, how far did individual samples deviate from the mean value? How representative of all the samples is the mean? Sampling glacier ice at 10 m increments along profiles totalling 2.95 km in length is summarised by a range (-15.0 to -13.3‰ and a mean value (14.1‰. Did the sample δ18O values have a normal distribution around the arithmetic mean? The authors state (line 23 page 5851) that "the mean annual δ18O value was -14.68$\pm$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. 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. 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.) 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. 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. 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). 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). Section 4.2 is somewhat confusing; results and discussion should have been separated. 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. Study of this section of the paper (4.3) is hindered by the poor quality of Figure 2. 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. 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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 5841, 2015.