Hydrology and Earth System Sciences Discussions

Interactive comment on "Weathering rates and origin of inorganic carbon as influenced by river regulation in the boreal sub-arctic region of Sweden" *by* J. Brink et al.

Anonymous Referee #3

Received and published: 21 May 2007

The paper deals with weathering in two large contrasting catchments in northern Sweden. One catchment has been heavily built out for hydropower, while the other is completely saved from that. The former has its headwaters to the west of the Caledonian mountain ridge, giving high precipitation rate and large sea salt deposition and the headwaters of the latter is to the east of the mountain ridge. The authors find the weathering rate to be much larger in the pristine catchment than in the dammed catchment and speculate the reason to be shorter residence times for water in the dammed catchment. This is a very interesting idea, but the authors fail to prove this hypothesis for different reasons given below.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

The Material and Methods section is incomplete regarding handling of precipitation and runoff data. It is not stated if precipitation data are the recorded records or if, and to what extent the data is corrected for gauge efficiency. In the official precipitation network in Sweden rain is underestimated by around 10 to 20 % depending on gauge exposure and snow catch may be underestimated by as much as 40%. In runoff predictions this is not so important because the error is compensated for by a similar (with opposite sign) error in the evaporation term. In the case of chemical mass balance calculations on the other hand this may introduce a large error when precipitation rate is multiplied by chemical concentration in wet deposition. Also it is not stated how precipitation was interpolated to arrive at aerial precipitation. Precipitation is highly variable in the mountain region and the interpolation must take topography into account. To what extent that was done is not stated. The runoff from Sitas is given as 225 mm yr^{-1} larger than precipitation over the same catchment. There must be some error in the tables. In section 3.1 it is stated that flow discharge and precipitation volume was quantified with the HBV hydrological model. However, precipitation is a driving variable in that model and it is not clear what the authors mean, especially since they refer to an unpublished report (Sahlberg, 2004), which is not readily available.

According to climatological maps from SMHI (http://www.smhi.se) evaporation from the mountain area should be less than or close to 100 mm yr^{-1} and for the remaining ca 80 % of Lule river catchment evaporation should be 200 – 300 mm yr^{-1}. From data given in Table 1 the evaporation from Akkajaure catchment is 135 mm yr^{-1}, which may be a little high but still reasonable. For the rest of Lule river catchment Table 1 gives (after some calculations) evaporation of 140 mm yr^{-1}, which is too low. If precipitation over that part of the catchment had been corrected by 18 – 20 %, evaporation would have been the more realistic 246 – 258 mm yr^{-1}.

Precipitation and runoff data were not from the same time period for all sub-areas, for example the headwaters of Kalix river were represented by data from 1998 to 2000, while a much longer period was used for the other areas (1985 to 2003). Nothing

is written about how representative the period 1998 to 2000 was compared with the longer period. The sub-period could at least have been compared with the long period where the long record was at hand. Generally there is a lack of error analysis in the paper.

Water samples for chemical analysis were taken during one year in winter, spring and summer/autumn. For weathering calculations these data of runoff chemistry were compared with wet deposition for 21 years (and in one case 26 years). How do you know that your runoff chemistry samples are representative for the whole period? You don't even mention the problem.

Total elemental deposition for each catchment is estimated from bulk deposition (mainly wet deposition) and aerial precipitation. The precipitation chemistry stations are few and mean elemental concentration was calculated as weighted means of concentrations at given stations. In some cases only one station was used. Very little is said about how the weights were accessed. It seems as if you have used arithmetic means between the selected stations. Whatever you did, you have not told us how it was done. As I understand from the mineral compositions of the bedrock you describe there is no mineral containing chloride. Chloride is also passive in the water cycle and is one of the best possible hydrological tracers we have. Why not find the weights from a postulation that chloride input should equal chloride output? There are some extra chloride inputs from peoples food (but the population density is small in the area) and from road salt (in winter very little in that part of Sweden; in summer on dusty roads, an amount that could be estimated from information from the road authorities). If the weights are found from this balance it comes out that when only one station is used the weighing factor is larger than unity that is the deposition (aerial precipitation * deposition concentration) of chloride is smaller than the discharge. In the case of Kalix river mouth the chloride discharge is six times larger than the deposition. It seems as Pålkem is not a good approximation for mean deposition concentration.

Accumulation of cations in biomass was not considered (following Drever and Clow,

1995). It was about 22% of the weathering in the upper 80 cm of a soil profile in Vindeln, Northern Sweden (Lundström, 1990). Timber extraction in Norrbotten resulted in a loss of about 3 meq m^{-2} yr^{-1} basic cations. There has been a net increase in standing forest volume during the last decades (3,7 milj m^{3}sk yr^{-1}, National Forest Survey). Nutrient extraction from soil and storage in that biomass was not considered. The accumulation of soil organic matter and weathering products in that was not discussed.

P559 L4-5 and Table 1; There is something wrong here. What is named "runoff volume" is numerically equal to "mean precipitation", which is obviously wrong. Check where the error is.

P561 L6 Did you use the lake profile data in this paper? If not, why mention them?

P562 Sec. 3.1 The information given on treatment of precipitation and runoff data is not enough. E.g. was precipitation data (driving variable for the HBV model) corrected for gauge efficiency?

P 562 L 25 eq. Why element fluxes versus deposition. Deposition is an example of element flux and e.g. discharge is another example of element flux. Use congruent terms, e.g. element deposition and element discharge.

P563 There is some lack of consistency in definition of terms; "bulk precipitation" is used as total amount of elements deposited by precipitation, "bulk deposition" is used as element concentration in the precipitation.

P563 L5-15 It would have been interesting to see which weights you gave to the different precipitation chemistry stations. I wonder why you don't use Cl$^-$ as a conservative tracer. It seems you don't have any minerals that contain chloride. Then there should be a chloride balance from the headwaters and down to the river mouths. To achieve this balance you need to give different weights to the different precipitation chemistry stations.

P 563 L 3 and 4 Throughfall consists partly of dry deposition, wet deposition and

canopy leakage (from inside the leaves), which show that your definition of dry deposition and wet deposition is unclear.

P563 L5; How did you actually calculate total deposition from measured "bulk precipitation", i.e. wet deposition? What error did you introduce by this simplified calculation method? Be specific!

P563 L6; Table 3 should be Table 2

P563 L22-25 In a note to Table 2 Reivo chemistry is included, but here it is omitted. Which is correct?

P 564 L 8 Table 4 should be Table 6

P 564 L 15-20 Why is not marine contribution considered? Or is it? You have to subtract deposition before you find the ratios.

P 565 L 8-11 Chemical weathering rate should have the unit $mol\ km^{-2}\ yr^{-1}$. Whyagain divide by drainage area? What is done is not clear here.

P 565 L10 Table 6 gives F as specific weathering flux rate (i.e. $mol\ km^{-2}\ yr^{-1}$). Then the text here and Eq. 10 and 11 must be wrong.

P 565 Eq. 1 and 9 are identical.

P 565 L 9 Parenthesis says that what follow is the cationic silicate fluxes, but in the next line the cationic carbonate fluxes are given. Why is the parenthesis there at all?

P 566 L 7 μeq should be $\mu eq\ L^{-1}$ (I guess). If your unit is μeq then why do you calculate the sum of microequivalents of e.g. cations as sum $= (Na^+ + K^+ + 2Ca^{2+} + 2Mg^{2+})$? The equation looks as if you use moles, not equivalents.

P566 L 1 and L 9; Table 5 should be Table 4

P 566 L21 I would have emphasized contribution from sea salt (as spray or wet deposition). The high Na and Cl concentration in precipitation in the western parts is not

taken up in the Discussion section, even though it has a dramatic influence on both mass balance calculations and error propagation.

P 567 L 6-9 The first part of the sentence is correct, but you cannot from that infer the last part of the sentence. See comment on P 569 L19-23.

P 568 L 5 It would have been nice to have turn-over rate for major water bodies, at least for the large lakes.

P568 L 11 Table 6 should be Table 5; L 28 Table 6 should be Table 5

P 568 L 27- P 569 L 3 It seems very odd to relate silicate weathering to silicate rock outcrop, since weathering is much dependent on surface area, that is weathering rate is much faster from soil than from rock. Why not relate it to silicate rock area according to the geological map. Then it would be possible to weigh physical weathering products from silicate rocks from those from other rocks.

P569 L3 Use unit kmol km^{-2} yr^{-1} and you get rid of the ugly "x 10^3" everywhere.

P 569 L 19-23 Frankly, isn't it a positive correlation that has been observed. I think it is not really shown that there is a mechanistic relationship between them. One reason for a positive correlation between Si and DOC/TOC could be that less clay minerals are formed when DOC is high, because Al is complexed by DOC. In such case weathering rate is not affected, but the transport and fate of weathering products are.

P 569 L22-27 I get the opposite when assuming chloride to balance that is using relative weights for your precipitation chemistry stations that give the same chloride input as output via runoff. I got Lule total 92, highland 122, lowland 86; Kalix total 86, highland 152, and lowland 78 kmol km^{-2} yr^{-1} respectively. My calculations gave higher weathering rates than your calculations.

P 570 L 10 Another reason for decreased weathering rate in Lule river may be less soil particles in suspension (which would give a high concentration gradient of weathering products close to the particle surfaces) after dam construction.

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

P 570 L 16-18 Is it really so? The shift towards the Si-corner in Fig. 3 is hard to see.

P 570 L 19-22 Did you eliminate sea salt influence before normalizing by creating ratios to sodium? If not I cannot see any rationale for dividing with sodium.

Section 5.2 contains a lot of talking and very little that is connected to your data. I would rather like a deeper discussion on your actual data. What are the limits in your data set? How well is deposition estimated? What degrees of freedom does that give for your discussion? How large differences do you need to be sure that you have observed any difference? What is the effect of different lengths of the different data sets used and different time periods? Can you draw conclusions on differences less than e.g. 20% or 50%?

As stated on comments to P 569 I came to quite different conclusions from your data. I have a feeling that your discussion and conclusions stands on very weak ground. You calculate some figures and give a story that fit them, but you don't discuss the possible errors in the same figures. Maybe there is no ground for any conclusion.

P571 L21-25 In the high alpine Valtajokka it is likely that the deciduous forest (13%) is mainly found in the lower parts that are in groundwater discharge areas. The influence of increased carbon dioxide partial pressure due to root activity here would have limited influence on weathering and I would not by your argument.

P571 L 29 Table 7 does not exisit

P572 L7 To call it a theory is a little too much.

P572 L10 Table 6 → Table 5

P572 L15-17 You don't have degrees of freedom enough to state that the reason for a difference in silicate weathering rate is difference in transit time followed by difference in lithology. You never presented any data on transit times. From difference in specific discharge you get one part of the answer but you did not present any data on soil thickness, hydraulic gradients and porosity. And you never presented any data on

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

lithology for Killingi catchment.

P573 L 3-5 Chemical processes does not "favor" some processes before others. It is only a matter of equilibrium and kinetics. In an environment with alternating minerals with different weathering resistance the easiest weathered mineral in each sub-volume of rock will first be attacked. In other sub-volumes other minerals may be the easiest weathered. In a landscape the integrated effect of this would be a relatively large contribution from easily weathered rocks but also a contribution from less easy weathered rocks.

P573 L3 "optimizing" is a strange word.

P573 L 15-17 It is rather the opposite. The soil/water contact increases and the transit time in the soil increases. On the other hand the result of damming on ground water flow pathways is that a smaller portion of water from uphill will penetrate the soil, and especially the deeper soil layers. That means that less carbonic acid will penetrate to deeper soil layers and contribute to weathering there.

P574 L10-13 "The organic transport of carbon ...." Is it really organic transport? Is it not "Transport of organic carbon ...?" There are many of these peculiarities in the text, which the authors or a linguistic skilled person should carefully check. In line 10-13 please observe that the organic carbon was originally sequestered from the atmosphere by photosynthesis.

In the first sentence you talk about transport of carbon, but in the next sentence, where you want to show the size of it you give us not transport figures but concentration figures. Is it your intention to mislead the reader (large figure for Kalix, small figure for Lule). The transport of TOC is actually equal between the rivers; 1.95 Gg km^{-2} yr^{-1} for Lule river and 1.99 Gg km^{-2} yr^{-1} for Kalix river.

Why refer to Nilsson (2004) who used monitoring data for the period 1990-2000 when you use the same data source but the period 1985-2003 in table 4. I suggest you stick

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

to the data set you have already chosen.

P574 L10-27 How about the titration (and degassing) of bicarbonate by organic acids when bicarbonate rich groundwater discharges into the water courses? It will decrease total carbon to the sea.

P574 L21 insert "be" after "necessarily"

P574 L24-27 This cause – effect is not really shown (but a wish)

P574 L25-26 What do you mean by "a destruction of soil layers"

P575 Conclusion: First paragraph; You have not given data for this conclusion. Second paragraph; According to my calculations with your data and postulating chloride input equal to chloride output I get opposite results compared with yours. Third paragraph; It is not a good idea to introduce a new definition for "organic carbon", especially since you mix the common definition with your new definition.

P575 L3 You have not given data enough for a general statement like "for sub arctic boreal climates ...".

P575 L9 shorter residence time

P575 L20-22 Carbon loss by soil respiration may be at least ten times as large as carbon loss as dissolved constituents in water discharge, so for the carbon balance it is more important to get better estimates of the former than the latter.

Table 1. Unit for mean precipitation should be mm yr^{-1} (not mm). You denote year in different ways, i.e. as "yr" and as "a" in the same table. Choose one of them and stick to that all through the manuscript. Specific discharge is given the unit L m^{-2} yr^{-1}, which is OK, but it would have been simpler to use the same unit as for precipitation, i.e. mm yr^{-1}(which is actually exactly the same, mm = L m^{-2}).

Table 1. There are some errors in your water balances. Evaporation ranges from -225(!) to +183 mm yr^{-1} with median 135. SMHI gives values from <100 in the west

to 300 mm yr^{-1} at the coast in the east. Did you correct the precipitation data (or are they observed values)? We know that we underestimate snow precipitation by as much as 40% and rain by 12 – 20% in our network stations. Non-corrected values influence your weathering calculations. At least you should state how you treated data. It seems as if the precipitation values for the mountainous areas are closer to correct than those closer to the coast, which seems to be about 20 % too small. Are the precipitation gauges different between these areas? Perhaps mountainous climate stations are automatic stations, while those closer to the coast are manual. Did you use hydrological years or calendar years in your water balance calculations? It seems as you use calendar years. For long time periods it does not matter, but for Kalix river headwaters you have only three years of data and the difference in snow water storage between the beginning and end of your period (using calendar years) might have been substantial. Temperatures are given with one significant digit only, except in one case where you give two significant digits. Were there differences in the measuring techniques or interpolation needed that motivate this difference?

Runoff volume is wrong in the table at least for Lule River mouth where it seems to be precipitation volume and Sitas where it is neither precipitation nor runoff volume. By the way, runoff from Sitas is larger than precipitation in the table.

You never use runoff ratio in the text. Why present it in the table? You give three measures for runoff in the table. One is enough, and preferable "Runoff (mm yr^{-1}). And why not give the three fundamental entities of the water balance that is Precipitation, Evaporation and Runoff, all in unit mm yr^{-1}.

Table 2. Why is ion concentration in precipitation so extraordinary high in Lule river mouth (e.g. Cl 18.84 μmol L^{-1}) compared with Kalix river mouth (Cl 5.15 μmol L^{-1})? Could it be contamination? This anomaly must have had large impact on your weathering calculations. It is also very important to consider the fact that the headwaters of Lule river is to the west of the main mountains, while the headwaters of Kalix river is mainly to the east of the mountains giving much lower precipitation totals as well as

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

lower sea salt concentrations in that rain. It would have helped if your text had been explicit on these facts.

Why is element concentration for Kalix river mouth weighted as 100% Pålkem (0% headwater), while Lule river is weighted as x% Pålkem, y% Arvidsjaur and (1-x-y)% headwater? There should be a weighing factor for headwater also for Kalix river.

Table 4. The legend says "Chemical analyses . . ." Why then give a note saying data not corrected for precipitation input? Chemical analyses should be what come out of a chemical analysis of a water sample (in this case).

Table 5. Why not use kmol or even Mmol instead of μmol? First sentence in legend; Contribution to what? Last sentence can't be interpreted. Read it without the parenthesis and you may see what is wrong. Silicate fluxes unit; why not use kmol km^{-2} yr^{-1}? In the table there is a "*" at the line HCO$_3^-$ referring to a note below the table, but below the table there are three notes identified with a "*". Does all three refer to HCO$_3^-$? I guess the unit for the first 15 lines in the table should be μmol yr^{-1}, not μmol. Why not introduce a line for chloride budget to put your fluxes in perspective.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 555, 2007.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper