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Biogeosciences Discussions

**BGD**

1, S335–S338, 2004

Interactive Comment

# *Interactive comment on* "Regional hydrology controls stream microbial biofilms: evidence from a glacial catchment" *by* T. J. Battin et al.

**T. J. Battin et al.**

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We mostly welcome the comments of both referees (#2 and #4). They have generally appreciated our paper, and have now suggested comments that will greatly improve the paper for BG. We will carefully consider these suggestions in our revised version, which will be submitted in due course.

Referee #2:

1. Rhithral streams are relatively more stable than kryal streams. The latter experience strong diurnal changes during ablation, while rhithral streams are subject to pluvionival fluctuations.

2. We will include the Brown (2003) paper. This makes in fact a strong case for our paper, and will be of interest to discuss apparent discrepancies such as sub point 1.

3. We do recognize that is was Milner (2001) and not Brittain and Milner (2001) who propose the longitudinal MZB gradients and "windows of opportunity".

4. We will eventually explore the possibility to discuss the link between DOC and

biofilms in more detail. Two recently published papers in Limnology and Oceanography will greatly help with this discussion. 5. We will reference glacial history. The referee is certainly right with the lack of replication sites. Yet it should be mentioned here that the logistical efforts associated with field research in remote high-alpine areas throughout the year is tremendous. Basically this prevented us from system-level replication. Our conclusions are based on four stream types within the same catchment. This conveys a certain perception of coarse-level landscape heteregeneity , which certainly co-varies with environmental forces. Therefore we suggest to change the title to: "Hydrogeochemical controls on stream microbial biofilms in a glacial catchment".

6. Water temperature in our kryal site was sometimes elevated during summer. Dark sediments and solar radiation probably contributed to sensitive warming of a very shallow water column. Therefore, this does not agree with the "4°C" epitomized by Ward. This can be easily discussed having the Brown paper on our desk now.

7. We briefly introduce the fulvic acid technique in the Method section and refer to the key paper by McKnight.

8. Basically we used the PCA to reduce the 4 variables to 2 dimensions for graphical representation and with minimal loss of information.

9. Conductance and calcium concentration were in fact highest around the onset of snowmelt, decreased successively with snowmelt to reach minimum values during ablation in summer.

10. Our data clearly indicate that it is geology but not the snow that imparts the chemical signature to the streamwater. We did not suggest snow to impart this signature, and do fully agree with referee #2 that we need to consider flow path of snowmelt. We will better develop this issue in the revision.

11. We can introduce some values on microbial biomass in the text to make this point clear – though they are included in the figures already. We agree that this could help



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the reader to better understand the meaning of "hot moments".

12. We need to better place some of the arrows indicating the onset of snowmelt in some panels. Snowmelt started later in the upstream sites!

13. Glacial flow will be at a minimum during winter. Yet besides groundwater it constitutes the major hydrologic source to the stream. Aquatic/terrestrial connectivity (e.g., seepage etc) is substantially reduced during snowcover, and groundwater is typically very low in DOC (especially in biodegradable DOC). Therefore, we suggest that the glacier constitutes an important "allochthonous" DOC source to stream microbial heterotrophs during winter.

14. The aquatic/terrestrial connectivity is of course implicitly contained in the Introduction. We will now include it more explicitly in our revision.

15. We agree that we should carefully discuss the various flow paths of meltwater, and will dedicate more space to this in the discussion.

16. Our results do not suggest a major effect of temperature on microbial parameters. Of particular interest would certainly be a temperature influence on bacterial production, yet regression analyses did not reveal any (not fully shown in the BGD version). We can include this point in the revision. 17. Flow path (in terms of mixing) is certainly a major controlling force here and obviously with similar effects for all entities considered. This could explain some of the coherent temporal patterns.

Referee # 4

1. Tributaries have higher sediment particulate organic matter (POM) and chlorophyll a. Reduced velocity and less frequent flow-induced disturbance allows a more copious algal community to grow in these streams. This is part of the benthic POM measured as AFDM. As correctly pointed out by referee #4, the tighter connectivity with the terrestrial milieu also increases inputs of allochthonous POM in these streams; peat is a major terrestrial source of POM fragments.

2. As mentioned above (sub point 7), we will add details on the fulvic acid method. We acknowledge that the mixing model is a very rough proxy, yet we believe that is suitable to reveal patterns at the landscape level (see also Hood et al. 2003, WRR). We used glacial ice FI values and FI values from water draining peat as the major end-members. We do not suggest that changes in FI values correlate with C. nivalis. We simply point to the fact that C. nivalis can be a major DOC source to glacial ice and that it occurrence can influence the FI value of glacial ice. It was certainly not our ambition here to discriminate between season, ablation and the occurrence of C. nivalis.

3. We will change the concluding line of the abstract.

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Interactive comment on Biogeosciences Discussions, 1, 497, 2004.