

Interactive comment on “Hydrogeomorphic controls on runoff in a temperate swamp” by S. C. Kaufman et al.

Anonymous Referee #2

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General comments

This paper provides a case study of streamflow, water table, dissolved oxygen and electrical conductivity during two summers in different parts of a swamp. The work suggests that relatively small changes (although they are called large changes in the paper) in summer precipitation between years can result in major differences in hydrological flowpath partitioning within the swamp. Basically there is change in hydrological signal when the water table drops below the surface. Response to rainfall is slower and less efficient and streamwater chemistry is different. This is to be expected.

I think the paper is just about worthy of publication but really the contribution is not of major significance. The authors try to link the work to wider processes or other environments but I think the authors could have a more humble conclusion by admitting that more work is required to assess whether their findings extend to other swamps.

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The paper tries to claim it does more than the data actually shows and I therefore recommend that the aims are re-written and clarified and that the results are not confused with conjecture. Furthermore I am not sure whether the findings are totally new. I have read a couple of other papers on groundwater reversal in peatlands including Devito et al. (1996) and Seigel et al. (1995), although the paper might be new with respect to forested swamps. However, as an indicative case study that shows that processes in swamps change between and within different summers the paper merits full publication following a number of modifications.

Specific comments

1. On page 485 line 27-30 the paper sets out the aims. However I am not sure that the paper succeeds in achieving these aims. The paper certainly does not have data sufficient to have analysed the 'hydrogeomorphic' controls on streamflow (only a couple of them) and since flow paths themselves (and their chemistry) were not measured I am not convinced the paper really assesses groundwater - surface water interactions. The paper in fact does what I say in the first line of my review above. I think the aims should be rephrased (perhaps some hypotheses tested) to more clearly reflect what was actually done. The aims as they stand are difficult to interpret.

2. On page 487 line 20 the paper describes piezometer nests that were used. However, I am not totally clear where the results from these nests were used in the paper. I think they may have been used to generate data on hydraulic gradients shown in Fig 6. However, this needs to be clarified in the results section.

3. Many of the results are in the discussion section. For example Figures 6 and 7 are not mentioned until the discussion. I think that for clarity all the results should be kept together in one section and then followed by a separate discussion. In fact the whole of the results and discussion section seem to merge into one and are not really distinct. I think some rewriting of these two sections would help clarify the main points of the paper without diluting the actual scientific findings from the site. Unfortunately

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the discussion section seems to contain a lot of conjecture without data to back up suggestions. This should be rectified in a revised version.

4. Page 488 lines 2-6 - Suddenly we are told that DO and EC are being measured. These were not mentioned earlier and it is not clear how measuring these is addressing the aims of the paper. I think perhaps if the aims of the paper (or even some hypotheses) were presented in a clearer way then it might be more obvious how DO and EC map onto the hypotheses/aims.

5. The two seasons investigated had precipitation of 344 mm in 2000, termed the 'wet' year and 304 mm in 2001 termed the 'dry' year. The difference is termed 'large' throughout the paper but actually the difference is only 12 %. Therefore relatively small differences in precipitation seem to have a major impact on swamp hydrology at the location investigated. However, the authors also note that both years had summer precipitation below the 30 year average. I wonder whether this means that the swamp is close to the threshold of change (i.e. from surface to groundwater dominated). In normal wetter years then the swamp would be well above this threshold. So what I am saying is that the authors could discuss the idea of a threshold of response for the swamp beyond which the dominant runoff production processes change. Essentially this relates to the point at which the water table is allowed to drop well below the surface.

6. Figure 3 plots EC and DO for both summers. However, without discharge plotted (I realize this is shown in Figure 2) it makes the data difficult to interpret. Furthermore a lot of the text refers to what happens to DO and EC during individual storm events and differences related to antecedent conditions. I think, therefore, that some graphs to show event response of discharge, DO and EC are necessary and that Fig 3 is difficult to interpret on its own. So maybe Figure 3 can be translated into a summary table of means and a couple of alternative figures showing storm event response should be plotted up instead.

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7. The conclusions section contains material that was not really covered in the paper. It is written in a similar way to the aims in that it tries to over-sell the work that was done rather than just admitting in simple language what was really done and what the implications are. For example, look at the following statement from the conclusions: ‘high variability in flow pathway characteristics in Beverly Swamp arise from contrasts in the sub-catchment topography, geology, stream inputs and wetland-stream interaction’. I cannot see in the results how the paper has demonstrated this. How were catchment topography and geology analysed? How were the wetland stream interactions analysed? Only DO and EC were measured at the stream outlets and while such data may hint at processes it does not provide hard evidence since the chemistry of the flowpath components was not measured.

More humble conclusions that reflect data actually presented in the paper should hopefully follow once the results and discussion section have been re-written and restructured as suggested above and once the aims of the paper have been clarified and expressed in a more straight-forward manner. As it stands the paper claims to do more than it actually does.

Technical corrections

Page 484 line 19 - ‘season to season’ - you only compared two summers.

Page 485 line 2 - What does ‘on the scale of water table movement’ actually mean?

Page 486 line 2 - Why was the survey not carried out over the entire year?

Page 487 line 19 - Rephrase “as well.”

Page 487 line 27 - Rephrase “A one-tailed t-test revealed that these two automatic gauges did not reveal.”

Page 492 line 17-19 - However, you can not be sure because you did not measure the DO or EC from the flowpaths themselves. This would obviously have been of great help in interpreting the streamflow values.

Page 493 line 8-12 - This is another example where the event plots of storm precipitation, discharge, DO and EC would be useful.

Page 495 line 7 - Please mention that this is 'saturation-excess overland flow'

Page 495 line 9 - It is not a large decrease in precipitation. In fact it is a relatively small difference and yet this seems to have been enough to produce very different responses from the swamp.

Page 496 line 2-5. I am not sure what the relevance of this is to the paper.

References

Devito, K. J., A. R. Hill, and N. Roulet. 1996. Groundwater-surface water interactions in headwater forested wetlands of the Canadian shield. *Journal of Hydrology* 181:127-147.

Siegel, D., Reeve, A., Glaser, P., Romanowicz, E., 1995. Climate driven flushing of pore water in peatlands. *Nature* 374, 531- 533.

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