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Biogeosciences Discussions

Interactive comment on "The effects of biomanipulation on the biogeochemistry, carbon isotopic composition and pelagic food web relations of a shallow turf lake" *by* B. M. Bontes et al.

Anonymous Referee #2

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General Comments: This paper describes the impact of fish removal on the biogeochemistry and lower -trophic food web relationships in a turf lake. In so doing, the authors employ a relatively new approach of combined fluorescence activated cell sorting and IRMS of fatty acids to yield species specific 13C signatures of algae and zooplankton. Further they are able to convincingly demonstrate that fish removal fundamentally altered the chemistry of these ecosystems by raising the pH and O2 and lowering the CO2 thereby shifting the source-sink nature of these lakes (e.g., biomanipulated lakes become a CO2 sink!). I see the paper as a significant contribution to our understanding on the controls and feedbacks between the biology and chemistry of these ecosystems. Further the species specific 13C work is an exciting contribution to our ability to trace

C through the trophic food web (despite the conclusions of the authors for this specific study- generalist consumers). I find the manuscript appropriate for publication in Biogeosciences. However, prior to publication I feel as though the Discussion would benefit from further revisions. Currently, the Discussion in many places is very qualitative and the organization and writing clumsy in sections. I suggest several relationships which can be explored to make it more quantitative. I feel once these suggestions are considered and addressed the manuscript will be significantly improved and will provide an important step in elucidating the coupled biological and chemical controls on C cycling.

Specific Comments: Introduction- Too much attention is given to the topics of eutrophication and biomanipulation. This is relevant background information, but it merely distracts the reader from the novel aspects of this study. Shorten these paragraphs (and potentially combine). You might rearrange the introduction and begin with the last paragraph on p.1000 (line 23) which is well written and highlights the unique approach of this study. Regardless, I would lead with a short intro paragraph that sets the stage for the unique contribution of this work, and how it fits into the larger picture, rather than a description of turf lakes. I found much of 2nd paragraph on page 1000 to be redundant to information presented later in the discussion (p.1013 lines 1-5;p. 1008 lines12-14) . I find the material is best placed in the discussion to enhance data interpretation. I would consider cutting from line 8-16 and folding the rest of the information into another paragraph. I would add a paragraph which stresses that clearly defined source end-points are required for using 13C to trace carbon flows through trophic foodwebs. The phytoplankton 13C endmember, however, has been difficult to constrain. Consequently, many previous studies have reached inconclusive results (France 1996 and others). The method presented not only allows an accurate measure of the phytoplankton 13C endmember, but also a species specific signature. I would add further emphasis to these points.

Methods Were the sorted zooplankton samples combusted (as a whole) for 13C anal-

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ysis (p. 1004 lines 15-18)? If not, state which FA were selected as representative of zooplankton biomass. If FA were used, was a correction factor applied for FA relative to zooplankton biomass as with algal components? What does the last sentence on page 1004 mean? Are you stating that grouped or various primary producers 13C signature were used as potential endmember sources of C? Baseline seems to imply a control or a value pre-manipulation. Is that what is meant?

Results I would rearrange the sentence p. 1006 line 9 to emphasize the dominant phytoplankton first (cyanobacteria) and follow with other algae “The -W and R treatments were dominated by high densitiesĚĚ.with additional contributions from green algae ( ) and diatomsĚ

Are DOC or bacterial abundance data available to support the hypothesis of increased mineralization of flab and macrophyte biomass by bacteria at the end of August?

Section 3.3 Reference appropriate Figures and Tables for results. Currently many statements are made without directing the reader to the appropriate Figure or Table.

The last paragraph of the results should be rewritten. I recognize that some qualitative description of trends should be given. However, as written it does not improve the readers understanding of the results- instead it is merely confusing. Given the error bars and overlaps of isotopic signatures in the -R treatment- it is difficult to conclude that specific algal species might have supported the zooplankton diet, rather it looks as though they were supported by a mixed diet.

Discussion: I find the discussion in general in need of some major revisions and rewriting. Often, the results are discussed very qualitatively. The writing needs to be made more concise. The sentence on p. 1011 line 14 needs to either be further developed or folded into another paragraph. It should not stand alone. On p. 1012 lines 11-16, this section should be rewritten. The portion discussing OH-(aq) and HCO3-(aq) is confusing and needs to be properly referenced. Further on p. 1012 line 18, I can not figure out what the authors are trying to say. Please rewrite.

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On p. 1013 line 9- p. 1014 line 12, the authors discuss the results very qualitatively. For example, did the authors explore whether there was a relationship between d13C-CO2 and the d13C of individual species? A quick plot of data in Tables 4-6 (Diatom 13C and 13-CO2) suggest there is not, yet the authors conclude diatoms and green algae are primarily supported by CO2 (aq) (p. 1013 line 10-12). This should be addressed - and these relationship explored for all algal species. Was the concentration of CO2(aq) plotted vs 13C of algal species (p. 1013 line 25; p. 1014 line 2 and line 12)? If the 13C of HCO3- is calculated is there a relationship with cyanobacterial 13C? The authors conclude qualitatively that eukaryotes are using CO2(aq) and cyanobacterial (HCO3-). It would help if some of these above suggested relationships were explored in order to support these conclusions. The equation of p 1014 line 6 needs to be appropriately references. Where do the numbers 20 and 10 which are varied come from? The authors might want to reference Finlay 2004 (L&O; 49: 850-861) and citations therein.

p.1014 last sentence- continued to p.1015- Please direct the reader to the appropriate graphs and figures for these conclusions (e.g. Fig 3a and b). Here (p. 1015), in the results (p.1009 line 7), later in the discussion (p. 1015 lines 3)- , in the conclusions (p 1016 line 8) and in the abstract, the authors refer qualitatively to a preference of certain zooplankton species for eukaryotes vs cyanobacteria. I think it would be beneficial here to make some assumptions, determine 13C endmembers (or a range using S.D.) and do a quick mass balance so that they can make this quantitative. For example they could take the average 13C of the diatoms and green algae (e.g. a combined eukaryote signature) and the average cyanobacterial 13C signature as the 2 potential source endmembers for several of the zooplankton, and calculate the actual percentages of each consumed using the equations: d13CZOO = f1*d13CEuk + f2*d13CCyano

f1 + f2 = 1

Where CZOO is the measured d13C signature of zooplankton species and CEuk and CCyano are the endmember values for eukaryotes and cyanobacteria, respectively.

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Lastly, in looking at the data in Table 4-6, I find that Asplanchna are at times more negative than potential algal sources (e.g. Table 5 12-Feb; 24-Mar; 25-Nov) I am curious as to why and think this should be addressed in the Discussion. Are these data corrected for fractionation differences between FA and biomass? In addition, I believe the change in Diatom signatures between 22-Apr and 19-May (Table 6) to be significant- especially given the small change in 13C-CO2(aq) again I think it should be noted.

Technical comments: p. 1002 replace nowadays with Presently or Currently

p. 1002 line 12 replace “was” with “were”

p. 1002 line 12-14, Combine sentences to read “Ĕmethod of Peterson (1896) with a total reduction of planktivorous and benthivorous fish stock by almost 75% (source Witteveen and Bos.).

p.1002 line 18. Rewrite to “For correct interpretation it is essential to note that wind, as well as fish, was significantly reduced in both enclosures.”

p. 1006 line 7 Should be Chl. a.

p. 1007 line 1 - should be Cumulative not Accumulative

p. 1007 line 4; zooplankton numbers were greater in -FW relative to -R as well, July not the only time.

p. 1008 line 9- were the d13C values of DIC and CO2(aq) averaged? If so why, I don’t think they should be.

p. 1008 line 20–Microcystis d13C data is not presented for -FW in Fig. 3a. This data if available needs to be added or this statement corrected.

p. 1009 line 23; Is there turbidity data? If so it should be referenced and included.

p. 1011 line 20 Change “upward and downward” to positive and negative.

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p.1012 line 28- Fig 3a should be referenced not Fig. 4.

p. 1013 line 6 change disentangle to elucidate.

Table 1- units for Chl a should be mg (micrograms).

Figure 2b caption- (370 designated by dashed line- not heavy)

Figure 3- Vertical lines need to be added between treatments- and -FW,-W and -R added to appropriate sections. State whether these numbers are corrected for 9 per mil fractionation of FA relative to biomass. Add “a” and “b” to panels.

Fig. 3a not references in text.

Interactive comment on Biogeosciences Discussions, 2, 997, 2005.