Response to reviewer comments, AQSC-S-16-00066, “Ecological determinants of Potamogeton taxa in glacial lakes: assemblage composition, species richness, and species-level approach”

The review comments are below with our responses in italics. Line numbers refer to those in the original submission.

Comments for the Author:

Dear Dr. Beck

Thank you for submitting your interesting study to Aquatic Sciences. We have now received two reviews, both of which suggest minor revision. Please, thoroughly respond to all comments in order to improve the readability of the manuscript to readers.

Reviewer 1 provides thorough comments, I agree with the Reviewer that restructuring the Discussion would make it easier to non-specialists to pick up the most important results.

*We have edited the discussion to improve readability. See comments below.*

Reviewer 2 was wondering

- the citation that P. crispus is light limited due to thicker snow depth on frozen lakes and

- why lake depth was more important determinant of species richness than lake size

I don’t see any conflict with these issues:

- Thick snow cover on frozen lakes can effectively decrease light penetration and

- Lake depth presumably better reflects important processes than lake size, e.g. greenhouse gas fluxes correlate better with lake depth than size. However, the availability of lake size data is much better than lake depth data, which might be reflected in literature, i.e. more citations to size than depth.

*We agree that our conclusions about snow cover and lake depth our justified. We have provided an appropriate response below to review 2.*

Reviewer #1: This paper describes how environmental and spatial variables structure the assemblage composition and species richness of Potamogeton species in Minnesota and Wisconsin. Variance partitioning was used to examine the relative contribution of various local, climate and spatial variables. Individual distributions of common species were also analyzed. I found the paper to be well written and interesting, particularly in light of the current interest in using aquatic plant community "health" metrics as a measure of anthropogenic disturbance. Most of my comments involve the clarity of the figures and some points made in the discussion. My recommendation is to accept this manuscript pending minor revisions.

The biggest source of concern/confusion for me is the discussion of dispersal limitation. It may be that my understanding of the terminology is limited, but I understand dispersal limitation as describing a situation where suitable habitat exists for a given species, but access to that habitat is blocked because the species is unable to get to it. It seems to me that the authors describe species being unable to colonize certain areas due to unsuitable conditions (e.g. Northern Minnesota=too cold, long winters; Southern Minnesota = too eutrophic), rather than those areas being blocked to colonization by lack of dispersal. I was unclear to me whether there are lakes with suitable water quality in Southern Minnesota that also lack diverse Potamogeton communities, and that an explanation for this might be dispersal limitation due to the number of eutrophic systems in that region. I think this is an important point to clarify, because there is currently great interest in the importance of dispersal limitation in the upper Midwest, especially as it relates to repopulation of lakes with native species (both fish and plants) after successful efforts to control pollution and restore water quality. Another potential factor that could limit dispersal is the relatively low number of lakes in the south and west regions. It seems to me that lake density could affect many forms of dispersal, from floating downstream to being carried via road by recreational boaters.

With that said, I think you do present convincing evidence for dispersal limitation playing an important role in structuring Potamogeton communities. The importance of the pure spatial effects could very well be due to dispersal limitation. The invasive P. crispus, which we would not expect to be fully dispersed throughout the study area like the native species are, gives us some good evidence to support this, since it has the highest fraction of variance accounted for by pure spatial effects even though it is not as limited by eutrophication as many other species appear to be.

I think that some of my difficulty interpreting the conclusions might be helped by restructuring the Discussion. The message that I took away from this paper, is that spatial effects, alone and through geographically structured local and climate variables, drive the distribution of Potamogetons in this region. I think that starting with the discussion of the pure effects alone contributed to my initial confusion. It seems contradictory to the reader, when you conclude on page 12 that geographic variation in environmental variables was unimportant, but on page 15 when discussing shared fractions of variation, you find that climate variables are geographically structured and the shared portion of the variance is substantial (which one would certainly expect to find). It is easier for my brain to work through how all of the variables (shared and pure) are related to Assemblage composition, and then move on to discussing Richness, etc. As I look back over the Results section, it might work better if the Discussion followed a similar order of topics, or if you used a similar structure to the Species Level discussion, which I found much easier to digest.

The Conclusion looks good overall. I wonder if you might want to bring up the dispersal issue again, in light of potential improvements in water quality that the we are working to achieve in the ag zone. Does this data make a case for stocking native species when water quality improves? All of our efforts to prevent the spread of invasive species will also hinder the re-establishment of native species. It may be a good point to add to your discussion of management implications.

I have attached some detailed comments in an Excel spreadsheet.

Page 6, lines 10-12: Were all of the surveys conducted in summer, versus early season surveys targeting P crispus?

Page 9, lines 17: replace 'for which reason the different models can be' with 'which allows the different models to be'

Page 9, line 56: Here you mention 38 significant axes with positive Moran I scores

Page 10, line 58: Here you say 12 of 58 spatial variables were selected - should this be 38?

Page 10, lines 25-37: To make this sentence easier to follow, I would suggest using semicolons to separate the important variables for each variable group. So I would change the comma to a semicolon after 'local variables' and 'climate variables'

Page 11, lines 29-31: Use of the word 'numerous' - I'm not sure if there is a convention for when the threshold to numerous is crossed - it looks like 5 species had explained variance less than one for local effects and 11 for spatial effects. I think 11 seems like it could be 'numerous', but 5 seems more like 'several' to me. Or maybe the threshold for 'zero' explained variance was higher than 1, in which case you can ignore this comment.

Page 12, line 27: You rarely refer to the tables in the discussion, but I found it helpful to refer back to them and would like at least an intial reference to the data being discussed.

Page 12, line 27-31: I don't understand how the relative importance of pure spatial variables suggests minimal effect of geographically structured variables. Doesn't the relative lack of importance of pure local and climate variables combined with the substantial variance accounted for by the shared variables point toward important geographic structure that the statistics cannot separate?

Page 13, line 11-14: I agree that eutrophication appears to be limiting Potamogeton assemblage and richness in the southern region, but I'm not convinced that this is dispersal limitation. It seems like the habitat is unsuitable in that area for most species, although there is evidence of dispersal limitation for P. crispus and a comparision with P. pectinatus is interesting since both are tolerant of high TP.

Page 13, line 24-25 Highlight that although only max depth and TP were correlated with richness, the amount of variance explained by these local variables was high.

Page 13: The discussion of depth and habitat heterogenity is interesting.

Page 14, lines 41-47: I'm not sure what you are getting at with the sentence about cold climate being an important filter for species. We just saw that species richness increased with colder temps.

Page 14, lines 48-56: I think that these conclusions are good ones, but they are better supported after including the individual species discussion about patterns in curly leaf and sago vs the rest of the species.

Page 15, line 24: add "combined" after "three variable groups"

Page 15, line 34-37: This is not a request to redo your whole analysis, but did you explore other methods like boosted regression trees? I liked the presentation of a similar analysis in this paper by Dallas and Drake (2014,Ecosphere5(9):1-13): http://onlinelibrary.wiley.com/doi/10.1890/ES14-00071.1/full

Page 15-16: I really like the species discussion - it flows better for me and is easier to follow than the first section.

Page 17-18, 58-4: This wasn't explained well enough to be clear to me (see the comment on line 13). The discussion needs to cover both the increase in species richness and we go north AND the lack of many species at the far northern edge of the study area. So we are seeing eutrophication pushing species back in the southern areas and extreme winters doing the same in the far north?

References: I tried and couldn't find a single mistake on the referenc list - nice job!

Figures (all maps): The map figures were difficult to read, especially on the version that I printed out. I think that the shade of the spots goes from grey to black when they overlap each other, but the contrast is not high enough for this to be obvious right away (I thought our printer needed toner at first). I think that the goal of these figures is to get an overview of the patterns, so the fact that the lake dots merge together into blobs is probably intentional, but it initially looks a little sloppy. I can't really say without playing around with these whether another way would work better - maybe it wouldn't.

Figure 1: The species geographic centers are weighted (sized) by species occurence: does that refer to occurence across lakes or the frequency from PI surveys, or a combination of both?

Figure 2: Glad you included this figure, it really helps to understand the variance partitioning. I'm not sure if it would be possible with out using color, but it would be cool if you could establish a shading scheme on this figure that you carried through to figure 4.

Figure 4: This one shows up ok (but not great) on my computer, but it didn't print out very well. I like the idea of how you chose the shading (giving each partition it's own shade), but without using colors the contrast level of the greys isn't high enough. It is really hard to distinguish the different sections of the bars in the lower two panels.

Reviewer #2: In general this is a very good paper representing recent modern data analysis with very extensive and detailed data. As a macrophyte specialist, I cannot tell much of analytical methods and therefore my comments are mainly related to discussion and some minor things.

Table 2.

- can Perimeter of lake be 0.00 km ?

- Color cannot be 0.00 - there must be some missing value or other error?

- I'm really wondering Secchi value of 40,88 m - it is really near the world records in lakes

<http://www.secchidipin.org/index.php/monitoring-methods/the-secchi-disk/secchi-records/>

*The minimum value for lake perimeter and maximum value for secchi depth were in error and they have been changed. We have not changed the ‘0’ measurements for color after verifying the measured values from the original data. Table 5-3 in Wetzel 2001 confirms that zero color in lakes is possible for Pt-Co units. These values were observed in only two lakes. See Wetzel, R. 2001. Limnology. 3rd Ed. Academic Press, California.*

Discussion:

I would like see slightly more discussion of Introduced species of which Potamogeton crispus is the only one. It is mentioned that it is invasive, but its role as introduced plant should be emphasized.

I have not seen the paper by Valley and Heiskary (p. 16, l. 17) but the citation that P. crispus is light limited due to thicker snow depth on frozen lakes sounds really odd. Please explain the possible relationship.

I'm also wondering why lake depth was more important determinant of species richness than lake size. Can you still check the results, because most international litterature highlights the importance of lake area.