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Discussions

Interactive comment on "Local nutrient regimes determine site-specific environmental triggers of cyanobacterial and microcystin variability in urban lakes" *by* S. C. Sinang et al.

Anonymous Referee #2

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General comments

The authors explore a very important issue in the management of water resources, i.e. the role of environmental factors in the development of toxic cyanobacterial blooms. In investigating this topic, they also recognise the importance of local selection factors in the occurrence of toxic blooms, which is a topic of crucial importance for management strategies. This last issue about local specificity however has not been broadly reviewed by the authors, which may actually have missed some previous important publications with regards to diversity of cyanobacterial blooms at the local (lake) and regional scale.

In general, the manuscript is readable and concise, however the authors should revise the text of the manuscript. A few mistakes can still be found, e.g. p. 11111 line 3-5 “The management of toxic cyanobacterial blooms is one of the biggest challenges due to the variability cyanobacteria biomass and cyanotoxins”. where a word is missing. Also p. 11112, line 21: “[. . .] are permanent lake” should read “[. . .] are permanent lakes”. Throughout the manuscript: Ammonium is an ion should be written NH4+

The manuscript has been reviewed by 2 people here, and we agree on the paper being worth of publication after major revisions. Our main concerns are: the text seems a bit overstated in the abstract, discussion and conclusions section, findings regarding site-specificity of environmental factors in explaining cyanobacterial dominance, and MC variation, are not absolutely novel and the paper would benefit from a more honest assessment of results in relation to previous work. This study is more confirming previous work than bringing in new knowledge. The correlations reported are not new, and do not bring much new information in this field or research. Furthermore, some environmental factors such as temperature and pH were measured but not included in the statistical analyses although the authors mentioned them as being important explanatory factors in previous studies. Hydrological and morphological characteristics of the lakes were mentioned in the sites description but never included in the study. There are also some doubts about the approach used to to test for lake-specificity.

Specific comments

Abstract When stating the objective in the abstract, “In this study, we investigated the site-specificity of environmental triggers for cyanobacterial bloom and cyanotoxins dynamics” The authors should use “microcystins” instead of “cyanotoxins”, which is a term too broad for this study where only one type of cyanotoxins, namely the microcystins, was investigated.

1. Introduction Objectives (2) and (3) described (line 10 to 13 of p. 11112) are somehow a repetition of the same objective. “Identifying the relationship between environ-

mental factors and cyanobacterial biomass and toxin dynamics bloom in each lake" sounds to me like it is a site-specific investigation of the relationships. I don't understand what the 3rd objective adds to the previous one.

2. Methods The authors use a rather complicated way to test for lake-specificity in the response of cyanobacterial biomass / toxicity, comparing the slope between 2 regression models (page 11118). It is not clear from the text what is the rationale for this particular approach, does this approach require correction for multiple hypothesis testing? In my view it could have been solved by adding lake ID as an explanatory variable in their regression models, and if significant) study its interaction with the other explanatory variables. In most cases it is fair to let all variables compete in the same model (after testing for collinearity).

3. Results This study was conducted over a period of a 3 months with bi-monthly sampling in each lake resulting in a time series of 6, 4, and 6 time point in lakes Jackadder, Bibra and Yangebup, respectively. However time is not taken into account in any of the analyses nor mentioned anywhere in the manuscript. Temporal data also need to be treated in order to account for temporal autocorrelation, which has an effect on statistical analysis. If/how authors have dealt with serial autocorrelation of data in their analysis has not been mentioned in the manuscript.

It is mentioned in the introduction that temperature and pH are known to be important factors in modulating cyanobacterial biomass and MC variability (lines 19-25). "A range of environmental factors, including nitrogen and phosphorus (Schindler, 2012; Srivastava et al., 2012; Chaffin and Bridgeman, 2014; Van de Waal et al., 2014), TN : TP ratio (Smith, 1983; Q. Wang et al., 2010; Van de Waal et al., 2014), temperature (Davis et al., 2009; Rolland et al., 2013), salinity (Tonk et al., 2007), and iron (Ame and Wunderlin, 2005; Nagai et al., 2007; C. Wang et al., 2010) have been shown to have pronounced effects on either cyanobacterial dominance, microcystin production, or both." In the present study, temperature and pH were measured, however, the authors excluded these variables in the statistical analyses without stating why they did

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so.

Section 3.2 Fig 1. shows the proportions of different genera in the cyanobacterial communities of the three lakes. Is the community only composed of these genera, or where there more genera present which are not shown in Figure 1? It seems strange to me that there is a diversity= 3-max 4 genera per lake. Does this figure only show the potentially toxic genera? The legend is not clear enough and this figure is confusing. Section 3.5 RDA: We would like to see the % of variance explained and the results of the test of significance by permutation. The results of the RDA should be more clearly reported. Also, all variables were included in the model, without removing auto-correlated explanatory variables. There should be an extra step of selection of the variables to include in the model to choose the best model to explain response variables.

Discussion p.11123, lines 1-2: "In this study, TFe was negatively correlated to cyanobacterial fraction in Jackadder Lake, while in Bibra Lake, a positive correlation was shown between the two (Fig. 3a and b)". The authors do not specify here that these results were obtained when all lakes were combined. The authors report that in the lake-specific RDA in lake Bibra (Fig. 3b) TFe is positively correlated to the cyanobacterial fraction. This section is confusing.

p.11123, lines 2-3: These correlations illustrate the cyanobacterial ability to dominate under low phosphorus availability" Were P concentrations measured in the study lakes ever low? According to Table 1, TDP values were between 12 and 40 ug L-1 and TP was between 20 and 1150 ug L-1. Therefore, I'm not sure if the P storage strategy described in this section can support the negative correlation observed study between cyanobacterial fraction and phosphorus concentration in the present. Previous studies have reported the threshold of phosphorus inducing cyanobacterial dominance being around 20-30 ugL-1 which is within the range of the results reported in the present study.

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Furthermore, Briand et al., 2008 is misquoted here. In their study, Briand et al. found a positive correlation between TP concentrations and Planktothrix agardhii cell density (PCA, Figure 4).

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