Dear Referee #2,

Thank you very much for your helpful and very positive comments on our manuscript "Local nutrient regimes determine site-specific environmental triggers of cyanobacterial and microcystin variability in urban lakes" by S. C. Sinang et al (HESS-11-C4963-2014). They provide very important feedback to improve this manuscript.

No	**Comments**	**Response**
1	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+	We agree to these comments and the manuscript will be corrected as follows: *1. "The management of toxic cyanobacterial blooms is one of the biggest challenges due to the variability in cyanobacteria biomass and cyanotoxins".* *2. "Jackadder Lake and Yangebup Lake are permanent lakes……"* *3. NH_4 will be written as NH_4^+ throughout the manuscript.*
2	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.	We agree that we might have some overstating statements in the manuscript. We will restate some sentences such as: Page 2 Line 17-20: *"The findings of this study suggest that identification of site-specific environmental factors under unique local conditions might be an important strategy to enhance positive outcomes in cyanobacterial bloom control measures."* Page 16 Line 5-7: *"These results illustrate that reducing phosphorus and iron concentrations in water bodies could potentially reduce the overall toxicity of cyanobacterial bloom, even though it might not completely prevent from the occurrence of cyanobacterial bloom."* Page 17 Line 11-12: *"However, reducing phosphorus and iron could reduce the amount of microcystin being produced within cyanobacterial cells."*

3	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.	We intentionally did not include pH and temperature in the analysis as these parameters were not significantly different between the study lakes. Moreover, all lakes are located in the same region and therefore subjected similar temperature. We will include this into the manuscript.
4.	Hydrological and morphological characteristics of the lakes were mentioned in the sites description but never included in the study.	We agree to this comment and we will include the lakes' characteristics to support our findings on different nutrient regimes.
5.	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.	We agree to this comment and the sentence will be rewritten as: *"In this study, we investigated the site-specificity of environmental triggers for cyanobacterial bloom and microcystin dynamics".*
6.	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 environmental 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.	We agree to this comment and these two objectives will be combined as: *"identify the site-specific relationship between environmental factors and cyanobacterial biomass or microcystin dynamics."*
7.	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	We agree that the suggested analysis could have been an easier option. However, we still believe that the method described in the manuscript using R was also applicable.

	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).	
8	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.	In this study, sampling dates in each lakes were two weeks apart and this could have reduce the chances of autocorrelation between the data points. To ensure that autocorrelation is not an issue in our data, we have carried out sample Autocorrelation Function (ACF) and Partial Autocorrelation Function (PACF) within the SPSS. From the analysis, the autocorrelation coefficients for all parameters were within the upper and lower confidence limits. Therefore, we are confident that our data are independent from each other.
9	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.	The figure only shows the proportions of potentially toxic genera. The figure legend will be revised as: *"Average biomass (μm^3 mL^{-1}) proportions of potentially toxic cyanobacterial genera in Jackadder, Bibra and Yangebup lakes during the study period."*
10	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.	In section 3.5, we believe that we have included the percentage of variance explained in the sentence below: Page 13 Line 10-14: "*The canonical ordination showed that 72, 80 and 70% (Jackadder, Bibra and Yangebup Lakes, respectively) of the combined variability of cyanobacterial fraction, cellular microcystin concentration*

		and extracellular microcystin fraction can be explained by the measured environmental factors (Fig. 3a–c)." Even so, we agree that test of significance by permutation should be included. Therefore, we will include the F value and Prob (999 permutations) to section 3.5.
11	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.	In this section, the site specificity of TFe was described based on the RDA analyses carried out on each lake separately. Only the general correlation pattern presented in Table 2 was obtained from analysis on the combined dataset.
12	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	Similar to our responses to Referee #1, we agree that our use of the words "low phosphorus availability" were not strict enough and we will carefully edit our manuscript accordingly. For instance, we will substitute "low phosphorus availability" [p 13 line 3] or "phosphorus limited conditions" [page. 15, lines 27-29.] with "lower relative phosphorus availability". Regarding to the P storage strategy: we will make this clear by changing this sentence (will now read: "Although cyanobacteria as a group can dominate under a wide range of conditions, high phosphorus concentrations have been shown to potentially limit the ability of cyanobacteria to become dominant in the phytoplankton community (Chorus and Bartram, 1999;

	present study.	Reynolds et al., 2006). One reason for that is the higher grow rate of other phytoplankton compared to cyanobacteria, and, as such, their ability to utilize nutrients faster under high nutrient conditions.”
13	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).	We apologize for this mistake and we will make an amendment on citation to Briand et al., 2008 in this sentence.