Dear Dr. Julian

HESSD-Manuscript “River water quality changes in New Zealand over 26 years (1989–2014): Response to land use and land disturbance” (HESS 2016-323).

Thank you for providing the answers to the comments by the reviewers . I carefully went through the list of your replies. You responded to all points that were mentioned. However, not all of your answers are convincing and I list the critical aspects below:

Reviewer # 2:

Reviewer: “Furthermore the study primarily focusses on suspended sediment driven water quality constituents like suspended sediment concentration, total nitrogen and total phosphorus but the analysis is restricted to monthly data, hence the most important short term events with high concentrations of the abovementioned compounds are not considered in the study.”

You responded: “Furthermore, although targeted sampling of high flow events is very relevant for load estimation of particle-related contaminants, it is not appropriate for state-of-environment monitoring like NRWQN (e.g. Davies-Colley et al. 2011; cited), covering both dissolved and particulate constituents, for which random or pseudo-random (e.g. regular monthly as in the NRWQN) sampling is most appropriate.”

My assessment: This argument is only partially convincing. High flow events also matter and are part of the state of the environment. However, low flow conditions (generally well characterized by sufficiently long time series of grab samples and median values) and high flow events have different ecological relevance (see Stamm, Jarvie & Scott 2014 for illustration of my argument). Hence, what your sampling scheme and statistical methods reproduces are conditions that prevail for most of the time in the streams and what the organisms living there experience for most of the time. Therefore, it makes sense to look at these metrics and analyse these trends. However, this is not an argument to disqualify the critique that high flow events are not/only poorly captured by the sampling and statistical strategy. High flow events may be essential for water quality assessment (depending of parameters). This holds on the one hand if you think about downstream systems (including estuaries); on the other hand, it may also be essential for in-stream processes: if you wish to understand for example bed sediments in streams with all their ecological relevance you will hardly be able to do so by only knowing what happens during low flow conditions.

Much research has been done illustrating how grab samples may severely underestimate loads of compounds entering streams predominantly during high flows and how difficult it may be to detect trends in time with such a sampling strategy (e.g., Moosmann *et al.* 2005).

You have to explicitly mention these aspects in your manuscript and you have to make it clear to the reader what the results actually represent and what not. In the current version this is completely lacking: there is no discussion about sampling effects on results for example.

Reviewer: “Furthermore the manuscript is very long (41 pages text only) and not very specific including repetitions.”

You responded: “The manuscript is long because of our comprehensive coverage of both spatial and temporal effects of land use on a wide range of river water quality variables in complex large catchments. Arguably, the paper could be split into two manuscripts, but we feel it will have a greater impact as one paper. Further, an understanding of temporal effects is necessary in order to explain some of the spatial effects, and vice versa. We do not understand the comment ‘not very specific.’ We did a lot of investigation on land use practices and processes that were responsible for the patterns and relationships we observed. Maybe the reviewer is referring to our scale of analysis: catchment-scale. On line 95, we state: “Most of our analyses were performed at the catchment scale because it integrates the spatiotemporal changes that are reflected in our water quality measurements, it is the appropriate scale to analyze diffuse pollution, and it is the most appropriate spatial management unit (Howard-Williams et al., 2010).”

My assessment: I had a fresh look at the manuscript by reading it carefully once more and very much agree with the reviewer. Being comprehensive is nice but if this leads to a lengthy manuscript that distracts the reader from the essentials it has to be avoided. Being concise is beneficial to both – authors and readers: to the reader because he or she gets the relevant novel information as quickly and clearly as possible, to the author because the readers will like the paper more, which increases the probability of being cited later on.

Given the fact that Reviewer # 2 called for a clear focus of the manuscript and your suggested focus and effects of land use *intensity*, I strongly recommend that the result and discussion really concentrate on this aspect. Below, I list a some examples of lengthy and repetitive sections and paragraphs that should be avoided:

- L. 281 – 283, 740 – 741 (and elsewhere): These details on which catchments shows what is hardly relevant for the general reader. Only indicate such details if they illustrate an aspect a reader cannot not understand otherwise and which is essential for the manuscript.
- L. 297 – 312: This can be shortened.
- L. 417 – 429: These two paragraphs do not focus on changes in land use intensity, which should be the focus of the manuscript. Hence, they can be massively shortened or even skipped.
- L. 450 – 457: This paragraph can also be shortened without loss of information essential to the manuscript.
- L. 520 – 547: These two paragraphs do not focus on changes in land use intensity, which should be the focus of the manuscript. Hence, they can be massively shortened.
- Section 5.2 (L. 597 – 634): This is not the focus of this manuscript because you focus on land use intensity. You can shorten this part substantially without loss of information.
- L. 536: This is repetitive and can be skipped (L. 405 – 406, L. 520, Tab. 5)
- L. 638 – 651: Repetitive, skip.
- L. 667: This is repetitive and can be skipped.
- L. 718 – 720: This is repetitive and can be skipped (L. 507, 654!).
- Section 5.3.4 (L. 749 – 784): Lengthy descriptions of land use effects related to land use categories with little relevance for this study. Without loss of information you can either skip or shorten to 2 – 3 sentences at maximum.
- L. 846 – 855: This explanation of an outlier in the data set is superfluous. You already presented in quite some detail the reasons for a first outlier (L. 834 – 845). This first case may be included as a proof of concept for what the data set allows for, the second does not add any general insight. The general scientific audience is not interested in all details that may be of relevance for local or regional water managers.

- L. 898 – 902, 915 – 923: The content of the two respective paragraphs is very redundant and can be significantly shortened without loss of information.
- L. 907 – 911: This reads like a political statement for an NZ-internal audience and has no link to the actual content of the manuscript.

Reviewer: "… and some conclusions are made without clear evidence."

My observation: Along the line mentioned in general terms I stumbled across two points I'd like to mention here:

i) On L. 87 – 88 you make a bolt statement about the NZ water quality monitoring program ("it has one of the longest comprehensive national water quality datasets in the world"). I suggest that you back this with some information that supports this statement.

ii) ii) Your conclusion regarding the possible effects of increasing water clarity (L. 581 – 595). This is an interesting point but you do not provide evidence for your statement that "when combined with increasing nutrients, warmer water, and lower flows, the perfect recipe for toxic algae blooms is created." (L. 581 – 583). You cite (McAllister, Wood & Hawes 2016) but these authors seem to contradict your statement by claiming: "While quantitative data on sedimentation rates in rivers is lacking at a national scale, increasing land use intensification and forestry are likely to result in increased sediment in rivers, which may be partly responsible for observed rise in Phormidium proliferations." (McAllister, Wood & Hawes 2016, p. 292). Please provide references that support your statement.

Editor comments:

Already in my initial comments I asked for scatter plots displaying the relationships between discharge and water quality parameters. You argued that this would be an overkill. As a consequence, the entire flow normalization (L. 137) that you do as the first step in the data processing is basically hidden from the reader. As a consequence, your entire section 4.3.1 reports data without showing actual data although you qualitatively describe concentration-discharge relationships. This is not satisfactory and could be easily alleviated. If you combine related water quality parameters (e.g., total P, DRP etc.) together in one plot, you can display these concentration-discharge relationships with 4 to 5 matrices of 9x9 plots for all 77 sites. You have already now plot matrices of 16 x 16 in the Supplementary material.

Therefore, providing scatterplots of the concentration-discharge relationships with the LOESS functions is doable and will provide the reader with essential access to real data. This will improve the quality of the manuscript substantially and will make the entire process of data processing much more transparent. I strongly suggest that you add this information.

Because your focus is on the effects of intensity change you might consider to motivate the issue by actually including a figure in the main text that illustrates that land use did hardly change between 1990 and 2012 but that intensity did (based on Tab. 2 & 3 in the Supplementary Material).

Overall recommendation:

I recommend that you revise the manuscript based on your response to the reviews and that you pay due attention to the critical points that the referees and myself have raised.

Sincerely

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References:

McAllister, T.G., Wood, S.A. & Hawes, I. (2016) The rise of toxic benthic Phormidium proliferations: A review of their taxonomy, distribution, toxin content and factors regulating prevalence and increased severity. *Harmful Algae,* **55,** 282–294.

Moosmann, L., Müller, B., Gächter, R., Butscher, E., Herzog, P. & Wüest, A. (2005) Trend-oriented sampling strategy and estimation of soluble reactive phosphorus loads in streams. *Water Resources Research,* **41,** W01020, doi:01010.01029/02004WR003539.

Stamm, C., Jarvie, H.P. & Scott, T. (2014) What's more important for managing phosphorus: Loads, concentrations or both? *Environmental Science & Technology,* **48,** 23–24.