

Interactive comment on “River water quality changes in New Zealand over 26 years (1989–2014): Response to land use and land disturbance” by Jason P. Julian et al.

Anonymous Referee #2

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General comment

The manuscript “River water quality changes in New Zealand over 26 years (1989–2014): Response to land use and land disturbance” aims at investigating the relationships between land use and water quality, means suspended sediment, nitrogen and phosphorus concentration. A huge database of monthly data from 26 years and 77 watersheds has been analyzed. The main concern I have with the manuscript is that based on relatively simple statistical analysis mostly only weak correlation could be achieved between land use and selected water quality variables. Nearly all presented Spearman rank correlation coefficients were relatively low (approximately below 0.7 or even much lower). Choosing this correlation coefficient means that outcomes are de-

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scriptive and no quantitative functional dependences can be derived. 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. In general the manuscript is highly descriptive, clear hypotheses are missing, a sound reasoning for the approach based on former studies is not given, and some conclusions are made without clear evidence. Therefore it is not clear what is new and what has already been investigated in former studies. Furthermore the manuscript is very long (41 pages text only) and not very specific including repetitions. Therefore I reject the manuscript and suggest a submission to a journal with a more narrow scope.

Specific comments

Abstract: Not very specific, no specific numerical results are given on land use impacts on SSC and nutrient concentrations

Introduction: The first section is too general and not enough focused on the study presented. Review of international literature on dependencies between land use and water quality is weak. In Europe and the US much effort has been undertaken e.g. in connection with the SPARROW model (regression based water quality model) or time series analyses on land use induced N and C losses (e.g. in the UK; Worrall et al.). A focus could be given on grassland impacts. Some former studies from New Zealand are mentioned, e.g. time series analysis of nearly the same data set (see References), but the manuscript could benefit much more from these studies if they would be used to clearly identify the present knowledge and use them to define research questions which are still open. The objectives are not quite clear. The study wants to illustrate long-term relationships among land management, geomorphic processes and river water quality. It is not clear why temporal changes in land use should be compared to temporal changes in water quality variables (line 92) when the investigation later on shows that land use changes are usually minor (line 313) or mostly negligible.

Line 281: These are very general and obvious findings, e.g. that discharge increases with catchment area, I would take this out

Line 289-312: Land use distribution and patterns should be regarded as site description and not presented as part of the results (the objective of the study is to investigate relationships between Water quality and land use)

Line 313: The same is true for the land use change description, especially because land use change was mostly negligible.

Line 324: livestock densities expressed as SU?

Line 349: Relationships between disturbance (by the way a clear definition of disturbances would be helpful) and catchment characteristics should be restricted to those which are a) significant and pronounced and b) meaningful, for example for me it is unclear why disturbances has been related to mean annual sunshine duration, what does a rs of -0.25 tell us?

Line 368ff: Very general outcomes are presented which we would expect in any catchment, e.g. that suspended sediment concentration decrease with flow, furthermore most of the findings have already been reported elsewhere (20 years ago, see references of the manuscript) for the study region. Because of the suggested statistical analysis no detailed functional quantitative relationships can be defined.

Line 404: This section is very descriptive and simply repeats what is shown in Table 5 and table 6. A restriction on significant trends would be useful.

Line 439: The statement that total nitrogen was high if the concentrations are above 0.25 mgN/l is at least questionable. I would asses these levels still as pristine. The same is true for the assessment of TP with concentrations of 0.03 mg/l and DRP of 0.009 mg/l assumed as high levels. In Europe the eutrophication level for DRP is 0.05 for streams. Please revise.

Line 459: It is boring to be informed again that discharge increases with catchment

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area. Please take this out, a number in a table should be sufficient

Line 469: This correlation is quite small and should not be over interpreted, is there a table available on these correlations?

Line 491ff: In the result section wordings like surprisingly , interestingly should be omitted, especially if an additional discussion section follows. By the way why is it interesting that TP is increasing only in two catchments?

Line 542: What does this mean, only single values or 90% percentiles?

Line 545: Please clearly state in the introduction which work has already been done regarding to state and trend analysis (e.g. Ballantine and Davies-Colley Water quality trends in New Zealand rivers: 1989-2009 Environmental Monitoring and Assessment 186, 3, 1939-1950) Line 617: What does this tell us, recovery is quick because of high Pret? But high Pret should decrease the release of P from soil to soil pore water? Line 667: 0.45 is definitely not a high correlation, please correct Line 672: already discussed before Line 896: it is assumed that there is no doubt that fencing has improved the clarity of NZ rivers, but this is only a possible explanation. No evidence is given in the manuscript. In the presented study no specific quantitative analysis on fencing and sediment concentrations has been conducted.

Reference:

Ballantine DJ, and Davies-Colley, RJ, 2014, Water quality trends in New Zealand rivers: 1989-2009 Environmental Monitoring and Assessment 186, 3, 1939-1950 Worrall, F., H. Davies, T. Burt, N. J. K. Howden, M. J. Whelan, A. Bhogal, and A. Lilly. 2012. The flux of dissolved nitrogen from the UK - Evaluating the role of soils and land use. Science of the Total Environment 434:90-100.

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