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Discussions

Interactive comment on "Impacts of drought on the quality of surface water of the basin" *by* B. B. Huang et al.

Anonymous Referee #1

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This manuscript requires more extensive editing of English usage and grammar than can reasonably be expected of peer reviewers. Nearly every sentence contains a grammatical error or unclear wording. As a result, I do not feel I can provide a complete technical review. I recommend that the authors obtain professional English copyediting services and then resubmit if they choose. However, I was able to identify some serious technical issues with the manuscript that make it unsuitable for publication in HESS, irrespective of the English usage and grammar problems. I have detailed those below. 1. The introduction is nearly as long as the rest of the paper. This is an inappropriate balance; the greatest emphasis should be on the methods, results, discussion, and conclusions. The introduction is currently too long and provides too much basic information on water quality. It needs to be shortened, and it needs to be much more

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focused on previous research that provides the motivation for the specific work done by the authors. Also, many more citations need to be given to support general statements made about drought and water quality. 2. The methods are lacking in sufficient detail. How was the soil collected? From where and when? What were the climatic conditions at the time of collection? Had fertilizer recently been applied? How much soil was collected? Was it held intact? How was the leakage water collected? For how long was it collected after rainfall simulations stopped? How were the leakage samples processed and analyzed for nutrients? What was the chemical composition of the simulated rainfall water? What was the temperature of the soil and the rainfall water and were they held constant? What statistical methods were used to analyze the data (see related comment below)? How were the soil moisture, rainfall duration, and rainfall rate levels selected – were they based on real-world conditions typical of the study region? (If so, provide some documentation of this.) 3. Many statements in the results and discussion section are unsupported by either the study data or citations. Some examples (this list is not exhaustive): a. "With the aggravation of the drought degree, there will be a rise in the soil temperature. . ." (I did not see that they measured soil temperature, and no citation is provided). b. "To the soil of lower drought degree, its infiltration capacity is strong; therefore a large amount of nitrate infiltrates into the deep subsoil" (I did not see that they measure nitrate at different soil depths, and no citation is provided). c. "The soil of severe drought degree will become hardened and impervious" (I did not see that they measure soil impaction, quantitatively or qualitatively, and no citation is provided.) d. "The heavy runoff during the rainfall results in a mass loss of nitrate. . ." (I could not tell whether they measured surface runoff or subsurface drainage during the rainfall simulations, so am not certain if the remark about heavy runoff is speculation or actual observation. No citation is provided either.) e. "Raindrops disperse the soil particles and affect soil infiltration and the leaching of nitrate" (Again, I could not tell whether thy measured soil moisture/infiltration or made direct observations of surface particle disturbance. Again, no citation is provided.) f. "Due to excessive use of chemical pesticides and fertilizers, a great amount of nitrogenous fertilizer remains

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in the soil" (First, pesticides do not contain nitrogen. Second, there is no discussion of previous fertilizer use where the soil samples were collected and no apparent analysis of the nitrogen content of the soil used in the experiments. No citation is provided that would document previous accounts of this.) g. The paragraph running from the bottom of P 14474 to the top of 14475 – no citations are provided for the theory here, and no experimental evidence is provided to link the study results to the theory. 4. The factorial design of the study lends itself to statistical evaluation. However, there is no indication that the authors did any kind of statistical evaluation of the results. Rather, it sounds as though all of their conclusions are based on qualitative evaluation of the data. A statistical analysis of the results needs to be included. And the use of "significant" or "correlated" to describe study findings should be avoided when those findings are not supported by a statistical test. 5. Section 4 "Assessment of the impact of basin scale" should be excluded in its entirety. I assume the intent is to verify the small-scale experimental results with watershed-scale observational results. But there are several critical problems with the approach: a. The authors do not make a clear enough link between "drought conditions" in the two situations. Most notably, the correspondence between soil moisture in the experiments and rain-free periods in the watersheds is completely undocumented, although the authors use qualifiers like "mild" and "severe" drought for both. Also, there is no consideration given to the duration or severity of the rainfall in the watersheds, though that information may be available. b. There are many other factors that may be affecting concentrations at the watershed scale besides soil moisture and rainfall. No consideration is given to these possible confounding factors (timing of fertilizer application or more rapid in-stream biological uptake, etc). c. The conclusions on p 14477 (beginning with "In light of the case analysis of Nenjiang river basin) are unsupported by the analyses. The observations are all from the first rain event after a dry period, so no conclusion about point or nonpoint source predominance during the dry periods (eg before sample collection) can be made directly. Also, the only factor they looked at was the first rainfall after the dry period, so they cannot rule any other factors out. As such, it cannot be concluded that first rainfall after a drought is *the

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key* factor giving rise to changes in water quality. 6. Similarly, many of the overall conclusions in section 5 are unsupported by the analyses. a. "Climate change and human activities are at present the main causes that give rise to the deterioration of water environment in the basin area during dry period" is far too broad of a conclusion to draw from the study results. b. ". . .the key factors that determine water quality are a decrease in river discharge, a rise in temperature, and runoff scouring. . ." The authors did not look at any of these factors. c. "A rise in temperature will enhance the self-purification capacity of BOD5 in water, but there is no significant change in BOD5 density due to the reduction in discharge during the drought." Again, the authors did not present temperature results, and they did not present watershed results during the dry periods, so cannot make statements about changes in river discharge (or any other change) during the dry period. They can only make observations about what happened during the first rainfall after a dry period. d. The first paragraph on p 14478 presents the only conclusions supported by their study. This paragraph is currently difficult to read, so I can't fully evaluate the statements. But at least they are directly related to the experiment conducted in this study. 7. The title needs to provide a location. 8. Please clarify what is being shown on the y-axis of figures 2 and 3. "Change" is ambiguous.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 14463, 2013.