Second review :

This is an improvement over the original submission, but there are still problems that need to be dealt with. The general issue is that the manuscript still claims a level of originality and significance that is not justified by the evidence that is presented. I assume that the authors are not intentionally shading the facts, but unfortunately this is the impression that the manuscript gives.

For example, the introduction is still written as if nothing like the authors' River Lab has ever been built before. But various "field lab" setups have been built, many times. Indeed, there was a paper published in HESS earlier this year describing a field lab based on ion chromatography that is very similar to the River Lab. The authors are well aware of this prior work (and indeed it was pointed out in a previous review), but they still refuse to acknowledge it. Instead, for example, they only cite the HESS paper following a statement about "issues related to sample transport, filtration and storage". Refusing to acknowledge prior work and instead citing it for trivial or tangential points is inappropriate and one would hope that the authors would recognize this.

The manuscript says that "online instrumental devices in which continuously pumped water is injected have been suggested as an alternative to monitor water chemistry." Such systems have not only been "suggested", they have actually been built and used, in some cases for many years.

The manuscript continues, "To date, these systems have only been used to monitor nutrients such as dissolved N or P." This is false and the authors know that it is false. They know perfectly well that another group has already published an ion chromatography system very similar to theirs and that measures the same ions that they have measured, but their manuscript appears to have been carefully written to conceal that fact.

Likewise the authors have not acknowledged the major recent overview of high-frequency sampling applications (Rode et al., 2016), even after this was pointed out to them in the previous round of review.

The slanted presentation extends to technical matters as well. The manuscript simulates the addition of 2% and 4% noise, saying that these are "representative of the relative analytical precision reported for most laboratory IC devices (Neal et al. 2011; Aubert et al., 2013a)." The implication is that the data from Neal et al. or Aubert et al. would look as messy as the simulations presented here, but this is a gross distortion. For example, the precisions of the IC measurements of SO4 in Neal et al. are about 2%, but at concentrations of only 2 ppm, rather than the roughly 60 ppm presented here. Because IC noise percentages are typically higher at lower concentrations, the Neal et al. measurements would likely not be much worse (and possibly better) than the RL measurements at comparable concentrations.

The data from the River Lab look very nice, and it is neither necessary nor appropriate to try to make them look better by misrepresenting the accomplishments of others.

The comparison of the River Lab data and the IGPG lab data depends critically on the calibrations that are used for the two instruments. Here the manuscript directly contradicts itself. On line 268 the reader is told, "The calibration procedure in both laboratory and RL is the same using the same set of calibration solutions." But just one page later, in explaining the different results obtained from the two instruments, the manuscript says, " In addition, the most accurate measurements were obtained with the RL rather than with the laboratory equipment because (1) the calibration curve of

the RL was made from a series of solutions (dilutions of the "River x1" solution) having the same element ratios as the solution used for the accuracy test (the "River x1" solution) ... with our in-lab IC instruments ...we used a series of calibration solutions having the same concentration for all elements..." This is a rather obvious discrepancy and it is surprising that apparently none of the authors have noticed it, even after the issue of calibrations was raised in the previous review.

There are language problems here that should not be present in submission to a major international journal (particularly after revision). The SI is particularly bad; after finding 14 language errors in just two pages, I stopped counting. There are 10 authors on this paper, some of whom are really good at scientific English, and if they have really all read and approved the manuscript it is hard to understand how so many errors could still persist. Apparently nobody has even run a spell-checker; otherwise bloopers like "ratther" would have been caught.