Dear Editor,

We would like to thank the two referees for their comments and useful advice on our manuscript. We appreciate the rather positive feedbacks provided by both reviews.

In the following, we will answer to all the reviewer's comments and suggest modifications that address reviewer's remarks and hence should improve the manuscript. We think that most of the reviewer's concerns are relevant and that addressing them will result in a significant improvement of the manuscript. Regarding some of the other concerns, we provide below the reasons why we do not agree.

Reviewer 1: General comments

This proof-of-concept paper presents a new system of water chemistry monitoring (that the authors have termed 'River Lab') that offers both higher frequency monitoring and higher analytical precision than existing approaches. The authors describe this as a technical breakthrough that opens a new era of investigation into the hydrochemical signal of rivers. I agree that this is an exciting development and that we do need, generally in earth sciences, higher temporal and spatial monitoring for new catchment functioning insights (something that the Plynlimon dataset, that the authors refer to, showed superbly well). So I think this River Lab system is heading us in the right direction.

First, I think the title is rather good! I must admit to not knowing, when I first read this, that potamology is the study of rivers, but now that I do, I have a) learnt something, and b) been impressed with a cool title. The paper in general was really nice to read. It is well-written, clear and accessible throughout. The introduction was especially well- written and had a good progression through explaining to the reader what the norm is in stream chemistry monitoring, and why it is important to do better. It is also well- referenced. The figures are largely of good quality (although with a couple of minor exceptions noted below).

Overall, I think it is a good paper. I have only one 'major' comment: Regarding the offsets between the two sets of data (RL and Lab) shown in Figure 4, they seem rather large. You should further discuss the implications of this. They are systematic for most of the species. As you state on line 261 and throughout the paper, the RL data is more precise than the lab data (this is good, and you demonstrate this with data), but are the RL data more accurate than the lab data or vice versa? How could you determine this? You don't discuss accuracy at all. What is the use of a precise instrument if its accuracy is not so good! How does the existence of these offsets propagate into data analysis and process understanding? Surely we should care about the absolute magnitudes as well as the variations in amplitude. Or are the variations in amplitude most important?

Authors: Regarding the comparison between the River Lab (RL) and in-lab measurements, we emphasize that the accuracy of the RL is demonstrated (Section 4.1 of our manuscript) through the measurements of the synthetic river water samples "River x1". The corollary (given that the difference between the two sets of measurements is systematic and beyond analytical precision) is that the in-lab measurements are - marginally, the 1-to-3% observed difference being fairly small compared to precision reported elsewhere for similar measurements - inaccurate.

Such accuracy with the RL was most likely achieved 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); (2) the RL is continuously processing solutions with a similar matrix, thereby minimizing memory effects and cross-contamination that can compromise measurements if widely differing samples are run successively on the same instrument. These two conditions were not met with our in-lab IC instruments, where we used a series of calibration solutions having the same concentration for all elements, and for which the measurement sessions took place between other sessions with very different samples. This is likely to lead to inaccuracies. Of course, we think that the accuracy of in-lab measurements could be improved by matching the matrix of the standards and samples, and/or by temporarily dedicating an instrument to one given type of sample (e.g. all samples from the same river). However, such conditions are not reflective of the "classical use" of an IC instrument in a lab, and we thus believe that our comparison is fair.

In the revised version of the manuscript, we will state more clearly that the RL provides the set of accurate measurements, and that our comparison is rather a check that the observed *relative* variations over a day are repeatable with another instrument.

Reviewer 1:I have quite a few, more minor comments, and these are listed below.

Minor comments

Line 24 Extra "a" in the sentence

Authors: This will be corrected in the final manuscript

Reviewer 1: Line 32 Is drought actually 'boring', hydrologically-speaking? I don't think so! Maybe rephrase to 'low flows' or something similar.

Authors: We used this term (importantly, with quotation marks) to reflect the fact that the variations in concentration were unexpected, but we recognize now that this is a bit blunt. This will be changed.

Reviewer 1: Abstract Combine into one paragraph

Authors: This will be corrected in the manuscript

Reviewer 1: Line 48 Need to include the page number in Kirchner et al 2004, where that quote was taken from

Authors: We will include these page numbers in the manuscript.

Reviewer 1: Line 41-42 Your references need to be in some order... ie alphabetical or chronological. . . (this comment applies throughout the manuscript)

Authors: We will fix this issue when revising the manuscript.

Reviewer 1: Line 53 "such as a single storm events" singular or plural?

Authors: We will change this sentence to singular: "such as a single storm event".

Reviewer 1: Line 60-63 Would be great to read here example(s) of discoveries that Neal et al (2012)'s monitoring made. . . what didn't we know before that this study demonstrated was possible to know with high frequency chemistry monitoring?

Authors: We propose to add the following sentence:" The high sampling frequency on a long time scale provides new insights into hydrogeochemical functioning and a novel resource for catchment modelling. "

Reviewer 1: Line 62-63 Why are "act of discovery" and "manual approaches" in quotation marks? Are they quotes from Neal et al (2012)? If so, say so. Otherwise, they don't seem to need quotation marks. . . (ie. not jargon)

Authors: These are quotes from Neal et al. (2012). We will add this in the text.

Reviewer 1: Line 76 Does "Temporal" need to be capitalised?

Authors: No, we will correct this in the text.

Reviewer 1: Line 77 Is there a website / papers you could refer to on the CRITEX program?

Authors: We will add the link in the manuscript: https://www.critex.fr.

Reviewer 1: Line 85 "Unsuspected" seems strange. Perhaps "unexpected"?

Authors: This will be change to "unexpected".

Reviewer 1: Line 98 I guess the 45 km2 refers to the Avenelles sub-catchment, rather than the Orgeval watershed, but this is ambiguous from the sentence structure.

Authors: Yes, it refers to the Avenelles sub-catchment. We will change the sentence to be clearer.

Reviewer 1: Line 99 Should be "on average"

Authors: This will be corrected.

Reviewer 1: Line 105 Are these spring flash flood events from snowmelt? It would be good to see a figure of streamflow and precipitation inputs for a typical year or years.

Authors: No, these are flash rain events. As we want to keep the article short and focused on methodological aspects rather than on the field site, we will simply provide a reference linked to an article with all information about the hydrology of the river.

Reviewer 1: Line 112 Give full names of IPGP and IRSTEA

Authors: IPGP is "Institut de Physique du Globe de Paris" and IRSTEA is "Institut national de Recherche en Science et Technologie pour l'Environnement et l'Agriculture". We will add these explanations during revision.

Review 1: Line 139 "the chosen analysis time is 30 minutes". Why is this the 'chosen' frequency? Is this the fastest the cationic and anionic measurements can be made? From my understanding of the paragraph starting at line 115, the probes in the primary circuit could operate on a 1-2 minute basis. So is 30 (or 40) minutes or the complete analysis (including anionic and cationic) an arbitrary decision or is this the highest frequency this set-up can manage? Would sampling at a higher frequency be desirable? Please elaborate on this.

Authors: In our prototype, the sampling frequency is limited by technical issues. This will be explained by adding the following sentence: "However, the performed tests show

that the frequency for a complete analyse of cation and anion is actually limited by the filtration device" (particularly by the turnover of the water inside). (Line 241- 243). Therefore, this highest frequency is not an arbitrary decision but the highest frequency reachable with our prototype.

Reviewer 1: Line 143 Is there a word missing here?

Authors: Yes, "water". We will correct this in the text.

Reviewer 1: Section 3 I would like to see more comparison with normal laboratory design and protocols in Section 3. Eg. is the calibration and cleaning frequency in keeping with lab practices?

Authors: This information (working conditions, quality control, instruments models, columns…) is already in the supplementary information. We will move it to the main text.

Reviewer 1: Line 155-162 Very cool!

Line 190 Perhaps better to say "<" rather than "better than", or give a range of %

Authors: Yes, this wording is awkward, and will be corrected accordingly.

Reviewer 1: Line 191 Earlier you refer to Table 1 as "Table 1" and now you refer to it as "Tab. 1". Be consistent.

Authors: This will be corrected in the final manuscript.

Reviewer 1: Line 201 "Fig S1 2". I can only see a Fig SI 1 in the supplementary document. Have you mis-referenced this?

Authors: Yes, we will correct this in the text.

Reviewer 1: Section 4.3 Could you apply autocorrelation analysis to your long time series (from the field) to check these cross-contamination errors under low vs high flow conditions?

Authors: It is a relevant remark. It is one of our ways of research ways planned to perform such analyses for future papers.

Reviewer 1: Line 234 "In the case". . . change to "in this case".

Authors: We will correct this in the text.

Reviewer 1: Line 249 Why did you only pick two dates at low summer flow? Why not for a high flow day? It would be good to see such a test at high flow periods too. Would we expect to see the same reproducibility for high flow?

Authors: We thought that the summer drought would be an interesting period to perform these tests in an "extreme" case (lowest variations observed) and to let the reader appreciate the performance of the prototype. We also performed this test during a flood event but we do not present the results here because analytical conditions at that time were not perfectly controlled, such that we do not deem the results to be publishable. However, first-order results from the test on the flood event are consistent with summer tests.

Reviewer 1: Figure 4 Font size and type are not consistent between the different sub-plots. Correct this.

Authors: We will correct this in the text (this comment echoes a comment of reviewer 2).

Reviewer 1: Line 297 Would the temperature of a laboratory not have similarly well-maintained temperature? In my (admittedly, somewhat limited direct) experience, they normally do.

Authors: The room temperature in our laboratory (and we believe, in most laboratories) is not as carefully checked as in the RL prototype (24±2°C). However, both instruments are the same and have a thermostat.

Reviewer 1: Line 329 I think you can remove the parantheses here "(an apparently 'boring' hydro- logical period)"

Authors: We will correct this in the text. "Boring" will be rephrased too, according to another comment of the reviewer.

Reviewer 1: Line 330-331 Did you actually sample all species, so that you have the equivalent data in Fig5+6 for all species (not just calcium and sulphate)? Do the fluctuations in those species show the same relationship with discharge? I would suggest including those data in the supplementary information. Using the average, Std D, skewness and kurtosis is great as a comparison tool, but how about some metric for how well the different sampling frequencies reveal the fluctuations in relationship to the streamflow hydrograph? How did you decide that average, Std D, skewness and kurtosis are the best comparison tools?

Authors: Yes, in principle we could add the other elements' variation as supplementary information but actually this is the purpose of a future article entirely devoted to summer daily variations.

Our approach to describe quantitatively the impact of the sampling frequency and precision on the signal relies on characterizing the concentration PDFs. The first four statistical moments (average, Std D, skewness and kurtosis) are classically used to characterize PDFs in a number of fields. As for using other approaches than simply the PDF (relationship with discharge, or time-series analysis), this will be the scope of future papers, and we did not want to expand too much on these approaches here.

Reviewer 1: Figure 6 Include a hydrogaph on here, like you did with Figure 5. Does it have the same diurnal variation as the sulphate shows?

Authors: For technical reasons, for the moment the discharge signal we have at hand for the summer drought is in the limit of quantification, hence not publishable, and requires further refinement (importantly, the discharge relative variations are lower than a few %). We do not think adding this discharge record is essential to improve the paper.

Reviewer 1: Line 348-349 revealing a diurnal structure [in sulphate]? Did the other species also exhibit the same diurnal structure? What do you mean by "specific to each element"?

Authors: We propose to add the following sentence to be clearer: "Each element exhibits its own type of daily variation in terms of amplitude and regularity"

Reviewer 1: Figure 7 This is a nice figure with LOTS of information contained within it. It's quite hard to imagine how these time series might look (the equivalent of Fig5+6). So again, this would support including the other species information in the supplementary information.

Authors: We agree with this comment, which meets concerns raised by the reviewer 2 (see below). We think that the calculations performed and generalized on all elements measured are meaningful to the reader however we think that having the other detailed time series would simply be too much for such a paper. Our goal with figures 7 and 10 was exactly to summarize the information that would be contained in these numerous time-series plots, to allow the reader to understand this at a glance. In addition, presenting an exhaustive report of all "real" time series (original signals) will be the subject of future papers.

Reviewer 1: Line 397-398 "artificially degraded the signals by adding a normally distributed noise" Nice idea!

Figure 8 Could you make these into line plots (i.e. connect your dots) so that we can see the noise chronologically? And also include the vertical lines (like in Figure 5) from the peak discharge down through the plots.

Authors: We tried to add the vertical lines as eye guides but found that connecting all dots makes the figure unreadable.

Reviewer 1: Figure 9 Again, make these into line plots? Also add a hydrograph.

Authors: We found that connecting all dots makes the figure unreadable.

Reviewer 1: Line 474 Is this a paraphrase or a direct quote?

Authors: A direct quote. We will add the reference to Kirchner et al. 2004 with the corresponding line number.

Reviewer 1: Line 475-477 Nice punchline. But (and I apologize for being really pedantic here), the quote is to 'hear' the notes, not to play them. The stream is playing the notes. Your RL is therefore. . . a really sophisticated hearing aid. . .???! (Maybe a better simile needed).

Authors: This comment is also formulated by the reviewer 2, we propose to change the sentence word "play" by "hear" and the sentence: "The improvements made possible by the RL allows us to hear almost each note of the potamological symphony."

Reviewer 2: General comments

Reviewer 2: This manuscript presents first data from a field deployment of an instrument package ("River Lab" or "RL") for high-frequency analysis of natural streamwaters. The instrumentation includes dual-channel ion chromatography and various physical and electrochemical probes. Data are presented to illustrate the quality of the chemical time series that can be obtained.

This manuscript makes a useful contribution to the growing literature on field-based chemical analyses of natural waters. In particular, the data presented here show that on-line analyses can be much more precise than those based on conventional sampling with later analysis in the lab.

However, the manuscript's characterization of the River Lab as a "breakthrough" (three times!) is not appropriate. Many other studies have also deployed wet chemistry instruments in the field (see Rode et al. ES&T, 50, 10297, 2016, and similar reviews). For example, a Swiss research group has recently published (also in HESS) a field deployment of another on-line ion chromatography system. That system arguably goes beyond the one presented here (it also includes isotopic analysis, and analyzes both rainfall and stream water, rather than just stream water alone), but the Swiss group does not use self-congratulatory terms like "breakthrough" to describe their work.

Authors: We are of course aware of the studies conducted by the Swiss group. We found it very interesting. We have already mentioned in our manuscript. However one could state that our manuscript also "arguably goes beyond the" paper by the Swiss research group in terms of analytical performances and their interest for hydrochemistry.

Reviewer 2: Likewise the claim that "... the high frequency and high precision of the RL enabled unprecedented observations of the fine structure in hydrochemical time series" is exaggerated. Similarly detailed results have been obtained before for other analytes using other field instrumentation, as well as by the Swiss group for major ions using IC. The present manuscript does a good job of demonstrating the precision of the RL, and does so in greater detail than I have seen before. But the observations themselves (at least the ones that are actually shown here) do not merit superlative terms like "unprecedented".

Authors: We appreciate the general comments of reviewer 2. We agree that the vocabulary is somewhat exaggerated and we will tone it down. However we notice that the word "breakthrough" that reviewer 2 does not like is used by reviewer 1 in his/her review.

Reviewer 2: Heroic claims like "Our study opens a new era of investigation into the fine structure of the hydrochemical signal in rivers" (line 478) will not create a favorable impression in the community of investigators who have already been working, in some cases for decades, on these questions. Similarly, claims like "Recording such fine stream hydrochemical variations is thus offering a new perspective on Critical Zone" (line 483) are unproven and should be removed, since no inferences about any catchment processes are derived in the present manuscript.

Authors: We partly agree with the reviewer that the words need more attention but we really think that our study is providing new information on the fine structure of river chemistry over such a long period of time. It was not our intention to make the reader feels that nothing was done before our study. We will remove the most "heroic claims"

Reviewer 2: Several time series are shown to illustrate the results that can be obtained with the RL, and to illustrate the results that would be expected at lower sampling frequencies. This is fine as far as it goes, but given that the manuscript claims that the system has been measuring seven ions continuously for over a year, it seems strange that only a few days of data, for only two ions, are shown. Where is the rest of the data set, for the rest of the ions? It is understandable that the authors want to defer the analysis of the longer-term data set for a later paper, but they should at least demonstrate its existence by showing it to the audience.

Authors: We understand the reviewer's comment but we would like to emphasize that in this article we first want to present the "proof of concept". This paper is thus not aimed at presenting the whole data acquired in one year. As correctly guessed by the reviewer, we "want to defer the analysis of the longer-term data set for a later paper". We only

selected carefully two characteristic periods to illustrate the importance of recording river chemistry at high frequency and with a good precision. Presenting results over longer periods may dilute our conclusions. As stated in the introduction (and well understood by the reviewer 1) our paper aims at presenting the feasibility of measuring river chemistry by transporting the lab instruments to the field, not "giving away" a full year of measurements.

Reviewer 2: To quantify the effects of subsampling and sampling error, the manuscript calculates how they affect the first four moments (mean, standard deviation, skewness, and kurtosis) of the distribution of measured concentrations for two brief sampling periods. Despite the time and space devoted to this analysis (two entire figures and parts of four others), it is not well posed and adds little to the paper, for the following reasons:

1) The moments of concentration distributions are rarely subjects of interest, particularly over such short periods of time.

Authors: This is a totally unsupported statement that would deserve to be developed to warrant a proper answer. But we emphasize that we use these moments as a comparison tool between signals (original and altered) covering the *same periods of time*. In addition, we are doing this not in the scope of retrieving information on actual processes (which is probably what the reviewer has in mind when he/she says "subjects of interest"), but just in a sort of sensitivity analysis.

Reviewer 2: 2) The distributions (particularly in Fig. 5) are sensitive to the interval of time that is considered. In particular, much comment is made about the "bimodal" distribution during the "flood event", but this is largely an artefact of the specific time interval that is chosen.

Authors: We totally agree. However we carefully chose the interval between the very start and the very end of the flood event in order to not "under sample" this food event. By choosing a larger interval we would have altered the distribution by adding values from the non-flood period. Actually, reading our manuscript again we do not find that "much comment" is made about this bimodal distribution. We also note that many of the flood events we observed since the RL has been started show several peaks, such that choosing another flood event would not result in a different interpretation.

Reviewer 2: 3) Skewness and kurtosis are mostly useful in characterizing uni-modal distributions, and their application to bimodal distributions is not very helpful.

Authors: It all depends on what the reviewer means by "useful". It is true that for multi-modal distributions, the interpretation of skewness and kurtosis is not straightforward, but it is so for average and standard deviation as well... Therefore, we recognize that the chosen statistical moments are not *perfect* metrics of the shape of such multi-modal distributions, but (1) they have the advantage of being estimable empirically, and (2) importantly they are used here only as a tool of *comparison* between different PDFs.

Reviewer 2: 4) Statistical moments calculated from only five data points (as in Fig. 5) really should not be taken seriously!

Authors: We agree with the reviewer, this is slightly misleading (in the sense that with such low sample numbers, the empirical estimators of the statistical moments have no

chance to be close to their actual values), and we therefore removed the estimated statistical moments from the strongly sub-sampled signals (1/day) in Figs 5 and 7.

Reviewer 2: 5) The error bars in Figures 7 and 10 are unrealistic estimates of the uncertainties in the data points, because they do not account for the rather strong autocorrelation in the time series.

Authors: We agree, and we propose to remove these error bars, and to replace them by a "cloud of points" corresponding to each realization of the sub-sampled (Fig. 7) / degraded (Fig. 10) signals. This would be more reflective of what we have actually done.

Reviewer 2: 6) Skewness and kurtosis are not ratio-scale variables (they do not have a natural zero), so calculating percentage changes in them makes no mathematical sense, for the same reason that a temperature increase from 1 to 21 degrees Celsius is not "an increase of 2,000 percent!"

Authors: We understand the reviewer concern: unlike average (at least average of ratio scale variables, such as chemical concentrations) and standard deviation, skewness and kurtosis are not ratio scale variables. However, we note that the "0" of the reduced centred skewness scale is not arbitrary. But indeed the simple fact it can take negative values shows that it is not a ratio scale variable. Obviously, a possibility would have been to use the absolute value of skewness, but this would have been too difficult to follow. We therefore suggest that in the revised figures we will present the absolute values of these metrics in Figs. 7 and 10.

Reviewer 2: 7) The changes in distributions and statistical moments shown in Figures 8-10 are unsurprising to anyone with even a modest background in statistics, given that we are mixing the distributions of the original data with an assumed error distribution that has a mean, skewness, and kurtosis of zero, plus a standard deviation that is substantial compared with that of the original data.

Authors: We are not sure to understand the reviewer's point. Results presented in Figs. 8-10 show that the mean is not affected by adding this normal noise, but all three other moments are. We believe that this will not be easily predicted by readers having "a modest background in statistics". And let alone quantitatively predicted - which is another interest of this approach: actual numbers are given. For example, our initial guess would have been, if anything, that only the standard deviation would be affected, since the standard deviation of the added signal is its only non-zero statistical moment.

Reviewer 2: 8) Statements like "the average is not sensitive to analytical precision" (line 429) are self-evident (of course it isn't, as long as the added noise has a mean of zero!).

Authors: This is true and our intention was not to present this as a surprise. We will rephrase the sentence about the average.

Reviewer 2: The time series plots show the effects of subsampling and added noise very clearly. The statistical analysis does not help (and is often misleading), and should be omitted.

Authors: As explained above, we do not agree with the reviewer about the interest of the statistical analysis (which allows us to build Figs. 7 and 10, and therefore to generalize the approach to all elements and report the results in little space, which would be impossible using the time series only). Therefore we will leave this statistical analysis.

Reviewer 2: The manuscript is mostly well written, but in some places the grammar and word us- age need improvement. To take just the two examples that arise in the title itself: "progresses" is not a noun in English, and "Potamochemical" is not a word in any language. While one can appreciate the authors' creativity here, the use of a made-up word like "Potamochemical" will seem pedantic to many readers. A few of us may still recall that "potamology" means "study of rivers", but the usage of this term has been declining steadily for about 50 years and there is no compelling reason to revive it. Readers should not need to reach for their dictionaries to look up obscure Greek roots of words, just to understand the title of a scientific paper.

Authors: We can understand the reviewer's feeling, which is exactly the opposite of that of reviewer 1. It is a personal point of view. On our side, we do not think that, because a term is declining, it should fade away. There is no intent in trying to look smart in this article, but we think that the almost forgotten term "potamology" should be revived so it could increase the visibility of our river and critical zone communities. We are not considering changing this word. We will change "progresses" to "progress".

Reviewer 2: Specific comments

The abstract claims that the RL was deployed "for over a year of continuous analyses" but the conclusion refers to measurements made "over several months". Which is it? Please provide plots showing "over a year of continuous analyses" of all ions, to substantiate the claim made in the abstract.

Authors: We refer the reviewer to its own sentence above: we "want to defer the analysis of the longer-term data set for a later paper" in preparation. This is absolutely unnecessary for this "proof-of-concept" paper.

Reviewer 2: The abstract says that the daily oscillations observed during the summer drought were "unexpected". Why were these oscillations unexpected, given that they have been reported in many previous papers, including several that are cited here?

Authors: We will remove the word "unexpected".

Reviewer 2: The characterization of sampling frequency in millihertz (i.e., 40-minute sampling is described as 0.42 mHz) is unhelpful and will strike many readers as pedantry. If one wants to speak in terms of frequency, a more natural time base is 1 day (40-minute sampling is 36/day, 7-hour sampling is 3.42/day) or 1 week (40-minute sampling is 252/week, 7-hour sampling is 24/week).

Authors: We propose to change in the manuscript the frequencies with "40-minutes", "7-hourly" and "1/day". We propose to remove all terms in "hertz" as we realize that is note clear for the readers.

Reviewer 2: The "flood event" represents an increase in flow of only about 50% or so, and even the highest flow in figure 5 (about 200 L/s) is 50 times smaller than the reported peak flow of over 10 m3/s (see line 105). In what sense is it appropriate to call this a "flood event"?

Authors: Indeed. To be fairer, we will change the term "flood event" to "rain event".

Reviewer 2: Figure 1 is artistic but less informative than it should be. Please provide a proper schematic of the instrumentation instead. Readers should be able to build a working version

of the instrumentation (or at least understand the challenges involved in doing so) from the information provided.

Authors: We think that this figure is a very good compromise between readability, such that the average reader will understand at a glance what the equipment is about, while the more interested reader will obtain first-order technical information from it. We will therefore keep this figure in the main paper. However, we will add in the next version a more technical sketch with all information needed in the supplementary information.

Reviewer 2: Figure 3 focuses the reader's attention on the conductivity data (which is not the subject here), because it is darker than the Cl data. Plot the conductivity data in a light color and the Cl data in a dark color, and connect the Cl points. And don't show the artificial 0-to-100 scale; show the real concentrations and conductivities.

Authors: Absolute values for each measurement are already given in the figure. We still think that a normalized scale help the reader to focus on the relative delay to appreciate the cross-contamination and not the absolute value.

Reviewer 2: Figure 4 and associated text: why are cations shown for one date and anions shown for another date? Show all the ions for one date, so they can be compared. Also, the fonts in Figure 4 are inconsistent, and the placement of the panels is erratic. Didn't anyone notice this? And the two colors look the same in grayscale; again, didn't anyone check? Also, please show the real hour of the real day, not an artificial scale (I went crazy trying to figure out what was going on here, before I discovered that the scale was fake and the cations and anions have nothing to do with one another because they are on different days (but why does chloride stop before the other anions?).

Authors: We apologize for these mistakes, and all of the requested format changes will be carried out. We will also state more explicitly that these analyses were carried out over different days for anions and cations (which was necessary for logistical reasons, but which does not impair our approach).

Reviewer 2: The laboratory analyses were reportedly done on "IC devices similar to those installed in the RL", but what does "similar to" mean? Specify exactly what instruments those were, what columns were used, how old they were, and so on. This is important, because the relatively poor performance of the laboratory analyses is attributed to the difference in sampling, rather than the difference in instrumentation. If the laboratory instruments are an earlier generation of IC, or are using older columns, or have not been maintained as well, or have not been operated as carefully, or, or, or... it is easy to see how the laboratory results could look poorer for many different reasons.

Authors: As already answered to the reviewer 1, such information is already given in the supplement. We will move this information to the main text.

Reviewer 2: The obvious offset between the laboratory results and those from the RL are particularly concerning. What efforts have been undertaken to verify that these do not indicate an artifact in either the RL or the laboratory?

Authors: We invite the reviewer 2 to read our reply formulated earlier to the first paragraph of the review 1.

Reviewer 2: In Figure 8, add error to the whole time series, not just the arbitrarily defined "event" period. Details (a partial list) 50: Rozemeijer, not Rozemeiler 51: Chapman et al is

1997, not 1996 (and it would be appropriate to cite Neal et al., Hydrological Processes 2531, 2013, here). 292: discretely, not discreetly many figures: probability, not "probality" (didn’t anybody notice this?) 476: "the best orchestra available": clever phrase, but an unproven assertion and will be perceived as inappropriate bragging 477: "to play the potamological symphony": no, instruments *record* the symphony; nature plays it.

Authors: We will add these new references and correct the typos. We also invite the reviewer 2 to see our reply to reviewer 1 regarding the mistake between "play" and hear".