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Discussions

Interactive comment on "Discharge measurement with salt dilution method in irrigation canals: direct sampling and geophysical controls" *by* C. Comina et al.

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Following the reviewer requests the paper has been extensively modified. We attach a marked reviewed paper in order to allow for a direct verification of the implemented comments. We hope to had satisfied most of his observations which allowed to improve, in our opinion, the overall quality of the paper. Detailed answers to reviewer observations are provided in the following:

Reviewer #1:

General Comments: This is a potentially interesting study but one that would greatly

benefit from more details and more work. The paper is logically and well written but there is a distinct lack of data presented in the text of the paper, so for example, I am not sure whether given our understanding on mixing lengths whether the salt dilution should have worked better than it did as it is not clear what the predicted mixing length was and how it related to the separation of measuring and sampling points. In a similar vein the abstract and text are almost devoid of details (distances, concentrations etc) and the discharge from the FERT measurements were never specified.

A: We do agree with the general comment of the reviewer that the paper we are presenting is only a starting point both for the FERT technique and for the multisampling apparatus. We have performed some modification within the text in order to state it clearly. We however think that, even if limited, the application we are presenting is potentially interesting and not so common, as recognized by the reviewer himself. We have added some more details in respect to the expected mixing length, which has been chosen on the basis of preliminary calculations, but also on the logistical availability of the reach (i.e. the canal section of the tests is one of the few available with the required length). Then even if a precise knowledge on the mixing length will be allowed possible flow variations and concentration distributions (like in our example in the left bank) are always to be accounted. Our point therefore is not strictly focalized on the discharge itself but also on the imaging of the salt plume. In this respect we clearly stated in the text that discharge measurement based on FERT are not provided since the methodology in his actual development is more focused on imaging and verification of the plume than on quantitative evaluation.

R: I have two major issues that are more important and which detract from the study.

a) There only seems to be one set of measurements (one canal on one day under one set of flow conditions). If a study such of this is to have impact there needs to be more – for example what would happen if you sited the measurement site at a different place, what happens at different flows? At the moment, there is no indication of whether this information is transferable.

A: We do agree with the reviewer that before an extension or a full application of the technique there will be necessary to have other results/field cases but we however think that the presented application we are showing could be a starting point for the development of the technique also for other authors. We have added some comments in this respect in the conclusions. As mentioned, the investigated canal presents also some complicated logistical conditions with lot of dead zones, curves and not easy to be accessed banks. Therefore the zone of the canal presented is one of the few available. Similar situations are found in a lot of field cases in which a discharge measurement is however to be obtained. Again the FERT imaging can be an aid in similar conditions to evaluate whether the tests performed could or not be reliable.

b) Both techniques provide estimates of discharge that may or may not be realistic. I am not sure why these experiments were not run in a setting where flow is already known (via gauges or V-notches); in which case we could assess not only the comparison between the techniques but how close each one gets to the correct value.

A: We have already applied the FERT technique under laboratory controlled condition (Sambuelli and Comina, 2010) even if for partially different scopes. Our aim in the present paper is to deal with field conditions. The salt injection method has been already proven to be quite reliable if all of the specifications in the tests performance are accomplished. Again the point is to deal with real field variability and logistical issues. Therefore we are not interested in a comparison of the methodologies under known and stable conditions, for which we are almost sure that there would be a good agreement among the tests results as given by the theoretical definition of the methods, but under real and variable test conditions.

R: The comments below are more minor.

Specific Comments

1. Abstract. While this is a good summary of what the study set out to achieve, you should also use this to explain the key results.

A: What we claim as our key result is clearly stated in the abstract: " Preliminary results of the single test site presented show that a correct visualization of the passage of the salt plume is possible by means of geophysical controls and that this can potentially help in the correct location of sampling points.".

2. P10037, lines 12-16. Are there any more useful details? For example can you specify what a peak to background ratio of EC should ideally be? Alternatively do we know how the relative errors scale to the peak:background ratio?

A: Some more details have been added to the text even if also to this point there is not uniformity of specifications: Kite (1993) suggests that peak EC should be 50% higher than background, while Hudson and Fraser (2002) suggest that peak EC should be at least 5 times higher than background. Moore (2005) proposes that increasing EC by 100–200% of background should be adequate for streams with low background EC (less than about 50 μS/cm), while Kite's (1993) guideline should be reasonable for streams with background EC greater than about 100 μS/cm.

Moore, R.D. 2004. Introduction to salt dilution gauging for streamflow measurement: Part I. Streamline Watershed Management Bulletin 7(4):20–23.

Kite, G. 1993. Computerized streamflow measurement using slug injection. Hydrological Processes 7:227–233.

Hudson, R. and J. Fraser. 2002. Alternative methods of flow rating in small coastal streams. B.C. Ministry of Forests, Vancouver Forest Region. Extension Note EN-014 Hydrology. 11 p.

3. P10037, line 24 onwards. The discussion of mixing lengths could do with a few more details. I expect that there have been some measurements of characteristic lengths for different stream geometries and flow rates, a few values would make this discussion a bit more valuable.

A: We have added some few details on the expected mixing length in the canal reach.

As mentioned however we didn't wanted to perform a too analytical discussion on that. Indeed most of the proposed formulae are strictly related to parameters that in real case histories show an high variability and can offer also variable results. Therefore our purpose is to combine a geophysical visualization of the plume in order to better define if the length is appropriate.

4. P10040, line 20. Not sure how the comment on turbulence relates to Fig. 2?

A: We do agree, that is sort of confusing, we have removed both references from line 20 and 23.

5. P10040, line 24. Did you measure the background EC during the course of the experiment?

A: The background EC has been monitored in the canal by means of a conductivity-meter and no main variation around a stable value of 170 μS/cm (Clemente et al., 2013; Perotti et al., 2013) has been observed. We have added a sentence in the text in this respect.

6. P10040, line 25. As above the manuscript would benefit from a few more details – what you say is that there is an optimum length without ever telling us what it is (or how it relates to your actual separation of measuring points which is partially governed by accessibility).

A: Few more details in respect to the expected mixing length have been added. See also comment 3.

7. Section 3, general comment. While this section is easy to follow, it would benefit from some more details. Frequently you describe the data in general terms and the reader has to look at the figures for the details. It would be much better to include more details in the text (e.g., report concentration ranges, velocities, distances etc). This little extra effort would help the reader follow what is going on much better.

A: We have performed the suggested modification by adding numerical values and

strict references to figures also within the text.

8. P10045, lines 3-13. There are a few interpretations in this section that I am not sure how were arrived at from the data. a. The flow in the canal is described as laminar – is that assumption or did you determine this (with dyes etc)

A: We have preliminary evaluated possible values of the Reynold's number given the natural variability of dimensions of the rectangular canal section and an average flow velocity. In most of our calculations the resulting Reynold's number was below 500 so in a almost "laminar" condition. We can partially agree with the reviewer however that the term laminar is sort of strong: indeed nearly all canals should be designed and operated at turbulent flow, according to many authors. However, as used by Reynolds, "turbulent" flow included everything above "glassy" flow, and it included also conditions found in most irrigation canals, with a low velocity and little vorticity. So we have corrected the statement with the use of the term "sinuous", with the meaning proposed by Scobey (1939) ("a turbulent flow, according to Reynolds, but with a rather placid flow").

Frederick Charles Scobey (1939). Flow of water in irrigation and similar canals. U.S. Dept. of Agriculture. Pp. 79

b. Do you know what the velocities are independently of what you can calculate in this study?

A: Yes, we have performed some sample velocity profiles by means of a current meter showing indeed an increased velocity from the banks to the center of the canal. Some comments have been added in the text in this respect.

c. Not sure what you mean by "coda".

A: Sorry, this was an earthquake derived term (indicating aftershocks after the "passage" of the main tremor) that we thought could be applied also to this out of topic experiment for describing the passage of the plume after the main peak. We have changed it.

d. The comment that the direct sampling may sample one of the peaks may not be the case – your sampling equipment has only a few ports and would be unlikely to well replicate the actual pattern no matter where it was located.

A: This is the main problem we want to discuss in the paper. We are conscious the a local sampling could be inherently limited and for this reason we have increased the number and spatial distribution of sampling points to allow for a more complete sampling. Here we however discuss the fact that if one or few sampling ports are located in a zone where there is, by means of FERT, evidence of higher concentration the obtained measure can be shifted towards higher values. In this respect it is indeed possible to sample only in a main peak zone. Also responding to the other reviewer it has been observed that this is not the full motivation for the shift in the two measurements. Indeed in the discussion we are mentioning all of the critical points of the comparison, all moving in the same shifting direction.

9. P10045, lines 14 onward. I am not really familiar with the FERT technique but it seems that you have two measurements to take into account. There is the conversion of EC to a concentration of Cl but there is the comparison of the FERT measurement to that of the EC meter. Do you have any idea if the two are comparable across the range of EC, and what types of uncertainty this might introduce.

A: FERT measurement is an overall measurement in the zone covered by the measuring quadrupole. In this respect it gives a "volume" estimate of EC in respect to the local one obtained by EC meter. In an homogeneous medium there is wide literature data confirming that the two measurements are directly comparable. This is the case also for the present case study in which the background EC of the natural water in the canal is, as mentioned in the paper, the same if obtained by means of EC meter and FERT. Possible differences in local measurements are related to the inverse reconstruction problem, as discussed, but not to the measuring principle.

10. P10045, lines 14 to 20. Is the distribution of values in the tomographic images

Gaussian (i.e. is a standard deviation the best measure of variability?)

A: The distribution of values in tomographic images is related to the conductivity distribution over a cross section of the canal. Given this our use of std in this part of the paper does not pretend to be related to any statistical distribution of conductivity (i.e. gaussian or not gaussian) but it has been considered more as an indication of the variability of reconstructed data over the image around an average value (in terms of rms or energy level of the image). Therefore std can be considered as an indicator of a non homogeneus tomographic image: the higher std the lower the homogeneity. The combined analysis of std and mean time evolution concurs to give a synthetic history of the plume transit while the series of the corresponding tomographic images give a "movie" of it. We have added some more comments in this respect in the text.

11. P10045, line 19. How much higher? This could use more detail.

A: The difference could be visualized on the related figure. Following also a previous comment we have added some numbers within the text.

12. P10045, lines 21-24. Was there any reason that you did not sample the geochemistry in your experiment of 30 seconds (it looks as if it should have been possible)?

A: We are not sure to understand correctly this observation... Sampling the geochemistry would present the same problems of the direct sampling of EC (in terms of location of sampling points) and therefore, in our opinion, would not be of particularly use. Moreover, given the stable background conductivity of the canal we are quite confident that the only geochemical difference in the water passing in the measuring point is the one related to the NaCl plume artificially created and this can be obtained with EC values. There was also a little misunderstanding in the experiment duration: 30 seconds is the time for acquiring a single tomographic image but the whole experiment is composed of several of these images for a total duration of about 350 seconds.

13. P10045, lines 25 onward. I am not sure whether this should affect the results.

If you produce a smoother peak shouldn't the net Cl concentration remain the same (i.e., the algorithm will produce a peak with a lower centre but higher values around the centre – or did I misunderstand something here?)

A: Here we discuss the fact that the inverse solution based on a damped least squares algorithm is more suitable to images which are inherently smooth. This relates also to the previous discussion in respect to the standard deviation. The damping factor has indeed the effect of artificially smoothing the image around a main peak (i.e. to increase also the std of the distribution) and reduce the difference in the imaged maximum. Therefore in a lower concentration curve it would work better.

14. P10046, lines 7-18. These two paragraphs are not well written, needs clarification.

A: We have tried to better rewrite these paragraphs.

15. I got to this point without ever finding out whether the spacing of sampling points was sufficient such that complete mixing should have been achieved – again more details are needed.

A: More details on the mixing length have been added in the text following also other reviewer observations. However our point is that, even if a not correct distance has been chosen (or was not possible due to logistical condition) the monitoring of the plume by means of FERT could allow for a more rigorous location of sampling points.

16. Additionally, I am not sure what the actual comparison between the techniques are – you reported the discharge from the salt gauging, but not the comparison from FERT (given that this is a paper largely about determining discharge, some comparison so that we can judge if it worth the effort is warranted).

A: We didn't want to compare the discharge of the two method. Indeed in his actual form the FERT suffers from the limitation evidenced; in this sense the two techniques are only qualitative compared and we are not aiming to obtain a discharge evaluation from FERT. Conversely FERT can be used as a visualization tool to obtain a more

significant discharge measure by means of a direct sampling with correctly located sampling points.

17. Conclusions. These are too brief and parochial; they just reiterate some of the specific points from the study. If you want the paper to have more impact, try to discuss the general points – what are the limitations of the method, what are the optimal conditions required to use it, is it really going to replace salt dilution?

A: We have rewritten the conclusions trying to be more specific.

18. Fig. 9. Is it possible to calibrate the time axis?

A: We have added indications in the time axis.

R: Minor Corrections A: Most of minor corrections have been performed.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C5914/2013/hessd-10-C5914-2013-supplement.pdf

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