Hydrol. Earth Syst. Sci. Discuss., 12, C6261–C6268, 2016
www.hydrol-earth-syst-sci-discuss.net/12/C6261/2016/

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Monitoring infiltration processes with high-resolution surface-based Ground-Penetrating Radar" *by* P. Klenk et al.

Anonymous Referee #2

Received and published: 22 January 2016

The authors present a new experiment at their ASSESS test-site using time-lapse GPR (2D, CO with 2 frequencies) for 2 irrigation experiments and one wetting and drying by elevation of the artificial groundwater table. They pinpoint the capabilities of their method to determine material properties and soil moisture quantitatively.

The manuscript (MS) has very high scientific significance since it contributes to enhance the method of GPR for hydrological application. Although the experiment is not immediately transferrable to real conditions it corroborates the basis for spatially distributed monitoring of soil water dynamics with GPR. However, given the substantial overlap to the previous publication (Klenk et al., 2015 doi:10.5194/hess-19-1125-2015) there could be more clarity about the additional insights from the new experiment with

regard to method, applicability and process interpretation.
The scientific quality is fair with drawbacks in terms of the completeness of the presented methods and the discussion of the results. The presentation is good but could highly benefit from more rigour in the structure and scientific argumentation.

The MS is likely to become excellent when the slightly superficial treatment of some core elements of the study is revised. As such I suggest to accept the manuscript for publication in HESS after careful revision according the following lines:

1 General comments

Dear colleagues,

thank you very much for this very insightful experiment and its presentation in your MS. I appreciate the methodology and your analyses very much. However, I find it a little sloppily argued. Some methods are hard to trace in the MS. Some results are rather discussions and other results are not discussed at all... Since I think that everybody can really learn a lot from your study the following comments are meant as constructive suggestions to improve the MS – especially, with regard to the audience of HESS with a broad spectrum of backgrounds in hydrological earth sciences.

1.1 Structure of the MS

The MS does merge method, results and discussion in section 2 with its few subsections. Although it is relatively clear about the experimental setup, the applied methods for interpretation of the data fall a little short and appear somewhat loosely. As such it remains rather imprecise with respect of reproducibility and with reference to possible alternative approaches. I suggest to revise the structure of the MS especially with regard to discussion points which pop-up every here and there. This may also help the alinement of the different steps of the experiment, the derived interpretations and possible intercomparison between the three experiments.

1.2 Precision of the presentation

The MS deserves a careful revision with respect to unambiguity and clarity. E.g. soil moisture could either be volumetric or gravimetric hence a unit should be given to indicate that. By what means are the water table depth measurements derived? What drip irrigation nozzles are used? How is the spatial position of the antennas guided and recorded - since this is one core prerequisite for time-lapse application? How long does it take for one acquisition - since you observe rather fast processes this temporal integral deserves attention. Where is the plywood from fig. 2 in fig. 4? [...] How do you derive and interpret the highlighted precision of 0.1 % soil moisture? I will pinpoint further details in the specific comments. But as it is an unnecessary flaw of the MS I suggest more attention to this in general.

1.3 Interpretation of the experimental findings

The MS quickly highlights the derived precision of the soil moisture estimate and the identification of the subsurface structures. However, relatively few reference for validation is given. This may deserve special attention for the two irrigation experiments as the lateral diffusion of the irrigation impulse is implicitly neglected. Studies in sand columns have shown that this lateral component can become substantial (Germer and Braun, 2015 doi:10.2136/vzj2014.03.0030). This will likely change the material properties for the lateral axis of acquisition. One possible way to take some estimate about the effect could be derived from deviations of the respective antenna couples with dif-

ferent distance. Another option might be some details about the differences in the radargrams from the two irrigation experiments in comparison to the wetting of the full sandbox. From the first experiment your findings imply that within 15 min a steady flow with 150/4 mm/h = 1e-5 m/s established over the whole depth to the ground water table (1.5e-5 m/s in the second experiment). That is a quite interesting finding, especially given the fact of surface water redistribution in semi-arid regions and infiltration at vegetated patches (as one example)... However due to the few references and the quick establishment of the signal it is hard to judge wether this effect is overinterpreted due to the neglect of the lateral component. Maybe the analysis could be extended by some water balance calculations? Moreover, I am very much in favour of the triplet of experiments. However, I miss some intercomparison of the derived findings with reference to processes (under different forcing and in different layers). Furthermore, the distances of the GPR antennas are mentioned but sparsely discussed. I would generally distinguish between effects of antenna construction (antenna ringing) and experimental setup (shallow background reflections, contact to the surface etc.). Is there anything to learn for multi-frequency applications with regard to their resolution and spacing?

1.4 Implications for the hydrological science community

So far the MS has a strong focus on the applied method with the given shortcomings with respect to rigour referencing and testing. Given the overlap to the previous study (Klenk et al., 2015) I would suggest to reference more realistically to "real world application" and process observation: What will be the requirements, limits and precision for less known and more heterogeneous subsurface conditions? Maybe some sort of sensitivity analysis to the interpretation through the CRIM model could be beneficiary. Since this is not the first study using the CRIM model and CRIM is not the only option to interpret effective permittivity it is also worthwhile to discuss the assumptions and limits of this. With regard to resolution of the different frequencies I would expect the higher frequency to resolve more details simply as a matter of the physics. The tradeoff is then attenuation which also increases with frequency. Hence the statements about this deserve more explanation and could be underpinned by some example in the presented radargrams. Hence, on the one hand the overall conclusions should be supported by more precise examples and calculations. On the other hand I propose a more realistic reference to expected behaviour and the application of the method under "real world conditions".

1.5 Reproducibility

With the MS a set of movies helps strongly to understand the presented findings. As far as I understand the best scientific practice it was desirable to include (at least some of) the presented data for reproducing the results.

2 Specific comments

P12216L10: give unit and reference/integration volume of water content

P12217L26: Allroggen et al. worked on a 3x3 m2 area.

P12219L1: maybe time-lapse is more clear than 2+1D?

P12219L13: from a brief checkup in your reference I cannot really see much more details in your previous article.

P12220L6: Fig. 4 comes before Fig. 3 is introduced. Correct order.

P12220L22&L25: Give details about intensity and distribution over the 0.2x1m2 patch. Commonly sprinkler devices should be calibrated.

P12221L24: The CRIM model could deserve a little more motivation. Especially the used parameters/variables need clear definition if the units are dimensionless. Since this is not the first application of this kind, some citations could be worthwhile. E.g. Lane et al., Object-based inversion of crosswell radar tomography data to monitor vegetable-oil injection experiment, SAGEEP, 2003

P12223L3: Some more explanation on figure 5 would be helpful. Maybe the radargrams could be annotated to guide the interpretation for readers with less strong background in GPR? The gravel layer is not in fig. 1. to which depth is it corresponding?

P12223L12: Generally yes and nice approach. Could it also be some imperfect contact of the antennas? Is this result only used within the interpretation of one radargram or transferred to others?

P12224L1: I do not quite get that point. I can see that the distances are largely different, but do not see why and how the 200 MHz pairs are non-equivalent with respect to assuming full equivalence to the 600MHz setups. This is could maybe addressed in the methodological discussion?

P12224L6: Small variations lead to large differences in assessed travel times which is referred to as high precision. While I can follow this argument in dry conditions, isn't it rather the opposite when it comes to conditions where total absence of water cannot be assumed? Hence the statement is somewhat exaggerating the methods capability. As mentioned in the general comments I would appreciate a more rigour assessment of the proposed methods and clear reference about the respective integration volumes.

P12225L19: "common offset" is sometimes abbreviated, sometimes not.

P12226L7: continuous → continue?

P12226L12: I like the figure 8a, but maybe keeping the y-axis as travel time could make it easier to relate the two graphs?

P12228L12: Isn't this a trivial finding? Resolution and attenuation increase with frequency. The issue is to find the best tradeoff between the two.

P12229L2: Is this referring to stretch over the whole depth? You mean that within 15 min a steady flow with 150/4 mm/h = 1e-5 m/s established? That is a quite interesting finding and should be discussed as pointed out in the general comments.

P12229L23: The method you derived these results with is not clearly referenced. And as such the precision is not really easy to judge. In any case the results are very nice, but I think this precision is not really a good benchmark: Especially since I do not see any way to validate it against and since it is integrating quite broadly without further observed reference. Another point is that you must have some sort of close to saturation conditions near the infiltration plumes. But your integral soil moisture signal peaks at 0.12. Hence there is a problem to this already obvious. The point was already raised in the general comments: Please be more precise in the process descriptions and the assessment of the proposed method.

2.1 Figure comments

Movies: The movies for the irrigation experiments lack a timestamp.

Fig. 1: Where is the gravel and concrete layer?

Fig. 3: Figure may deserve some revision. Why not two/three plots? What means manual measurements? For infiltration and percolation:] What happens to soil moisture over time?

Fig. 4: Would a scale-aware sketch be better? Where is the plywood?

Fig. 5: Annotations in the radargrams could be helping the interpretation. Radargrams in greyscale and picked horizons in colour for clarity? More detail on intercomparison of respectively strong/weak representation by the different antenna pairs. Refer to this in the later analysis?

Fig. 6: What is the precision of the picking? Is there any noise in the subtracted mean travel time? To me only CH4 deviates. Hence the agreement is between CH1-3 so far very similar.

Fig. 7: More annotations to guide and support your interpretation would help. Is it really necessary to have the radargrams down to the gravel/concrete layer, since you do not address the time-lapse deviations of this reflector in this figure?

Fig. 9: i-iv should appear in the caption too. I would even suggest to mark them in each time-lapse radargram. While generally visual comparison is ok, maybe one could think of a more aggregated way to present these differences by some kind of attribute connected to the respective reflector?

Fig. 10-12: I cannot trace the method applied here. I guess that it somewhat relates to eq. 4. Maybe it is just a matter of clarity in the MS structure, but so far I am not able to judge these results (see general comments). Generally, I am not clear about why the peak is hardly exceeding 0.12 although there must be conditions around saturation. I am also not clear why the three graphs are chosen although they exhibit some redundancy which is not clearly discussed or related.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 12215, 2015.