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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Imaging groundwater infiltration dynamics in karst vadose zone with long-term ERT monitoring" *by* Arnaud Watlet et al.

T. P. A. Ferre (Editor)

tyferre@gmail.com

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The authors have addressed a particularly difficult topic within hydrogeology: the dynamics of water flow in complex, layered, heterogeneous regions including relatively large preferential (karstic) pathways. They have applied an accepted and widely used hydrogeophysical method (electrical resistivity tomography) to this task. The strength of the study lies in the unique long-term (three year) data set in a karstic environment. The challenge, not surprisingly given the complexity of the system, lies in the interpretation of hydrogeophysical data and the transferrability of the method and results.

As is fitting for an exploratory application of a new method, the study was conducted in a very well characterized area. It would be worthwhile commenting on how well

the ERT data could have been interpreted in the absence of this additional data. This has direct relevance to the use of the ERT method for other, less well-characterized sites. Additionally, the system under study is particularly amenable to study because the water table traverses the known conduits in a typical year, flooding them during winter and running dry in summer. This, again, indicates a wise choice of method for a specific study area. But, it would be worthwhile to comment on this specifically when suggesting that the ERT method could be useful at other karst sites. In other words, it is well known that ERT can only monitor dynamics in as much as it identifies temporal changes in water saturation. How can a reader decide if those conditions are likely to exist at measurable levels at a site before deciding to conduct an ERT survey?

The ERT surveys appear to have been very well designed. The combined use of DD and GD surveys is thoughtful and the description of their differences in terms of spatial information and practical limitations is succinctly stated. The use of automated data collection and analysis, incorporating reciprocal measurements where available, gives confidence in the data quality. Similarly, the authors' recognition of temporal variations in contact resistance is noted as this is often overlooked in long term studies.

I was impressed by the approach used to correct for temperature effects. However, it isn't clear to me that the heat transport was coupled with water flow. Given the complexity of the hydrologic system, this may not have been possible. But, it would be good to add more detail regarding how the temperature distribution was determined to allow for temperature correction. (This may be suited to an appendix.)

It is not clear to me if the resistivity inversion is spatio-temporal or if each time slice was inverted separately. Given that you are looking for changes in time, it seems that spatio-temporal inversion may be more appropriate. But, I would like to have seen some discussion of this choice. It also strikes me that interpreting each time slice independently may be more subject to imposing small scale seasonal variations in areas that are actually not seeing any real variation. It would be very interesting to see if a time lapse inversion results in as good a fit with some areas showing no seasonal

EC changes.

In the end, I felt that the strongest element of this paper was the structural interpretation. This would be strengthened by more discussion of the process by which the arbitrary conductivity-bounds between regions were determined. It would be far more useful if this were explored automatically, perhaps using clustering techniques to propose alternative structural maps. For me, I think that some discussion of the EC limits is needed for publication. But, the paper would have more impact if this analysis were expanded and potentially seen as the basis for forming competing structural hypotheses. The danger as presented is that the authors may have unconsciously chosen EC limits to confirm their pre-existing structural interpretation. This would, of course, limit the value of all of the work that went into collecting the data.

Unfortunately, and not unexpectedly given the complexity of the system under study, I found that the hydrologic interpretations were somewhat qualitative. It is interesting to see that there are correlations and delays between responses. But, it doesn't seem to rise to the level of increased understanding of flow dynamics. This may simply be a matter of emphasis - you could highlight what was learned from the ERT that would not have been possible without it. But, it reads more like using your hydrologic insight to give plausible explanations of what you see in the ERT results. Understand, this isn't a strong criticism. I think that this is an advance and shows potential future use of ERT for monitoring dynamics in some karstic systems. But, I think that it is a mistake to make this the emphasis of the paper - starting with the title. Rather, I would focus the paper on the 'hydrostratigraphic' results - showing how you could use the time lapse ERT to identify structure in the subsurface. That could be expanded and extended and then it would be appropriate to say that this interpretation is consistent with what was seen in other hydrologic measurements. As an added benefit, this would allow you to shrink the hydrologic section, which is not as tightly written as the previous sections.

In summary, I think that this is a very strong paper and that it should be published in HESS. But, I think that the current emphasis on flow dynamics is not ideal. Rather,

it could be a very novel and interesting example of using dynamics to better define structure. This would be most interesting if it could be done automatically, e.g. using clustering tools, and if it led to multiple competing hypotheses that could be further tested in the field.

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