

Interactive comment on “Temporal stability of soil moisture patterns measured by proximal ground-penetrating radar” by J. Minet et al.

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The paper emphasizes the importance of temporal stability in the spatial distribution of water content, yet presents data for less than a month and only a half cycle of drying and subsequent wetting. I agree with the referees that the observation period is simply too short to meet the objectives of the paper.

One referee plays down the importance of the near surface water content. The authors claim it is of importance and increasingly being used. However, the reviewer asserts that this is mainly an artifact of observation techniques that simply are unable to look any deeper into the profile. Hydrologically, the near-surface water content is of much more limited value than the water content within the (generally much thicker) root zone.

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To limit its value even further, the top few centimeters of the soil often become hydrologically disconnected from the subsoil as the top soil dries out. Indeed, farmers actively promote this by making the top soil as crumbly as possible to conserve water deeper in the profile. These aspects are not at all discussed in the paper but warrant attention. In his reply, the senior author acknowledges the limitations associated with the low penetration depth, thereby apparently contradicting the claims in the Introduction. He also states that the value of remotely sensed shallow moisture contents lies primarily in detection of changes and time stability of patterns, but this brings us back to the objection of the very short observation campaign.

In combination, the very short time frame and the limited hydrological relevance of the water content in the top few centimeters of a soil, the inability to assess the effectiveness of the data acquisition procedure under a growing and fully developed crop cast doubts that really can only be met by a much longer period of observation – one referee recommends a full year, which seems sensible. Both referees agree that the use of radar data to assess the stability of soil moisture distribution patterns is novel, but also indicate that the test of this methodology is essentially incomplete because of the very short observation period. Also, only a single field with a single crop (with very limited crop cover) has been investigated. Since this field was used before for GPR studies this limited set-up does hardly provide any information about the applicability of the method for more variable circumstances in soils with more pronounced layering. Thus, the contribution of this paper seems to be incremental. The fact that it partially repeats material published earlier does not help. The lead author acknowledges this as a weak point of the study and indicates this was mentioned in the abstract and the conclusions. Mentioning a weakness does not negate it though, and the study would have been much stronger, had the data acquisition indeed spanned a full year.

One referee brings up some issues regarding kriging in combination with interpolation and suggests to desist from kriging. Given the fact that Table 2 shows that kriging underestimates the standard deviation considerably this may be worthwhile, but the

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reply to this comment clarifies the issue by making clear no interpolation was performed before kriging was applied. (Note, incidentally, the missing unit (%?) of the Nugget over Sill ratio.)

Given the objections against the intersection method raised in the paper as well as by the referees, is this method a realistic approach? The discussion of this method would benefit from a more substantial evaluation. Given the reply by the author, the intersection method appears to have very limited use, which would even diminish further for longer observation periods.

The authors seem to base their work on the assumption that terrestrial GPR is the best way to acquire spatial soil moisture distributions over (fairly) large areas, but in the referee comments a number of alternatives are given. It would be worthwhile to review these alternatives and present recommendations about which method to use under different circumstances.

Section 3.2: The effect of wheel tracks requires attention: if this effect is noticeable within a few weeks, how would that develop in a year? And how does this effect the representativeness of the observation areas/points for their surrounding with supposedly, much less wheel traffic? If the wheel traffic indeed compacted the soil and thereby influenced soil water flow, this would be a very undesirable trait for what is supposed to be a non-invasive technique.

Section 3.3: Can the increasing disconnect between GPR-derived soil moisture and that measured on soil cores shortly after heavy rainfall be related to spatial variation in the depth of the wetting front within the GPR footprint and/or the occurrence of preferential flow paths? Admittedly, full-blown fingered flow is unlikely in loess soils, but macropores may be present.

Fig.7: I cannot see the line representing TSI, unless it is horizontal (constant at zero), but that should not be the case, should it?

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