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Hydrology and Earth System Sciences Discussions

Interactive comment on "Water storage change estimation from in situ shrinkage measurements of clay soils" *by* B. te Brake et al.

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This work uses measurements from two field sites to assert that in shrink-swell clay soils which exhibit normal shrinkage it may be possible to estimate bulk soil water from the surface elevation.

General comments:

The authors claim that this study confirms that clay soils under Dutch climatic conditions exhibit normal isotropic shrinkage. However, I don't necessarily agree with this assertion for two reasons:

1) The authors assume constant, isotropic shrinkage throughout the moisture range.

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Interactive Discussion

Discussion Paper

This is clearly not the case, based both on other studies and the data presented in this study (as in the slopes greater than 1 in Table 2 and Figure 5). If the geometric factor, rs, is less than 3, then the volume loss could be less than the water loss and the slope of the shrinkage curve would be less than unity. rs must be independently measured to accurately relate ΔV and ΔW based on the methodology of this study.

2) The corrections used to account for the effect of changing layer thickness on water content (Equations [5] and [6]) are problematic, as it appears that as a result ΔV and ΔW may be inherently linked. For example, Figure 5 shows that the ΔW values are increasing beyond 60 cm, even though the deepest CS616 probe (at 80 cm) reports no appreciable change once at field capacity. This leads me to believe the plotted changes in ΔW are due only to the measured change in layer thickness. Of course in that case the shrinkage for those layers would appear to be normal. In reality, this only works if the shrinkage is indeed isotropic, but again rs would need to be independently measured to verify this assumption.

As such, this paper would be particularly improved by including a measurement of rs throughout the moisture range for the different layers (such as shown in Peng & Horn, 2007).

It is not clear why the authors chose to present and discuss the results out of chronological order (2011 before 2010). Also, it is unclear why the authors did not include the 2010 data for Field A, since those data would likely be more useful for assessing hysteresis between swelling and shrinkage.

Finally, this paper seems to lack an overarching, conclusive figure which ties the observed data together. I'm not entirely certain what that would look like, but perhaps a schematic of the two soil columns which shows the relationship between volume and water content (shrinkage curve) for each layer, or possibly a visual indication of the volume plotted at several points of a moisture curve. This could also include an estimate of what the surface elevation v. bulk soil water relationship would be, to indeed

HESSD

9, C6113–C6117, 2013

determine if the former is a good proxy for the latter.

Specific Comments:

1) p. 18 l. 19 – This is neither shown nor proven in this paper, and instead relies only on observations from other works. As such it seems out of place in the abstract.

2) p. 19 l. 18 – I don't see how wireless sensors give any greater spatial coverage than wired sensors, except for requiring fewer dataloggers.

3) p. 20 l. 6 – This would be improved with a brief statement about why these measurements were only "partly successful".

4) p. 22 l. 15-17 – This may be true for certain layers or elementary volumes, but not for the whole profile. Volume loss cannot exceed water loss unless solid material is also being lost from the profile.

5) p. 25 l. 20 – This equation is only valid for rs = 3. It may be more advantageous for the authors to present the equation where rs is still a variable (ie. Equation [3] in Bronswijk, 1990). Otherwise, it is very hard to ascertain what effect changes in rs would have on the calculated volume.

6) p. 26 l. 17 – Please discuss how the probes were installed, as this can be important in shrink-swell soils.

7) p. 27 l. 4-7 – Have the authors examined if the changing bulk density of a shrink-swell soil affects the calibration?

8) p. 28 l. 24 – p. 29 l. 4 – Were the readings between the disdrometer and the weather station consistent for the overlapping ranges? If not, could a correction be applied for the weather station data used to fill in the disdrometer data gap?

9) p. 30 l. 20 – I believe there was very minor swelling near the surface.

10) p. 31 l. 11-13 – As in comment 1 above, without having actual laboratory data

HESSD

9, C6113–C6117, 2013

for the shrinkage behavior of individual aggregates, any comparison remains conjecture. While I appreciate that this is not the main point of the study, there are multiple references to how these specific soils will behave differently at the field and laboratory scales, without any data to show this.

11) p. 32 l. 2-5 – This sentence is not very clear, nor is it evident to what the sentence is referencing. I suspect these data are captured within Figure 5, but there is no way to link dates with data points. Instead, this may be more straightforward if the authors reference the ΔV and ΔW ranges within Figure 5.

12) p. 34 l. 13 – This perceived shrinkage of the lowest layer while everything else is swelling is surprising. Do the authors have a theory about why this might be, or is it possibly measurement error?

13) p. 35 l. 12-26 – This paragraph is not very clear, particularly as to which data are being described. I suspect that the scatter in the EC-5 data may be due to contact issues with the probes in a shrink-swell soil. It has been my experience that some sensors may either cause or be located near a crack, which can cause non-linear, hysteretic-like data. Are the authors certain that the sensors are maintaining good contact throughout the study?

14) p. 37 l. 3-5 – As the EC-5 data are not shown directly, it is hard to assess the validity of the authors' hypothesis of sugar beet roots causing the decrease of moisture at 100 cm. Could a crack have developed near that sensor?

15) p. 37 l. 16-17 – If much of the water loss is from structural shrinkage, then surface elevation measurements can not accurately be related to soil water content until the soil has entered a normal shrinkage regime (ie. the measurement will be insensitive at the wetter end of the moisture curve). This should be discussed.

Technical comments:

1) p. 18 l. 16 – should say "relative".

HESSD

9, C6113–C6117, 2013

2) p. 20 l. 13-15 – This sentence is awkward and should be reworded.

3) p. 26 l. 17 – the word “of” is missing.

4) p. 26 l. 20 – should this say “inter”?

5) p. 34 l. 19 – I believe the reference should be for Figure 6c.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 13117, 2012.