

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 addresses the practically important issue of the water storage in a clayey vadose zone.

General comments

1. The authors use: (1) Bronswijk's approach to the measurement of the soil surface elevation change; (2) Bronswijk's approach to the recalculation of the elevation change to the volume change of the soil layer matrix (without cracks); and (3) the known sensors in the water content measurements at two different scales. It would be useful for the potential readership to clearly specify what is considered as the scientific novelty

C6077

of this work.

2. The drawbacks of Bronswijk's approximation in estimating the geometry factor and the necessary appreciable corrections to such r_s determination were argued in the cited work (Chertkov, 2005) as well as in the following work (Chertkov, Open Hydrology J., 2008, 2, 34). Based on the obtained data, the authors, in fact, confirm that r_s should change with water content. In any case, the approximation, $r_s = \text{const} = 3$ that relates to the time more than 20 years ago, is rough, even for the Dutch soils when the ground water level and capillary fringe are sufficiently close to the soil surface. The authors are likely to prefer this approximation (Eq.(3)), because of its outward simplicity. Therefore, for the readership benefit it is worth, at least, clear noting the drawbacks of Bronswijk's approximation and the necessity of the corresponding corrections in the r_s determination to avoid the uncontrolled errors in estimating the volume change of the layer matrix.

3. Some statements in the Conclusions contradict to other statements in the text (please see Specific comments 18 and 19).

4. A part of the Introduction (p.21, l.8 through p.23, l.21) contains a number of inaccurate statements and does not present all available approaches to the soil structure and shrinkage (please see Specific comments 1-9). It should be noted that the work is purely empirical, and the indicated part of the Introduction serves as some decoration that does not influence the basic part of the work. However, if the authors summarize the relevant general concepts they should make it accurately.

Specific comments

1. Description of the shrinkage stages (p.21) contains some confusion and inaccuracies.

(a) The authors simultaneously use the terms: "clay aggregates" (p.21, ll.2 and 8), "shrinking soils" (p.21, ll.2 and 3), "large samples" (p.21, l.10), and "soil aggregates"

C6078

(p.21, l.14). However, in all the cases the aggregated clay soils are implied in the form of a layer or sample.

(b) The standard wordy qualitative repetition about structural shrinkage (p.21, ll.10-13) transits from work to work without, in fact, any serious proof. The origin of structural shrinkage is physically and quantitatively explained and predicted by the existence of the specific aggregate surface layer (Chertkov, SSSAJ, 2007, 71, 641; Open Hydrology J., 2007, 1, 1; Geoderma, 2008, 146, 147; Geoderma, 2012, 173-174, 258), but not the structural (inter-aggregate) pores. The size of the latter cannot change without shrinkage of the contributive clay in aggregates, and in many cases the structural pores can have a negligible volume. However, even in these cases the structural shrinkage takes place in layers and samples of any size (please see the experimental references in the indicated papers), except for the case of the paste-like clay.

(c) The statement about normal shrinkage (p.21, ll.14 and 15) is inaccurate. Normal (basic) shrinkage (for any soil, large/small sample/layer) is characterized by a linear decrease of the total volume with the water loss between the end point of structural shrinkage and the starting point of residual shrinkage, with the slope that is only equal to unity for clay soils with the clay content more than a critical value (in particular, for pure clays). Otherwise, the slope is less than unity at the expense of the lacunar pores that develop at shrinkage inside aggregates and cracks that develop from the initial inter-aggregate pores (see the physical quantitative explanation in the papers indicated in Spec. comm. 1(b)).

(d) Along with the standard wordy "dense packing" explanation of residual shrinkage (p.21, ll.16 and 17) there is the quantitative physical definition where the clay air-entry point corresponds to the initial air phase appearance in the form of isolated air bubbles, and at the end point of the range (the shrinkage limit) the air phase becomes continuous in the pore space (with keeping the water films and isolated water bubbles) (please see Chertkov, Geoderma, 2003, 112, 71).

C6079

2. P.21, l.21 through p.22, l.7. For the physical quantitative prediction of the shrinkage curve accounting for the soil properties, sample/layer size, and texture/structure parameters see the papers indicated in Spec. comm. 1(b).

3. P.22, ll.7 and 8. "Small samples...in the measurement volume". This statement is erroneous. Many experimental shrinkage curves from different available works were obtained on the small samples (no more than 3-6cm) and showed structural shrinkage (for aggregated soils) (please see the papers indicated in Spec. comm. 1(b) and the experimental references in these papers). This additionally confirms that structural shrinkage is not connected with inter-aggregate (structural) pores.

4. P.22, ll.9 and 10. "Another expected...". A difference in slope of the shrinkage curves is also connected with sample size (Chertkov, Geoderma, 2012, 173-174, 258). The slope is equal to the ratio of the total volume change to the volume change of the soil matrix. The total volume change (with water content decrease) contains the negative contributions of the intra-aggregate lacunar pores and inter-aggregate cracks. The absolute value of the negative crack-volume-change contribution increases with the sample size growth and the slope decreases.

5. P.22, ll.10-12. "During normal shrinkage...". This statement is inaccurate (see Spec. comm. 1(c)).

6. P.22, ll.12-20. "Volume change... in packing...slope larger than one. According to...soil particles". Packing cannot be independent of shrinkage-swelling of the contributive clay in both the field soil and samples (see Spec. comm. 1(b)). The slope larger than one is physically impossible. It can be shown before any measurements. In addition, the authors themselves give a corresponding experimental reference.

7. P.22, ll.26 and 27. "...virgin swelling...with slopes close to one." At swelling the initial slope can be both more and less than one (Chertkov, AWR, 2012, 44c, 66).

8. P.22, l.28 through p.23, l.2.

C6080

- (a) Instead of "wetting cycles" there should be "wetting stages" or "phases" (?).
- (b) The physical meaning of the "net resulting shrinkage curve" is vague, especially, accounting for the large possible errors in estimating the volume and water storage changes.
9. P.23, ll.3-6. Bronswijk's (1990) measurements, both with and without overburden, relate to samples. The shrinkage geometry factor values for the layer and sample conditions are, in any case, quite different (Chertkov, 2005).
10. P.25, Eq.(3). Besides the non-realistic assumption that $r_s = \text{const} = 3$ this equation relies on still another hardly controlled implicit strong assumption about a spatially homogeneous water content distribution within the limits of the layer thickness while the water content decreases during drying and shrinkage.
11. P.25, ll.24 and 25. "Values for ... were substituted ...". In general, $z(0)$ value can differ from the z_s value at the saturation.
12. P.25, l.9 through p.26, l.3. What is the authors' estimate (even though intuitive one) of the resulting accuracy (relative and absolute) of the volume change determination of a layer ("volume" means the volume of the soil matrix with no cracks), in particular, for the data in Figs.5 and 7?
13. P.26, l.5 through p.28, l.12. The question as in Spec. comm. 12, but as applied to the water storage change determination.
14. P.31, l.24 through p.32, l.19. It is difficult to say something definite based on such analysis. It is clear, however, that the artefacts, such as the slope more than one, originate from the use of the postulated $r_s = 3$ instead of the actual $r_s(W)$ dependence that can be estimated in advance (see the above references). For still another error source see Spec. comm. 10. In addition, the occurrence of the slope more than one is just some formal sign of possible inadequacy of the handling algorithm. That is, even in the case of the slope less than one the results cannot be considered as dependable

C6081

from the viewpoint of a practical use.

15. P.35, ll.16 and 17. The statement "Since...was incomplete...the soil did not return to its maximum volume..." contradicts to the statement "At the start ...was almost completely recovered to..." (p.33, l.27 through p.34, l.1).
16. P.37, l.12. "Normal shrinkage...". See Spec. comm. 1(c).
17. P.37, ll.13 and 14. "...including inter-aggregate pores, cracks, structural pores...". Inter-aggregate pores and structural pores are the same (?).
18. P.37, ll.14-16. "This confirms...mainly experience normal isotropic shrinkage". "Isotropic" means $r_s = 3$. However, the authors themselves showed (p.32) that the over-estimated r_s value leads to the non-physical slope more than one. That is, the use of the corrected $r_s(W)$ dependence is necessary.
19. P.38, l.6. "...validity of the assumption of isotropic shrinkage...". See the previous specific comment.
20. P.38, l.7. "...can potentially be used...". This statement is only reasonable when accounting for the realistic $r_s(W)$ dependence.
21. P.38, ll.20-27. Any science is not needed when relying on such purely empirical approach.

Technical comments

1. P.18, ll.5 and 6. Instead of "intra-aggregate spaces" there should be "inter-aggregate spaces."
2. P.24, ll.4 and 5. One of two "makes"-words is superfluous.
3. P.35, l.7. One of two "relation"-words is superfluous.
4. P.38, ll.19 and 20. The reference: Te Brake et al.(2012) is only submitted. Is it reasonable?

C6082

C6083