

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

**B. te Brake et al.**

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We thank the reviewer, DSc, PhD Chertkov, for his extensive comments on our manuscript. The thorough review will help us to improve the quality of the work. Below we reply on the comments from the reviewer. For readability, the major parts of the reviewer's comments most relevant for discussion are quoted (indicated by GC or SC for general or specific comments), followed by the author's response (AR). In some cases the response to multiple comments is combined.

### **General Comments**

**GC1:** “The authors use: (1) Bronswijk's approach to the measurement of the soil sur-

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face 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 of this work.”

**AR:** The reviewer indicates that the goal of the work, and thereby the scientific novelty, is insufficiently addressed in the work. Field studies that apply laboratory-measured shrinkage curves with soil surface elevation variations have been published before, but the combination with two types of soil moisture sensors that exclude and include larger cracks is novel. Furthermore, the attempt to reduce the dependence on laboratory-measured shrinkage properties and instead rely more on field observations of the  $\Delta V$ - $\Delta W$  relationship for different soil layers is an innovative element of our approach. In this respect, the use of two types of soil moisture sensor is particularly valuable. It allows us to address scale issues stemming from the difference in measurement volume of the sensors and thereby illustrating its influence on to the obtained field soil shrinkage curve.

**GC2:** “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.”

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**AR (also related to SC18 and SC19):** The reviewer suggests we should state our assumptions more clearly and more explicitly. We agree and will modify the text accordingly, and thank the reviewer for pointing towards useful references for doing so. Bronswijk's assumptions of isotropic and normal shrinkage are indeed attractive, and do have observational support (Bronswijk, 1990, 1991a; Bronswijk and Evers-Vermeer, 1990). For the overall goal of estimating soil water storage change of the entire unsaturated zone and an extension of these estimates over larger areas, it is worthwhile to assess the applicability of these assumptions. To address the reviewers' concerns and to better test the assumptions, we intend to revise the paper by extending our analysis of in-situ shrinkage measurements to further assess the soil moisture dependency of the  $r_s$  factor (done only illustratively in the current manuscript on p.31, l.24 through p.32, l.19.). We are also planning to include an additional figure, illustrating the errors made by estimation of water storage change of the entire unsaturated zone by Bronswijk's approximations and discuss its value for water balance purposes.

#### **Specific comments**

**SC1a-5 and GC4:** These comments all deal with definitions of shrinkage phases, confusing definitions and inaccuracies. Many hints and references have been given by the reviewer to how shrinkage phases and influencing factors could be better defined/described.

**AR:** We will carefully reconsider definitions of volumes and use the right wording to discriminate between clay matrix (individual aggregates), large samples (including multiple aggregates and inter-aggregate voids) and a clay soils in a field situation.

We realize that the reviewer used a definition of structural shrinkage that differed from ours, which might originate from a different volume under consideration. We believe that in a field soil, structural (relatively large and stable) pores are formed by tillage, root activity, burrowing fauna, frost, i.e., various mechanical effects. It is believed that in an aggregated field soil, these pores will dewater upon first drying. The concept of

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the specific aggregate surface layer to facilitate water loss in the structural shrinkage phase, as put forward by the reviewer, assumes a rigid surface layer of aggregates, which facilitates water loss in the structural shrinkage phase. In a field soil, both processes might occur simultaneously, but water loss from structural pores is believed to be more significant. For clay matrix shrinkage the definition of structural shrinkage as currently in the manuscript should be adapted.

We will clarify the current definition of normal and residual shrinkage in the manuscript and improve the organization of the text. A more quantitative definition of shrinkage phases can be included in Figure 1, to aid the definitions.

**SC6:** "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."

**AR (also to GC4):** We realize the reviewer is right. We intended to convey that the arrangement of individual particles and or aggregates can become more dense when external forces work upon them (e.g., by tire pressure, forms of tillage, etc.). This process results in a reduction of soil volume but is not related to bonding of water by clay minerals. Change in aggregate or soil particle configuration, leading to a 'denser' soil, will result in specific volume (e.g. grams per cm<sup>3</sup>) increase of this part and simultaneous be counteracted by macropore/crack specific volume decrease due to the compressed configuration. This will therefore not influence the shrinkage curve slope of the total profile if expressed in specific volumes. Slopes larger than 1 could be an artefact if crack volume and vertical deformation are not independently measured, but this should then result in a change of  $r_s$  factor. We will adapt the text here and clarify the conditions to which this statement applies. The use of the words 'shrinkage curve' in line 17 is probably confusing here and the word 'apparent' might need to be included. We intend to rewrite this section in response to this and other comments.

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**SC9:** “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).”

**AR (also to GC4):** One of the reasons for  $r_s < 3$  (also advocated by Dr. Chertkov (2005) and Chertkov et al. (2004)) is the build-up of tensile stresses resulting in stretching of a shrinking soil prior to cracking. The internal cohesion of the soil initially prevents the soil from cracking, and as a consequence any reduction in volume must come from vertical shrinkage. Once the stress exceeds the cracking threshold, cracks form, and horizontal shrinkage is no longer hampered. In this stage the upper layer of the soil is not considered as a stretching layer anymore, but rather as a column of soil between soil surface and anchoring depth of the ground anchors (Figure 3). We hypothesize that during this stage,  $r_s$  will be close to 3.

As stated at GC2, we will extend the discussion on assumptions, as described by Chertkov (2005), and we will extend our analysis of in-situ shrinkage measurements to further assess the soil moisture dependency of the  $r_s$  factor.

Incidentally, this argument seems to implicitly support augmenting (and perhaps even supplanting) laboratory measurements of the shrinkage curve by field measurements of the kind we present here.

**SC10:** “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.”

**AR:** In the more detailed analysis that we are carrying out to prepare the revision, we examine the behaviour of  $r_s$  for each layer interval. The initial results seem to indicate that  $r_s$  can be assumed to be constant in most of the profile for most of the time. The reviewer appears to be a bit too pessimistic here. We disagree with the assertion that we assume a uniform water content in the vertical segments between sensors.

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This assumption is not required for our analysis, so we do not fully understand why the reviewer introduces it. The number of soil layer thickness gauges (six in a profile) is quite high, and having even more (to increase the vertical resolution in monitoring swelling and shrinkage) is practically unrealistic.

**SC11:** “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.”

**AR:** We believe late in the winter and early in the spring (the reference dates), the assumption of saturation is quite reasonable though (although we agree that May is a bit late, the evapotranspiration is normally well under way by then).

**SC12:** “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?”

**SC13:** “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.”

**SC14:** “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 from the viewpoint of a practical use.”

**AR (also to SC18 and 19):** Slopes in Figure 5 are generally close to one for the deeper layers, indicating a lower bound for the lumped  $r_s$  factor for these layers (since higher  $r_s$  factors would result in slope larger than one). The local and total slopes in the shallow

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layers (0-10 and 0-20) are considerably larger than 1, indicating a need of adapting  $r_s$  factors. We intend to derive realistic upper and lower bounds for  $r_s$  from the data, and from these bounds derive similar bounds for the storage change in the soil. Based on these, we are aiming to include an additional figure, illustrating the errors made by estimation of water storage change of the entire unsaturated zone by Bronswijk's approximations and discuss its value for water balance purposes. A comparison with Meteorological data and soil moisture data will be provided.

**SC17:** "P.37, ll.13 and 14. "...including inter-aggregate pores, cracks, structural pores...". Inter-aggregate pores and structural pores are the same(?)."

**AR:** We will define these terms more clearly in the introduction and review the use throughout the paper. Inter-aggregate pores can be either cracks or structural pores.

**SC18:** "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."

**SC19:** "P.38, l.6. "...validity of the assumption of isotropic shrinkage...". See the previous specific comment."

**AR:** See GC2 and SC14.

**SC20:** "P.38, l.7. "...can potentially be used...". This statement is only reasonable when accounting for the realistic  $r_s(W)$  dependence."

**AR:** A clause indicating a precaution immediately follows the statement in the paper.

**SC21:** "P.38, ll.20-27. Any science is not needed when relying on such purely empirical approach."

**AR:** The reviewer is a bit bold here: the discrepancy between the usual measurement scale and the scales at which related hydrological problems of public interest arise,

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dictates that we make attempts to bridge this gap. Obtaining data directly in the field at a larger scale is one way to do so. Empirical science of such a nature should not be discarded too easily, as it may leave one with tools and data that may have limited relevance. The reviewer himself recognized this in his earlier comment (SC9): there he claims that the shrinkage factors of samples are different from those in a layer, thus implying that laboratory-measured shrinkage factors cannot be applied directly to the field. The logical conclusion seems to be that one must then (at least partly) rely on field data, as we have done. Yet in this comment, this approach is discarded as purely empirical. In a surprising twist (particularly for this journal), the reviewer seems to imply that empirical approaches are inherently unscientific. We respectfully disagree with that implication.

### **Authors response to Technical Comments**

**TC1-3:** agreed

**TC4:** We will update the reference as the paper in question moves through the review process and ensure the final reference will adhere to HESS guidelines.

### **References**

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