Effects of dynamic changes of desiccation cracks on preferential flow: Experimental investigation and numerical modeling
by Luo et al.

In the paper, the authors deal with the important issue of preferential flow, which has been widely analysed in the literature of the last 30-40 years. The work carried out in the paper consists of including the swelling-shrinkage process, induced by changes in soil water contents, in the well-known Dual Permeability Model (DPM) proposed by Gerke and van Genuchten (1996), to account for the dynamic changes of fractures volume with soil wetting and drying.
The approach used by the authors incorporates the swelling-shrinkage approach proposed by Stewart et al. (2016a, b) to describe change of porosity in both the soil fracture and matrix domains.
The work is based on an important experimental work carried out in the laboratory on both small and relatively large columns filled with disturbed soil. The evolution of shrinkage with wetting and drying has been determined experimentally by analysing the images of the soil surface taken by a HD camera. Also, an improved exchange term proposed by Gerke et al. (2013) has been included to account for the exchange between the fracture and the matrix domains.
The data set coming from the column experiments has been used to evaluate the effectiveness of the simulations coming from the proposed model as compared to simulation coming from both a single domain and a rigid double domain model.

General remarks
Based on my reading of the manuscript, the paper is quite well structured. I found everything quite clearly written and explained. The issue dealt with is clearly discussed in the Introduction of the manuscript, with an exhaustive literature. The figures summarize quite clearly the results. Some parameters in the tables should be described better. Also, a table with some more information on the correlation among fitted parameters should be given, along with their confidence intervals. The materials and methods are well explained, with enough and clear details on the experimental work. The development of the fitting procedure is not completely clear. Results are complete and well-illustrated. Most of the discussion and conclusions coming from the numerical simulations seems well supported by the data.

To me, the issue dealt with in the paper is not novel. From a conceptual point of view, the paper mostly retraces the work already carried out by Coppola et al. (2012; 2015). Compared to the latter, the work under review incorporates a new approach for swelling-shrinkage changes of hydraulic properties in both the soil fractures and matrix, as proposed by Stewart et al. (2016a, b). Also, the soil considered is a reconstituted soil, differently from the work by Coppola et al., who calibrated, validated and tested the model on data coming from experiments involving in situ undisturbed soil plot and undisturbed soils samples taken from the soil matrix of the same plot.
Some statements in the Introduction and in the discussion and conclusions seems a bit forced and misleading. I will try to argue about them, also to discuss some other issues the authors dealt with in the manuscript.
In the Introduction, the authors state: "*Coppola et al. (2012); (2015) took another step forward to allowed crack volume and/or hydrological properties to vary as a function of soil shrinkage. However, the relationship proposed in the model, a natural logarithm function involving the suction head and crack proportion, lacks physical consistency with the variation of porosity. This implies a disconnection between hydrological properties and porosity in the crack domain.*" A similar statement may be found again in the conclusions of the paper under review. To me, this statement appears as a wrong and approximate interpretation and reproduction of a quite hurried conclusion drawn by Stewart et al. (2016a), who wrote (page

7912): "*Coppola et al. [2012, 2015] allowed β and/or the soil hydraulic properties (e.g., volumetric water content, hydraulic conductivity) to vary as a function of soil shrinkage. However, the relationships proposed in those models lack physical consistency, in that domain specific hydraulic properties (e.g., hydraulic conductivity) remain constant regardless of changes in porosity distribution (e.g., β). This disconnect* (as they wrote in their paper) *between hydraulic properties and swelling ….*".
As may be deduced in Stewart et al., the disconnection they speak about concerns the fact that hydraulic properties are not allowed to change with porosity changes. By reading carefully the paper by Coppola et al. (2012), this argument is unfounded. In the section 3.3, the authors clearly explained how the $\vartheta_a(h)$ and $K(\vartheta_a)$ (ϑ_a is the moisture ratio of the soil matrix) experimental data points measured on the soil cores were converted to as many $\theta_a(h)$ and $K(\theta_a)$ points by using the equation 10a and the $e_a(h)$ values measured at the same h. Thus, the $\theta_a(h)$ and $\theta_f(h)$ (and the corresponding $K(\theta_a)$ and $K(\theta_f)$) parameters comes from the $\vartheta_a(h)$ adjusted for the $e_a(h)$ data and, once used as input in the code, already account for the deformation of both domains with changing h. In other words, the θ and the K values calculated during simulations for a given h value at a given simulation node already accounts for the deformation of pore-size distributions of both the domains under swelling/shrinkage. What's more, the authors also allowed the fraction of the matrix and fracture porosity to change along with hydraulic properties. As this is not a simple task from an analytic point of view, they assumed a logarithmic function describing the ß(h) evolution, but this is another story and has nothing to do with the physical inconsistency and the disconnection between changes in porosity and hydraulic properties Stewart et al. discussed about.
As for the paper by Coppola et al. (2015), it simply showed three scenarios where the swelling-shrinking cycles were assumed to alternatively affect 1) only the hydraulic properties, 2) only the fraction of the two porosities with no effects on the hydraulic properties, 3) both, in the combined approach already presented in the 2012 paper. So, saying that these approaches do not account for changes in hydraulic properties is simply unfounded.

In any case, if the physical inconsistency lies in the disconnection between changes of porosity and corresponding changes in hydraulic properties, as argued by Stewart et al. (2016a, b), this could more apply to the paper under review. In fact, the authors should explain clearly where in their paper they change the hydraulic properties with swelling-shrinking cycles. If I well understood, their approach assumes fixed hydraulic property shape parameters (see table 3) for both the domains and the porosity is assumed to change according to equations 18 and 19. The Ks is scaled according to the changes in the porosity. I guess the saturated water content also scales similarly, even if I did not find any explanation of how the change in the porosity scales the water retention curves. Is Ks only a function of the porosity, rather than of the whole pore-size distribution? Do the authors believe that swelling-shrinkage simply scales the hydraulic properties (that is, swelling shrinkage has only effects on the total porosity of both the domains), as suggested by unchanged shape parameters of the soil hydraulic properties, or rather it changes the whole pore-size distribution (as considered by Coppola et al. in the 2012 paper and in the approach they called ßk in the 2015 paper)?

To me, it seems that the argument of the physical inconsistency was introduced in the paper rather to maintain the usefulness of using a swelling-shrinkage approach a bit different from that previously used. I find the Stewart et al. approach actually effective and physically attractive, but this do not requires arguing that the approach by Coppola et al. is physically inconsistent.

Other comments:
Page 4, line 134: This means that the authors assume the Ks and the hydraulic properties obtained on a reconstituted soil sample represent the Ks and the hydraulic properties of the

matrix in the reconstituted large soil column with the same bulk density. This is confirmed at page 14, line 397. This would imply that the Ks depends only on the bulk density of the soil. The authors should discuss more this point;

Table 1: I did not understand what is the optimal water content, ωopt, in the table;

Page 7, line 221. This is the first time you introduce COMSOL in the paper. I would explain here what it is.

Page 12, line 337. Did the authors account for this interspace when calculating the fractures volume change?

Table 3: as for the parameters pertaining to the DPM and DPMDy, are they the final parameters coming from fitting or are they the initial guess values? In any case, the fracture parameters seem quite strange. The saturated water content simply suggests a void without solid particles, which is not coherent with the n parameter, suggesting at least a sand porous medium;

Page 24, line 618. I did not understand the sentence "…improper exchange term (Coppola et al., 2012; 2015). Did these authors use an improper exchange term?

Section 6.3. Model performance: It would have been interesting to see a table in this section summarising quantitatively the effectiveness of the model fitting, with optimized parameters, correlation matrix, confidence intervals, …)

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