# Reply to Professor Coppola's comments

Dear Professor Coppola, thank you for the positive and constructive suggestions to improve our manuscript. Below are my responses to your comments..

***Comment 1:*** *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.*

**Response:** Thank you for this insightful summary of our work.

***General remarks***

***Comment 2:*** *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.*

***Response:*** *We are very happy with your appreciation of the work. We agree that some numerical parameters in Table 3 need further clarification. Please see reply to comment 10. Regarding to the fitting procedure, we will add more information in the revised manuscript.*

***Comment 3:*** *① To me, the issue dealt with in the paper is not novel. ② 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). ③ 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.*

**Response:** Thanks for these comments. For comment ①, yes, we agree with you that the PF-DC is a classical research topic, but still important to continuing studying as we believe the approach to simulate and quantify the PF-DC has room for improvement. In our paper, the novelty lies in the implementation of the Stewart et al (2016a, b) model for soil swelling-shrinking behavior, evolution of desiccation cracks and associated hydrological process during wetting-drying cycles.

For comment ②, it is true that we build on the pioneering work of Coppola et al (2012; 2015) as also referred to. In our work we show that the empirical relationships between the crack area and the suction head in our experiments do not follow a natural logarithm function. We realized that this could be ascribed to the different soil samples and different boundary conditions. In our work we follow the shrinking-swelling model proposed by Stewart et al. (2016a, b) which fitted our observation better. Hence, the Stewart et al (2016a, b) function was incorporated into the Richards-based dual-permeability framework, leading to a slightly modified dynamic PF-DC model for swelling-shrinking clays.

For comment ③, yes, you are correct, we used a reconstituted soil in our lab experiments. Using a reconstituted soil or an undisturbed soil has its own advantages and disadvantages. The former can eliminate the effects of other macropores on the preferential flow but needs long time to produce highly developed desiccation cracks. The latter can provide well-developed desiccation cracks but may be affected by other macropores (i.e. root pores or earthworm holes).

***Comment 4:*** *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."* ① ***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.* ② ***In the section 3.3, the authors clearly explained how the $\vartheta_a(h)$ and $K(\vartheta_a)$ ( $\vartheta_a$ 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 $e_a(h)$ a 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.***

***③ As this is not a simple task from an analytic point of view, they assumed a logarithmic function describing the β(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.*

**Response:** Thank you for pointing this out.

For comment ①, indeed we do have a statement looking similar as Stewart et al. (2016a), but our questioned point is not the same. Indeed, we disagree with their statement that "…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 …". We are aware that the Coppola et al 2012; 2015 model allows the hydraulic properties in each domain to vary with the porosity of each domain . We will explicitly mention this in our revision.

For comment ②, as shown in our text (Page 3 Line 93-95), we do not mention this issue. What we focus on is the physical consistency of the empirical relationships between the crack area and the suction head. Here we did not adopt the natural logarithm function as we argue that it may be not suitable to other soil types.

For comment ③, we also realized that our text created some ambiguity. We will change it as follows:

Coppola et al. (2012); (2015) took another step forward to allow crack volume and/or hydrological properties to vary as a function of soil shrinkage. However, the relationship proposed in the model, an empirical natural logarithm function linking the suction head and crack proportion, could be not suitable to other kinds of soil.

***Comment 5:*** *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.* ① ***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?*** ② ***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 βk in the 2015 paper)?***

③ ***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.***

**Response:** Thanks for the insightful and thought-provoking comments.

For comment ①, indeed we fixed the SWRC shape parameters for each domain, and the matrix porosity is assumed to change with the saturation degree (from SWRC) by combining the shrinking-swelling parameters (Eq. 18 and Eq. 19). Our assumption means that the soil shrinking-swelling behavior has less influence on the SWRC shape but more influence on the porosity and therefore the saturated hydraulic conductivity. Hence, you're correct that in our model $K_s$ of each domain is only a function of the porosity.

For comment ②, we conceptualize the soil shrinking-swelling behavior, which has effects both on the total porosity and pore-size distribution. However, in this current study, we neglected the shift of pore-size distribution during shrinking-swelling process and assumed that process has more influence on the porosity variation. We will add more explanation involving our assumption in the revised manuscript.

For comment ③, we are sorry for the unwanted criticism and we will delete discussions related to the physically inconsistency.

***Other comment***

***Comment 6:*** *Page 4, line 134: This means that the authors assume the $K_s$ and the hydraulic properties obtained on a reconstituted soil sample represent the $K_s$ 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 $K_s$ depends only on the bulk density of the soil. The authors should discuss more this point;*

**Response:** Thanks for this fair comment. Here, please note that the $K_s$ obtained on a reconstituted soil sample represents the maximum $K_s$ of the matrix domain prior to shrinkage. When the soil matrix begins shrinking, the $K_s$ of the matrix domain will decline because its porosity decreases. The porosity of soil is negatively linear correlated to the soil bulk density. Therefore, you are correct that we conceptualize in our analysis that the $K_s$ depends only on the soil porosity. We will add this to the discussion.

***Comment 7:*** *Table 1: I did not understand what is the optimal water content, $w_{opt}$, in the table;*

**Response:** Sorry for the short in explanation. The optimal water content, $w_{opt}$, is often used in the road engineering field, and it refers to the water content corresponding to the maximum dry density (also called the best compaction status). We will add more explanation in the revised manuscript.

***Comment 8:*** *Page 7, line 221. This is the first time you introduce COMSOL in the paper. I would explain here what it is.*

**Response:** COMSOL Multiphysics is a finite element analysis solver and simulation software package for various physics and engineering applications, especially coupled phenomena and multiphysics. We will add this explanation in the revised manuscript.

***Comment 9:*** *Page 12, line 337. Did the authors account for this interspace when calculating the fractures volume change?*

**Response:** We did not account for the interspace. As you can see in Fig. 3, to avoid pixel distortion near the photo edge, we only cropped central area of the photo to be used as crack image analysis.

***Comment 10:*** *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;*

**Response:** For Table 3, only SWRC parameters for the crack domain and mass transfer coefficient $a_w$ come from empirically guess. The other parameters all come from fitting. Regarding the SWRC parameters for the crack domain, we agree that the $n$ parameter looks a little bit small but we believe it may be still acceptable. Most importantly, it is the most robust value when running the model under wetting-drying cycles.

***Comment 11:*** *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?*

**Response:** This indeed is misleading. As it can be seen in the manuscript (line 416-424), we mentioned that setting the $K_a$ as the arithmetic mean of hydraulic conductivity of the two domains would overestimate the $K_a$ when the hydraulic conductivity of the crack domain is much higher than that of the matrix domain. For instance, in our case, because we regarded the crack as empty space, its maximum $K_s$ is six orders of magnitude larger than that of the soil matrix. Therefore, the arithmetic form is improper under such conditions.

In Coppola et al. 2012; 2015, the crack domain was regarded as space filled with soil particles and the difference between the matrix and crack hydraulic conductivity fell within two orders of magnitude. Therefore, in that case, using the arithmetic mean was appropriate.

However, we hold that the crack domain should be regarded with a water storage space with large voids. Consequently, we state that the arithmetic mean leads to an improper estimate of the exchange term.

***Comment 12:*** *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, …*

**Response:** Thanks for the suggestion. We will try to add a table in this section.