Comments on the authors' responses and the revised manuscript of Shao et al.

The authors have incorporated all technical corrections and made minor revisions according to most of the reviewers` suggestions. Some points still need to be revised before publication, please see below.

A more comprehensive streamlining of the argumentation and detailed discussion of the impact of the chosen model setup and scenario would have improved the paper even further. For example, the authors do not explain why a quite large part of the discussion is on the coupling term in dual-permeability models. And at least some speculations of how other possible combinations of parameters, soil materials or spatial heterogeneity would affect the results and if this needs to be addressed in further research would have been desirable. This would also relate to the effects of intermittent and variable rainfall, which is acknowledged as important in the discussion, but not related to the present study. Maybe the authors could still include that in the manuscript.

Specific comments

Answer 9: COMSOL should be mentioned at p. 3 ll. 15 – 25 as well, even if the authors did not make use / had accession to the Geomechanics module of COMSOL, which seems to be available for the COMSOL software.

Answer 23: This expanded explanation ("at each point within a hillslope") should be included in the manuscript, as the spatial dependence is not clear from eqs. 16 and 17

Answer 28: I am still not sure what you mean by "a non-unique parameter set". Inverse modelling may result in equifinal parameter sets, as this often is an 'ill-posed' problem. But in this case, there are more than one parameter set. In contrast, the other approach you favour basically means that parameters are set arbitrarily without bothering to determine them from any data. While I still agree that this approach may be useful when using models as testable hypotheses, you maybe would like to revise the argumentation in this direction.

Answer 30: Please include this answer also in the revised version, that is, please indicate that saturation excess overland flow occurs, instead of "rainfall exceeds infiltration capacity".

Answer 39: "Hazard assessment" is still in the heading (5.4), and should be replaced with "slope stability". Instead, you replaced "slope failure" in the previous paragraph with "slope stability", which should be undone.

Answer 42: Please discuss which effects could not be modelled with a single-domain approach, for example, when taking spatial heterogeneity or a bi-modal soil hydraulic parameterization into account (e.g., "Zurmühl, T ; Durner, W, 1996: Modeling transient water and solute transport in a biporous soil. WATER RESOURCES RESEARCH 32(4), 819-829, DOI: 10.1029/95WR01678").

p. 14, l. 24 – p. 15, ll 6: This paragraph, related to rainfall exceeding infiltration, needs clarification. You state that rainfall never exceeds infiltration capacity, nonetheless, infiltration rate drops in Fig. 5a. You further state first that 90 % of the rainfall infiltrates into the matrix, later you write about 80 % of infiltrated rainfall in the matrix.

p19, ll. 11-13: "experimental studies … show a non-equilibrium phenomenon between the two domains" – please cite references, and perhaps explain the "phenomenon"

Technical comments
Heading 5.2. - Perhaps "Coupling term in THE dual-permeability model"