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Discussions

Interactive comment on "Assessing the cover crop effect on soil hydraulic properties by inverse modelling in a 10-year field trial" *by* José Luis Gabriel et al.

Anonymous Referee #2

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Review of "Assessing the cover crop effect on soil hydraulic properties by inverse modelling in a 10-year field trial" submitted to HESS by Jose Luis Gabriel, Miguel Quemada, Diana Martín-Lammerding, and Marnik Vanclooster

Gabriel and co-workers present a very comprehensive study on the effect of barley cover-cropping and minimum tillage on the hydraulic soil properties based on a 10-year experiment and subsequent inverse modelling for parameter identification. The study and the manuscript are well thought and worked out. It definitely fits in the scope of HESS and will be a fruitful contribution to the field.

That said and with all respect to this work, I however see some fundamental issues with the presented study which I would suggest the authors to reconsider:

1. Why is there no direct soil data from some time within the 10-year experiment used for validation?
2. How can one account for the effect of changing soil conditions on the initially calibrated sensor readings?
3. How do the authors account for equifinality and parameter interaction in their inverse modelling?
4. Is the WAVE-WOFOST model system evaluated for its general necessity/suitability in the study and potential parameter interaction and identifiability of the required soil parameters?

(1 & 2) The authors base their study on continuous measurements of soil moisture in 6 depth levels (0.1 to 1.1 m, 0.2 m increments) on six treatment plots. In addition meteorological state variables are recorded on site. As very good practice, the soil moisture sensors have been calibrated under field and lab conditions to the specific soil condition previous to the experiment period. At this time a pedohydrological analysis was done, too. Since the study is about the effect of cover crop and tilling routines on soil hydraulic properties, I suspect that the authors have considered to directly sample the soil repeatedly to answer their research question.

What I find especially challenging is that a calibration of the capacitive sensors to a specific soil condition might be in question once the soil matrix properties change. As such, there might be an issue that the changing soil properties under study may have indiscernible effect on the actual measured dynamics. With regard to interaggregate and macropore structure formation, the effect should be more clear to be identified as the resulting dynamics do not rely on precisely measure the absolute soil moisture but the relative dynamics during events. However, the authors do not show direct pedophysical measurements from during the experiments, nor do they discern different (event-scale) soil moisture dynamics (diffusive redistribution vs. initially advective percolation in soil macro structures). I find that an unnecessary flaw in the well-thought study. With regard to the presented results, I suspect that a more detailed view to different temporal integrals of the hydrological responses might allow for more specific and less equifinal parameter identification.

(3) The authors present a multi-step parameterisation scheme, which first addresses the crop parameters based on observed properties like biomass production, ground cover and root development. In a second step they used the Shuffled Complex Evolution Metropolis algorithm for optimisation (and uncertainty assessment) of the pedohydrological parameters. While this appears as very reasonable choice to derive the parameters for the system, it still cannot resolve the issue of potential parameter interaction and equifinality. Especially so, when the objective function evaluates the full observation time series and does not include the crop parameters. Given the criterions (NSE and RMSE), I would expect the parameters to trend towards acceptable timing and fit of the mean soil moisture. This to my understanding would not be sensitive to changes in bulk density but might only weakly consider the creation of macro-structure (which the authors point out to be one important process). A Richards-type single-domain model might still be capable to assess the research questions, but I suspect that some event responses and respective percolation properties, should be studied in more detail. For this, a more data-focussed approach might be the first step.

(4) Assumingly in order to dynamically determine evaporation, the authors coupled the Richards-type soil water model WAVE with the crop model WOFOST. While this appears to be a very reasonable step, it might not be central for the event-scale soil water dynamics. Moreover, it introduces a quite large number of potentially interacting parameters. Even if one reduces the about 120 crop parameters to the most sensitive 10, their interaction with the soil parameters is still in question. From the biomass plot in fig.

2 I get the impression that the model actually only simulates 3 to 4 different “classes” of production, which are not really coherent with the observations (maybe except for the very lowest ones). This actually is in line with my expectations that WOFOST (unfortunately) is not really capable to be used in a dynamic eco-hydrological setup. For the study at hand, erroneous crop simulations may be susceptible to blur the actual soil parameter effects without providing the desired advances of more correct evaporation simulations. Since apparently ground cover of the cover crops has been observed occasionally and since WOFOST suggests only 3-4 crop development scenarios, maybe a more direct implementation of an ET estimate might reduce ambiguity in the analysis?

Despite my concerns and suggestions for the methods used in the study, with regard to the current setup I find it necessary that the authors give more insight into the observed and modelled soil water dynamics, the parameters apart from fig. 3, the used time stepping, and at best some more details about the actual model realisations (especially since the authors use their own Matlab derivatives). This could partly also be given as supplement.

As such, I suggest the manuscript to be considered for major revisions.

Minor comments:

P2L4: soil size pore distribution » pore size distribution?

P2L31f: I am not sure, if inverse modelling is specifically useful “to overcome a parameter limitation problem” as it faces the issue of parameter interaction and equifinality. I would expect more specific explanation and citations here.

P3L2: Although I agree to the general attitude that “multi-sensor” probes have advantages, I do not see that the study could not be done with more standard soil moisture probes. Especially with regard to the nature of capacitive sensors being potentially more effected by changes in the soil properties, one could also think of alternative setups - eg. using TDR probes.

P3L3ff: I would expect that the uncertainties are not directly depending on the identification strategy of the parameters. Thus a consistent measurement over depth might allow for the assumption that uncertainties between the individual records might be reduced. However, under natural conditions there might always be air and gravel entrapments altering the control volume. Maybe I misunderstood the statement?

P3L18: I would expect the weather station's sensors to be more important than the logger...

P3L24f: I can grasp the study layout from the description. However, I would suggest a small plot, clarifying on the locations of the random plots, the respective treatments and the locations of the observation stations.

P4L2: I do not quite understand: There are 8 randomly chosen plots, but only in 6 soil moisture was monitored?

P4L4f: This calibration is very good practice. However, your experiment might raise the question if such a calibration remains valid for changing soil conditions. . . I expect this also holds true for the soil hydraulic properties in general. I would suggest to include a paragraph on this in the discussion. Section 2.3: The model system appears very parameter-rich and finally rather complex. Although I can follow your description having once coupled WOFOST with the hydrological model SWAP, I am not convinced that this description suffices to be able to understand the coupled model system and to reproduce your results. Moreover at this stage of reading, I slightly doubt that the model system is actually required to answer the research question.

P4L28f: Was WOFOST used to determine the crop development and soil water use? How has it been integrated? From my experiences, coupling WOFOST and any hydrological model may result in even worse identifiable parameter sets since the crop parameters compensate for soil definitions and vice versa.

P6L15: I would not consider 612 mm as “very humid” » relatively humid?

For the rest of the paper, I refer to my general remarks above.

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