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Hydrology and Earth System Sciences
Discussions

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

**Jose Luis Gabriel et al.**

gabriel.jose@inia.es

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We want to thank the reviewer all the suggestions made, because they will improve the final version of the manuscript. The reviewer can find here the answer to all the questions suggested but also in the attached documents.

Comments on the manuscript HESS-2017-643 -The manuscript explores the changes of soil hydraulic properties induced by a cover crop using the field measured soil moisture of several plots in a 10-years experiment, on a silty clay loam soil, in central Spain, and the results generated by a model fitted to those data. The subject is well-known, but since the published results are always limited to the site and managements sys-



tems, new works are welcome to enlarge our knowledge on the evolution on soil properties under long term treatments. The manuscript is well written and the results seem interesting. Nevertheless there are certain problems which require a major revision. Main comments 1. The use of a hydrological model to evaluate the change of soil properties, in this case the parameters of the van Genuchten soil water retention equation, might not be the best way to detect the modification of soil properties, especially in the case of a 10-years experiment of a cover crop in soil under rainfed periods, with a reduced number of direct measurements of these properties. In this work the authors have selected arbitrary depth intervals, different from the soil horizons described by Gabriel and Quemada (2011, Table 1), with samples of different textural classes.

Reply: The selection of the depth intervals was not made arbitrarily. It is true that the trench characterization made in 2006 and presented by Gabriel and Quemada (2011) was a little bit different (0-23, 23-40, 40-70 and 70-120 cm). However, with additional samples collected along the entire experimental plot there, it was decided to redefine the soil layers as follows: 0-20, 20-40, 40-80 and 80-120 cm. This definition was maintained since 2012, as illustrated already in Gabriel et al. 2012.

-In each of these depth intervals have taken samples to measure, later in the laboratory, the hydraulic conductivity at saturation, and saturated soil water content. Fitting the hydrological model WAVE results to the field measured soil moisture date, the hydraulic conductivity at saturation, the normalizing parameter of the matric component of soil water potential, $\alpha$, the exponent n, and the water contents residual and saturated, of the van Genuchten soil water retention equation were estimated for the successive yearly periods without a regular crop on the field.

Reply: We should reemphasize that there were two calibrations each year: one calibration for the fallow treatment (without a regular crop on the field); but also another calibration for the barley treatment. Barley is a regular crop broadly used under rainfed conditions in the region, with the only particularity that it is killed before harvest, but after flowering.

-The results shown in Figure 1 of this manuscript do not show great changes between years and treatments.

Reply: Differences between years could be quite important: rainfall in 2009/10 was more than four times larger than 2011/12. Temperature change may also be significant. For instance, minimum temperature in November 2006 was 1.5oC, but -9.4oC in 2007, leading to large differences in crop development.

-Nevertheless, there are some oscillations, which, at least in the shallower interval, 0-20 cm, seem related to the monthly rainfall shown in Figure 1. To check this apparent similarity, I took the data of rainfall in the three autumn months of every year, the main soil water recharge period in the Spain Mediterranean climate, from Figure 1, and plotted them against some of the van Genuchten parameters. Figure 1 here indicates a decreasing trend of the estimated van Genuchten $\alpha$ parameter with the autumn rainfall for the 0-20 cm depth interval. The trend was not so clear in the case of other parameters at this depth interval, which is the most directly affected by the rain water. Therefore, it seems that the estimated values of some parameters could be affected by the year rain, what is surprising. One could not expect soil properties changing by the rain. The authors must check these results to avoid a distortion in the estimation of soil properties with the model-fitting methods.

Reply: We agree that this effect is true in the case of the $\alpha$ parameter, but only for this parameter and only in the case of the barley treatment and not for the fallow. After some statistical analysis, we observed that when we considered larger or shorter periods than the three months suggested by the reviewer, the correlation between alfa and P decreased. We noted also that the $\alpha$ parameter has a low sensitivity, which results in higher uncertainty in the parameter estimation. This is confirmed by the larger error bars obtained along the soil profile. Finally, the structure of the top layer could be affected by rainfall distribution. Drop impacts can lead to particle disaggregation, and larger amount of rainfall can favour clay particles transport and with possible clogging. Also, the direct soil radiation lead to great and fast changes from wet to dry/crust



formation and very low temperatures can produce punctual ice formation at the very upper layer. All such processes can lead to differences in soil pore distribution and changes in the hydraulic properties. Changes have been made in the manuscript to reply to these remarks of the reviewer.

-2. Independent of the mentioned trend, the interpretation of the results in the manuscript, section 3.3 is excessively optimistic. The two phases indicated in the line 1, page 10, not too evident in the different plots of Figure 3, could be due to the influence of the successive rainy autumns 2008-2009, and 2009-2010, more than to a soil generated change.

Reply: The point that let us to support the two phase process hypothesis is that during a first phase, the properties in the barley and the fallow treatments varied equally. However, after a variable time, varying from 2 to 4 years, depending on the parameters and depths, the trends between treatments changed. We have included some clarification in the manuscript.

-The different values of the estimated residual water content in the deeper soil horizons could be due to the relatively stable soil water contents at those depths more than to a compaction, as the authors suggest.

Reply: We think that there is a misunderstanding, because we say in the manuscript that soil compaction effect is showed by the residual water content at 20-40, not in the deeper horizons, where we do not find any effect of time or treatment. We changed the wording “This suggests small differences in the micropore structure between fallow and CC treatments $\theta$. This suggests small differences in the micropore structure between fallow and CC treatments.”

-In any case the authors should provide additional support other than the reference to other experiments in a different place. The compaction should have induced a reduction of the saturated water content not too different between the two treatments in the second depth interval, 20-40 cm. However, the estimated values of the hydraulic conductivity at saturation at the third depth interval, 40-80 cm, do not suggest any compaction influence. Again, the discussion of the results must be thoroughly revised. 3. Is section 3.2 required for the manuscript? A simpler indication of the role of the crop on the estimation of evapotranspiration rates could probably be enough for the explanation of the results. Reply: This has been reduced and incorporated to section 3.1.

Minor comments 1. Some recent relevant articles are missing, among them van Es et al. (1999), Basche et al. (2016), Rorick and Kladivko (2017), who measured points of the soil water retention curve. Besides, the Introduction could be abbreviated. Possibly some of the other references might not be needed, (many double references for a single statement). Reply: They have been included

2. In line 2-3, page 2, if the soil bulk density increases the porosity consequently decreases. This part of the sentence is not needed in the text. Reply: It has been changed

3. The sentence in lines 5-8 should be rewritten, (too many dynamics). Reply: It has been changed

4. Is not the Köppen system more universal and more convenient than the Papadakis' one to classify a climate? (e.g. Peel et al. 2007). Reply: It has been changed

5. What do the authors mean with the term 'field capacity' (line 13, page 4)? Later, line 11, page 11, the term is explained, but I could suggest the use of a more sound definition of the term (e.g. Assouline and Or, 2014) to gain in precision.

6. The statement of lines 25-26 of page 5, is repeated from that of lines 18-19 of page 4.

Reply: Actually, they are not the same. On page 4 we are defining how we can get soil cover based on a picture, but on page 5 we are defining how to get soil cover based on the LAI simulated by WAVE. The misunderstanding can be produced by the fact that both methodologies are defined in the same manuscript, but they are independent and



both necessary in the manuscript. We have rewritten the sentence for clarification.

7. Does figure 1 need to contain the average maximum and minimum temperatures?

Reply: They provide some extra-information than only the absolute maximum-minimum, as in January 2009, no so different on average to December and February but with some critical punctual moments, and as all the lines are not crossing between them, the readability is not compromised.

8. Could the new index of Willmott et al. (2012) be more suited for the occurrence of very different values of soil moisture, (great and small), than the Nash-Sutcliffe efficiency index?

Reply: It is true, but we preferred to keep using in this experiment Nash and Shutcliffe for a better comparison with the previous work developed in the same experiment. We have included a sentence in the manuscript.

9. Is Figure 4 necessary for the manuscript? If the authors think it is, why the water retention curves are limited to the second and third depth intervals? The numbers on the labels of the x-axis must be negative, since the matric component of soil water potential has negative values. Reply: It has been removed and corrected in the manuscript.

10. The use of macro- and microporosity, (line 1, page 11), or of the velocity of the infiltration processes, (line 12, page 11), should be based on solid reasons. Reply: It has been rewritten

11. The sentence in lines 9-10 of page 12 is speculative. Reply: It has been rewritten
12. The sentences in lines 7-12 are questionable, and not a consequence of the results found in the manuscript. Reply: They have been rewritten

13. The reference of Scanlan (2009) is incomplete, (line 6-7, page 16). Reply: This is all the information that the Journal stile demands for this kind of books and PhD dissertations.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-643/hess-2017-643-AC1-supplement.zip

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