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Interactive comment on "Estimating hydraulic conductivity of a crusted loamy soil from beerkan experiments in a Mediterranean vineyard" *by* Vincenzo Alagna et al.

Anonymous Referee #1

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This paper presents the application of the beercan method to determine hydraulic properties of a crusted and non-crusted soil in a vineyard. The main conclusion is that method is an appropriate and practical method to characterize hydraulic properties of a crusted soil. Practical field methods are indeed very much needed to determine the effect of soil management on crust formation and its effect on soil hydrology. In that respect, this paper makes an interesting incremental contribution. Yet, the impact of the study would have been larger if the method was compared with another method. The conclusion that the beercan method gives reliable results cannot be checked since there were no other methods used against which the results obtained from the beercan method could have been compared. I also think that the presented results could

be extended by including other parameters that were determined from particle size distributions and used to estimate parameters and that were estimated from the beercan method. Especially the soil sorptivity and related to it the mean pore size are parameters that were derived from the infiltration test but were neither presented nor discussed.

Ln 155: 'the BEST-slope algorithm (Lassabatere et al., 2006) was considered to estimate the whole set of parameters of the hydraulic conductivity function.' Only derived Ks values are given. If I understand the procedure correctly, then the shape parameters of the water retention curve (except for the parameter hg which is related (inversely proportional) to the mean pore size) were derived from the particle size distributions. Since the particle size distributions do not change over time, do not differ between the row and interrow, it can be assumed that these shape parameters are the same for the different measurement locations and times. However, I think that the authors should include the shape parameters that they used. The assumption that the texture does not vary is a reasonable assumption. But, I think that the authors also need to elaborate on an assumption that they implicitly make, namely that the shape parameters do not change when the structure of the soil change. Can it be assumed for instance that the shape parameters for a crust are the same as those for the loose soil? It can be argued that the shape parameters represent the hydraulic properties of a larger soil volume and that these are hardly affected by a thin surface crust layer. But, then it is important to explain why this assumption can be made for the shape parameters but not for the hydraulic conductivity. Besides the shape parameters, I think it is also crucial to include the estimated sorptivity and related to that the mean pore size parameter. I suppose that these parameters are derived from the data next to the saturated hydraulic conductivity. At first sight, one would expect that the hg is larger in the crusted soil because the pore size is smaller in the crusted soil. But, since the crust is very thin and the wetting front is below the crust, the capillary forces on the wetting front are exerted below the crust. Therefore, I expect that hg does not vary between the different locations and times. But it would be good to have this information. It is also necessary

to explain here why hg of the soil below the crust determines the infiltration rate and not hg of the crust. This is opposite to the control of the saturated conductivity of the crust on the infiltration rate.

Ln 215:'during the last two campaigns': I think the authors refer only to the last campaign on July, 7.

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