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Discussions

Interactive comment on "Monitoring and modelling of soil–plant interactions: the joint use of ERT, sap flow and Eddy Covariance data to characterize the volume of an orange tree root zone" *by* G. Cassiani et al.

Anonymous Referee #2

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GENERAL COMMENTS

Cassiani et al. presented a very nice data set combining several techniques to close the water balance of an irrigated orange tree: ERT, sap flow, eddy covariance data, soil physical data; and to calibrate and validate ERT data under field conditions: TDR, pore water conductivity, petrophysical relationship for changing soil moisture content, ... The aim of the paper is to characterize the volume of the active root zone of the orange tree by coupling a Richards-type model with the experimental data and calibration for

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the root zone.

I appreciate the completeness and quality of the data set, which is far from evident under field conditions. The coupling of data and model in this context is also an important attempt which has often been tried by researchers, but rarely worked out or was very simplified.

Even though I think this work can be very interesting and innovative in this field of research, the authors still have to improve (1) their description of the used methodologies, especially for the modeling part. I did not find any specification on the equations used, especially for the sink term in the Richards equation. Based on the information in the paper, it is very difficult to understand how you can calibrate a volume of root water uptake with a 1-D equation, etc. This really must be explained more systematically. The calibration and validation approach, statistical decision tools, etc. should be discussed. (2) the use of the model outcomes. Next to the active root zone, results on sink term distribution and soil water fluxes based on the coupling of data and model should be given. In addition, I do not understand why the authors limit the paper to a two day period, whereas in the M&M part they speak of an experiment on much longer term... Next to the daily cycle, the dynamics over the growing season are of main interest in this context! (3) the authors explain in detail their setup to measure ET using an eddy-covariance tower, but I do not see where they use these data afterwards in the paper. As explained now, I understood that only the sap flow data are used as a forcing for the model.

DETAILED COMMENTS P 13354 l 8: this is the only place where 4-D inversion appears. If you use this term, please give more information in the M&M part on the type of inversion constraints put on the time dimension, since they can highly influence the result. P 13355 l1 irrigated water that/which is not taken up P 13355 l15-27 This part is a bit unsatisfactory. There are two options, either you do not speak of this at all, since anyhow, you do not aim to test several model types in this paper, if I understand well; either you included some more recent literature and other authors to make this more

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complete and up to date. See recent papers of Valentin Couvreur, Mathieux Javaux, Tiina Roose, ... Recent literature shows for example that there is a mathematical link between the two categories you propose and that they are not that different finally. P13357 l 10-13: As this is the main focus of your paper, I would be more complete on the existing literature applying ERT to characterize root water uptake and root system characterization. (You could deleted some of the general papers before, to gain space if necessary) More specifically, I would also like to see an indication of lab and field studies, since they have different focus and outcomes. Also studies on woody plants and agricultural crops could be differentiated here, because mainly the influence on the petrophysical relationship seems to be different for these two categories. Therefore I suggest adding the papers of e.g. Beff et al. 2012, Amato 2009, Michot 2003, Garré 2011,2012, Cassiani 2012, ... If the groups of Binley and/or Kemna already published some of their work on the effect of roots on soil electrical properties, this would also need to be added here (however, of these two I am not sure if there is already some formal paper). P 1358 l 15 mean leaf area index => over space AND time? P 13358 l22 Ks with falling head permeameter => specify how many replicates, variation of resulting values, ... P 13358 l 26 reflectomeTErs? P 13358 – 13359 Is it possible to make a scheme of the field with the location of all sensors relevant to the data presented in this paper with respect to the tree rows etc? P 13359 l 5 why did you adopt this setup with horizontal and vertical TDRs? How did you install them exactly, especially the horizontal ones? P 13359 l 17 Something that strikes me in the paper is the different time scales of the various data sources: eddy covariance since 2009, sap flow ??, TDR ??, ERT only 2 days in 2013. Can you specify this better in the beginning of the paper and also explain why this is so different. For example, why do you only have two days of ERT data. If you have a specific reason for this, state it more clearly in the objectives of the paper. P 13359-13360 Some things need to be specified more clearly to ensure reproducibility of the research: P 13360 l 2 CSAT3, I suppose. I may be wrong but, to my knowledge, CSAT3 is a sonic and not a gas analyser. I think thus that information about the GA is lacking. Especially, it's important to specify if it's an open path (type LI-COR

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7500) or a closed path (type LI-COR 7000 or 6262). Each system requires specific corrections. On the photograph in Figure 1 I can see the IRGA at intermediate height: that's a LI-COR 7500 open path. Higher, I see a sonic sensor but no IRGA. . . P 13360 l 10 That's a little bit short : you should give more info about flux computation procedure and corrections : how do you cope with high frequency attenuation (in closed path), with rain periods (if open path)? Do you apply the Webb Pearman Leuning (WPL) correction (if open path)? Do you apply a stationarity screening for data filtering? Eddy covariance computation packages cannot be used as black boxes. They must be parameterised in taking the system specificities into account. P 13360 l 16 This is a quite good result that probably validates the whole method. P 13360 l 19 Why the choice for the HPV technique, since it seems to be more and more abandoned by the community due to difficulties to find the 0 flow point. Please specify this. P13361 -13362 For the ERT M&M part add answer to following questions in the text: - what was the material and size of the buried, mini- and stick-electrodes? - how was the borehole made and good electrode contact ensured? How did you minimize hydraulic disturbance due to the vertical holes or if you didn't can you comment on the extent of disturbance of the flow field? - Did you arbitrarily choice the electrode configuration (based on some general characteristics) or you conducted some virtual or real field tests prior to the experiment. If yes, please give some info on that. - If I understand well, you have no measurements between sticks, only along the sticks? - An image of sensitivity distribution of the configuration for a homogeneous medium would be interesting to evaluate the set-up. - Which ERT device did you use for the measurements. - What kind of error model did you use and how did you obtain it? Or did you just put a constant error and if yes, is it the average value of all timeframes and all electrodes? The data quality seems good, especially under complex field conditions, so that's positive. - Specify which constraint was used for the timelapse inversion (time dimension). Figures 5 and 6: I have the impression the color scales are not optimally chosen to see the variability in the 3-D images. I think images in log scale or EC instead of resistivity would show more. Figure 6 is really not readable. Scales are too small. P13363 l5 You refer to fig 6

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here, but it is not clear at this point how you obtained the ‘EC derived total ET’. Please explain. P 13363 l 20-25 I particularly like the fact that you checked the effect of pore water salinity, an parameter that is often neglected, as you state yourself. However, could you specify with which frequency, which method of pore water extraction, where in the field, etc.? P 13364 l 5: Can you detail the experimental protocol? Did you wash the samples several times with the solution to obtain homogeneous pore water concentration? What was the sample size? Figure 7: why don’t you show all data?? I they fall on top of each other, the image should remain readable and the value of the graph would be much higher. . . Could you also show the fit you decided to use to convert rho in WC in the same graph? p 13365 l17 This would be a really interesting case-study indeed. Looking forward to that piece of work. P 13365 l 21 I see the importance and interest of coupling model and data, but I do not know why you have to throw away all the 3-D information to be able to do it. . . In that case, you could simply have put a vertical profile of TDRs and use that data as a source for the model. This would have been cheaper and faster. . . P13366 l 1 I think you should clearly split, both in M&M and in Results, experimental considerations and modeling considerations, in order not to loose the reader. P 13366-13367 Here I was lost and I am still not sure whether I understood correctly. âĂć For example, how can you find a volume of active roots if you use a 1-D model? If it were real 1-D, the transpiration rate (T_act) measured in units of L/T could be directly used and only the depth of the root system would matter. Is this what you did? The authors considered that the average horizontal area per tree (dˆ2, where "d" is the average distance between trees) is larger than the horizontal area the root systems have access to (rˆ2 < dˆ2). Thus the tree water uptake is concentrated in a relatively small volume and the horizontal soil moisture is quite heterogeneous. If at this point the authors still use 1-D simulations, they probably considered no horizontal capillary flow between the regions outside and inside of rˆ2. This has a direct implication on flow boundary condition which has to be taken in a "horizontally smaller" 1-D domain. The volumetric transpiration rate per tree being T_act*dˆ2 (in units of Lˆ3/T), the uptake rate per tree in a 1-D domain of horizontal area rˆ2 has to be T_act*dˆ2/rˆ2.

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In other words, considering that the root system doesn't have access to the water located outside of its area, the smaller the area, the more concentrated the 1D uptake rate, with a ratio dˆ2/rˆ2. I think this is not quite intuitive and not well explained in the manuscript. The hypothesis of no horizontal capillary flow between the outside and inside of the root zone can also be questioned and needs to be clearly specified. âĂć P 13366 l 5 which are the relevant parameters? Further in the text I find the retention curve parameters, but nothing on how you parameterized the sink term. . . In addition, you give no information on how these parameters were obtained. âĂć You state on the one hand that main variations are vertically, but on the other hand several characteristics of the field site make that you can expect 2-D surface heterogeneity: drippers, tree plantation (row-interrow), . . . Did you choose your ERT measurement area so small as to eliminate these horizontal heterogeneities? In p13367 l 10 you use the TDRs to validate some results, but on the other hand here you speak of heterogeneity yourself. Why aren't the TDRs installed in the same measurement area as the ERT with respect to the tree (even another tree would have been possible). P 13368 l 1 you speak of lateral forces.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 13353, 2014.