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Interactive comment on “The benefits of gravimeter observations for modelling water storage changes at the field scale” by B. Creutzfeldt et al.

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Summary:

This MS presents an interesting application of time-lapse gravity data for hydrological model calibration. The content should be of interest to those working in hydrogravity and hydrogeophysics in general. I have listed some technical issues that need to be resolved. Moreover, I would like to encourage the authors to improve structure and clarity of the MS, particularly the sections on model cal/val. Overall, these changes will require moderate revision of the MS prior to publication.

Review Comments:

C821

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1. P2223/introductory review: Electrical resistivity tomography should be mentioned as one alternative. “Cross-borehole geophysics” is not a very precise term. Both cross-borehole radar and cross borehole ERT have been used for soil moisture (e.g. Looms et al., 2008).

2. P2223/L27: There is a strict “footprint rule” (Leiriao et al., 2009): 90% of the gravity signal generated by a thin water layer comes from a circular disk of radius 10 times the vertical distance between the layer and the instrument. So the footprint depends very much on the depth of the hydrological target.

3. P2224/L18-24: This statement may be confusing. Yes, ground-based time-lapse gravity integrates over a footprint of a certain size (s. previous comment), but that footprint is still much smaller than a typical catchment used in hydrological modeling. Typically, such catchments would be several sqkm or 10s of sqkm in size. A ground-based gravity measurement can not give an “integral signal” over such large domains.

4. The paper operates with two models: The model used to derive “non-gravity” WSC estimates (section 2.4) and the macro-scale model described in section 3.1. The purpose of this division is not entirely clear. The gravity measurements could be very valuable in the cal/val of the section 2.4 model, which is operating at a more appropriate scale for comparison with time-lapse gravity data. It would be interesting to see how well certain parameters of the section 2.4 model are determined by the various available datasets (heads, soil moisture, lysimeter fluxes, gravity etc.). How comparable are the results from the section 2.4 model and the section 3.1 model, given that two different modeling approaches were used and the two models operate at different scales?

5. P2231/L13-15: This statement is confusing. The solution of the inverse problem always requires a solution of the forward problem. I guess the authors want to differentiate between a purely geophysical forward simulation / inversion and a hydrogeophysical forward simulation / inversion (s. Ferré et al., 2009 for terminology)

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6. As I read the paper, the macro-scale hydrological model is a spatially lumped 5-storage model. For each time step, one water storage per compartment is computed. Subsequently, the lumped storage estimates are distributed on a fine grid and each grid cell is vertically displaced according to the DTM. This approach is debatable: It is definitely appropriate for the snow storage, but the other storages (particularly ground-water) will show significant lateral redistribution of water in steep terrain. The authors should discuss this and evaluate how critical these assumptions are for their results.

7. The model calibration procedure and the procedure to evaluate the value of the different data types are complex and hard to understand from the MS. The authors should make an attempt to simplify the procedures and to improve the clarity of the presentation. As the focus of the MS is on the value of gravity data, it may be sufficient to show just three cal/val runs: One with all the traditional data, one with the traditional data plus the SG measurements and one with the SG measurements only. The key criterion to determine the value of the SG data would be the width of the ensemble spread in the validation period. The authors claim that inclusion of SG data generally reduces model uncertainty. However, from Fig 5, I gather that some traditional data combinations (e.g. BK3TRIME) produce an equally narrow ensemble spread as the SG calibration runs. I must admit, though, that I am not sure I entirely understood Fig 5, as the discussion in the text and the caption are very brief.

Details:

1. P2222/L10: Explain that this WSC data is from lysimeters
2. P2222/L13: “generalized” may not be the most appropriate term here. Effective?
3. P2223/L1: Generally replace “water storages” by “water storage” when referring to the state variable in general.
4. P2223/L25 ff: I don’t understand the logic. The topography around the sensor is only important because it determines “the vertical distribution of mass change below

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(or above) the sensor". I guess "but" is the wrong conjunction here.

5. P2225/L24: "weather" should be "weathered"
6. P2228/L1-2: "groundwater data" should be "groundwater head"
7. P2228/L18: "Simnek" should be "Simunek"
8. Table 1: "Groundwater" should be "Groundwater head"
9. P2233: "Firstly", "secondly" etc should be "first", "second" etc.
10. P2235/L6: "larger degree of freedom" should be "larger number of degrees of freedom"
11. P2235/L16: "evolution" should be "evaluation"

References:

Ferré, T. et al., 2009. Critical steps for the continuing advancement of hydrogeophysics. EOS, 90(23): 200.

Leiriao, S., He, X., Christiansen, L., Andersen, O.B., Bauer-Gottwein, P., 2009. Calculation of the temporal gravity variation from spatially variable water storage change in soils and aquifers. Journal of Hydrology, 365(3-4): 302-309.

Looms, M.C., Jensen, K.H., Binley, A., Nielsen, L., 2008. Monitoring unsaturated flow and transport using cross-borehole geophysical methods. Vadose Zone Journal, 7(1): 227-237.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/7/C821/2010/hessd-7-C821-2010-supplement.pdf>

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