Comments on the paper HESS 11/4163/2014 entitled "Estimation of heterogeneous aquifer parameters using centralized and decentralized fusion of hydraulic tomographic data from multiple pumping tests" by A.H. Alzraiee and collabs.

The main purpose of the work is to invert hydraulic tomography (duplicating interference pumping tests) by means of linear Kalman filtering. Though groundwater flow inversion did not widely rely on Kalman filtering yet, the major contribution, here, is in the comparison of how tomographic data can be handled. On the one hand, all interference drawdown curves as the responses to all pumping stresses can be gathered into a single set of data, the resulting flowing scenario being inverted in a single step. On the other hand, each pumping scenario can be inverted one at a time. The resulting sets of plausible solutions (ensemble Kalman filtering provides an ensemble of solutions, not a single one) are then fused.

Because ensemble Kalman filtering has to duplicate the so-called states of the system including both the state variables (here, hydraulic interferences) and the parameters (hydraulic conductivity and specific storage capacity), the technique may result in quite intractable computations if interference testing is fully represented by hydraulic head variations in time at specified spatial locations. Interference information is therefore aggregated by calculating zeroth- and first-order temporal moments of the drawdown curves. These moments drastically diminish the volume of information needed to represent the hydrodynamic responses, at the cost however of a rough depiction of the system's transients.

In general, the paper is well written and documented, even though one might find in some places too much (and useless) equations encapsulated in the lines of the main text and chopping the reading. With no doubt, the main topic addressed in the paper is appealing to the HESS readership, and methodologies and results are interesting. In my opinion, this paper could be accepted for publication in HESS after moderate revision. My main general concerns about the content are twofold, though they do not jeopardize tentative publication.

First, the system reduction when passing from a full representation of drawdown interferences to temporal moments is not discussed at all in terms of the incidence on the inverse sought solutions. As mentioned above, one can hear that Kalman filtering on drawdowns may become cumbersome, but temporal moments are also a severe reduction of the information on the system. It would have been interesting to know whether this reduction has some incidence on the inverse solutions (local parameter values, parameter variances and covariances, identification of the spatial structure (covariance) of parameters, etc.). If the problem is too much complicated when handling a complete tomographic exercise, perhaps one could limit the investigation to a single interference testing with one pumped well and drawdown responses at *n* observed wells. Incidentally, in view of the results provided by the authors, the identification of the specific storage capacity seems to be accurate. This parameter is reputed weekly sensitive (and difficult to identify) when dealing with classical head transients. Is this result the consequence of using moments, the zeroth moment pre-evaluating the conductivity field, the second step of inversion relying on the first moments and re-handling the "pre-inversed" conductivity and the specific storage capacity? Stated differently, is the two-step inversion conducive to a better evaluation of the specific storage capacity because the second step of inversion emphasizes its incidence and just adds cosmetics to the conductivity field sought within the first step? If this assertion is right, it could be documented and diminishing the volume of information by using temporal moments would become a true benefit.

The second point is that the paper mainly discusses on THE optimal inverse solution as the ensemble mean of the equiprobable realizations manipulated by the Kalman filtering procedure. It is also discussed on the variance of parameters but the ensemble of solutions (updated parameter fields) is hardly exploited. For example, the ensemble covariance matrix of parameters is not compared, by any means, with the covariance prescribed in the synthetic problem serving as reference. However, if the ensemble of solutions is good, each solution should have a covariance of spherical type as the one prescribed in the reference problem and the ensemble covariance matrix should also be spherical. This feature is simply stemming from the fact that, in theory, the ensemble of solutions, even updated by the Kalman filter, is a set of equiprobable realizations of the same spatially distributed random function. Fortunately, one may conceive that some violations of the theory are acceptable. Up to which point? Another example; the authors state that the parameters of the covariance of the log-conductivity (*Y*) and log- specific storage capacity (*Z*) fields can be introduced in the matrix system updated by the Kalman filtering. What happens when these updated parameters yield a covariance model weakly compatible with the ensemble covariance calculated over the *n* updated realizations of *Y* and *Z*? It is interesting to handle inversion problems in a Bayesian framework provided one explores the benefits of getting multiple solutions instead of a single one. In my opinion, this exploration is not enough developed in the present version of the manuscript.

Editorial suggestions.

1- P. 4171. I do not understand how the moments quoted in Expressions (3) and (4) could correspond to the temporal moment definition in (2). Basically, the steady-state differential equations proposed in (6), (7), and (8) and obtained by Laplace transform or direct temporal integration of the groundwater flow equation would calculate temporal moments obeying to the definition in (2).

2- P. 4173 Expression (9). If the notation $p(\phi, I)$ denotes for the authors the simple prior density function of ϕ (and not a joint density), then (9) is wrong and should be rewritten as $p(\phi / m, I) = p(m / \phi, I)\, p(\phi, I) / p(m, I)$. Notably, if *I* is, as stated by the authors, a "generic" information, it could be dropped in the notations, yielding the classical formulation $p(\phi / m) = p(m / \phi)\, p(\phi) / p(m)$

3- P. 4182 line 10-12. When it is referred to the geometry of the aquifer, the parameters listed in the text and those concealed in Table 2 are not similar. We have a 10 m thick confined aquifer in the text and a 50 m thick in Table 2. In the same vein, Table 2 quotes that the cell size is 10 m along the vertical direction where it should be 50 for a single-layer aquifer of 50 m thickness.

4- P. 4183 line 22 and Fig. 2. I do not see in Fig (2) the 36 observation points mentioned in the text. Fig (2) shows 25 observation points and 4 pumping wells.

5- P. 4183 line 2. I do not understand the sentence on how are calculated the mean and variance of errors on temporal moments.

6- Fig. (3) The map (d) should be labeled as being the "Estimated Z field"

7- P. 4184, Expression (25). I do not understand the definition given to the correlation coefficient between a true and an estimated field. To make it short and simple, let us take two variables *x* and *y* of zero mean. Defining the correlation coefficient as being $r = Cov(x,y) \Big/ \left(\sigma_x^2 \sigma_y^2\right)^{0.5}$, yields $\sum_i x_i y_i \Bigg/ \left(\sum_i x_i^2 \sum_i y_i^2\right)^{0.5}$, I do not see why one observes double sums over indexes *i* and *j* in (25)