Hydrol. Earth Syst. Sci. Discuss., 12, C5823–C5826, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C5823/2015/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Technical Note: Three-dimensional transient groundwater flow due to localized recharge with an arbitrary transient rate in unconfined aquifers" *by* C.-H. Chang et al.

Anonymous Referee #1

Received and published: 24 December 2015

Comments on the paper HESS- entitled "Technical note: Three dimensional transient groundwater flow due to localized recharge with an arbitrary transient rate in unconfined aquifers" by Chang et al.

This note takes the opportunity of re-assembling various investigations on analytical solutions to unconfined groundwater flow and provide us with a unified framework within which: 1- flow is three-dimensional, 2- the domain is of limited size, 3- the boundary conditions are of Robin type, that is, mixing both Neumann and Dirichlet conditions, and 4- to introduce a spatially-distributed source term as infiltration from the surface conditioning groundwater flow.

In its present form, the paper mostly appears as a good piece of algebraic development, and along this line, corresponds typically to a technical note. However, a few concerns make me feel that the writing is not raised to something braking from previous attempts to model unconfined groundwater flow.

First. Contrary to the idea concealed in the paper, I am not convinced at all that analytical solutions are in general the reference tool for concrete case applications. Usually, analytical solutions drastically simplify the problem when concrete applications are faced with complex situations. Todays, concrete application turn toward (eventually simplified) numerical resolutions of groundwater flow simply because these approaches can handle complex geometry, medium heterogeneity, coupling vadose and saturated zone, etc. The point is that analytical solutions are still useful as reference for numerical model and/or to assess the relevance of some second-order mechanisms added to numerical models.

Second. I doubt on the unconfined behavior of the aquifer modeled by the solutions of the authors. For the purpose of simplification, the analytical solution is based on two equations, namely, a diffusion equation corresponding to a confined system plus an additional equation for the free water surface only accounting for fluxes from the recharge over a limited area of the modeled domain. It is obvious that this dichotomy does not represent the continuity of flow between the vadose and the saturated zone that makes the unconfined systems so complicated. The authors would have been well advised to provide us with a comparison between their solution (and its simplifications) and a full three-dimensional resolution of the Richards equation for both the saturated and the vadose zone. The point is not to state that an analytical solution is able to deal with all the physics of flow, rather to show explicitly why the simplifications needed for building an analytical solution are reasonably acceptable.

Third. In association with the concern above, the provided analytical solution is compared with other analytical solutions grounded in the same theoretical framework. This way of doing usually goes with some self-satisfaction attitude because progresses are incremental and never work against the proposed methodology. Again, we would have been better informed if the proposed analytical solution had been faced with a (numerical) three-dimensional resolution of flow. It is now well known that solving a three-dimensional Richards equation with the problem of swapping between the unconfined non-saturated zone and the confined saturated zone is crux to model unconfined aquifers, especially when recharge is evoked as a condition triggering transient flow. Stated differently, one can be still interested in analytical solutions but it is mandatory to know when to apply them, what do they hide, and which (eventually useless) mechanism is overlooked. As an aside comment, we still seek for the usefulness of mixed boundary conditions when the paper only deals with the Dirichlet type of boundary condition.

Four. Technically speaking, the manuscript may appear unclear at some places. The first question raised by reading the mathematical development is the motivation to choose a distance from a well as the reference for building dimensionless coordinates in space. What if no well existed? Why not to build theses dimensionless variables by taking the size (along x and/or y directions) of the domain? Is there any incompatibility by doing so on the emergence of an analytical solution? A second concern is about the sensitivity of the solution to parameters. The authors delineate it as a first order-approximation (finite difference) of the derivatives of the solution with respect to (log) parameters. This calculation is de facto sensitive to the increment δp added to the parameter p when approximating dF/dp as [F(p+δp) – F(p)]/δp. My understanding is that the analytical solution is a double or a triple sum of elementary functions. Derivatives of a sum being sum of derivatives, why not to derive directly the analytical solution with respect to parameters? My first guess is that all the elementary functions enclosed in the solution are differentiable with respect to their parameters, with the consequence that an "exact" sensitivity evaluation could come out by directly differentiating the analytical solution. Notably, the sensitivity analysis performed in the paper is irrelevant. Calculating derivatives with respect to parameters is always local, with the meaning that the differentiation is performed in the vicinity of a prescribed value of the

parameter. Conclusions on model sensitivity are thus local and only valid close to the prescribed values of the parameters. These values are not reported in the paper and a relevant way to analyze the sensitivity would be to duplicate calculations at several points in the parameter space. A third concern is about the appendix which is in my opinion hard to read when it should be limpid. The reader is continuously invited to swap between the writing in the appendix and the equations in the main text. This does not help to understand how the authors technically proceeded for building their analytical solution. My standpoint regarding this feature would be to either remove the appendix, or give it some flesh to document the reader and avoid him back and forth motions in the reading plus hard time to pass from eq. n to eq. n+1.

Five. Even though I am not native speaker of English, I found a text riddled every ten lines with grammatical inconsistencies, awkward phrasings, etc. In any case, the manuscript would deserve pinpoint editing by a professional service. In its present form, the text is not completely clear and editing would probably improve readability.

Finally, I found the paper interesting because the technique concealed in it is undoubtedly sound. The main concern is that the authors missed the target of showing us the added-value of their contribution. They partly kick in touch by comparing their results with those they inherit from. At least, the paper deserves a rigorous editing before publication. Nevertheless, my feeling is still that a relevant paper in a reputed journal such as HESS should argue on the advantages and drawbacks brought by the study. In its present form, the study only brings advantages by flawed comparisons between quite similar approaches. I would recommend to reject the paper in its present form but encourage the authors for a complete re-submission following the philosophy depicted above.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 12247, 2015.