

# ***Interactive comment on “Ross scheme, Newton–Raphson iterative methods and time-stepping strategies for solving the mixed-form of Richards’ equation” by Fadji Hassane Maina and Philippe Ackerer***

## **Anonymous Referee #2**

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The paper compares the efficiency of a combination of two linearisation schemes for the solution of the non-linear Richards’ equation with different time adaptation criteria. The first scheme is a method presented by Ross in 2003, a kind of semi-implicit scheme, calculating the non-linearities with the solution of the last time step. The second scheme is using Newton iterations. The authors first show, that if applied to the water content-formulation of Richards’ equation, the method of Ross is equivalent to the first iteration of a Newton iteration. As the water-content form is only applicable to strictly unsaturated conditions, they use discretisations of the mixed form for the rest of the paper. In the Ross-type scheme, called time-adaptive (though both schemes use

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adaptive time stepping), the authors apply only the first-iteration of a Newton-scheme, calculating the coefficients again with the old solution, and shorten the time step until convergence. In the Newton-iteration scheme they calculate the coefficients with the last iterate until convergence. Thus in the Ross-type scheme the assembly of the linear-equation system to be solved is faster for the second or later iterates. For the adaption of the time step the authors either use an heuristic approach based on the number of Newton iterations (only for the Newton-based scheme), an approach based on an estimation of the truncation error, or a limit on the maximal allowed change of saturation. The different combinations of time-step control and linearisation approach are applied to three different test cases from the literature. The computational costs, measured in a normalized number of solves, are plotted against precision, measured as the deviation of the results from a reference solution calculated with a very fine time step and a given grid size. The authors conclude, that there was no real advantage of the Ross-type scheme.

**General comments:**

The authors address a question, which has been intensively discussed in the last decades. Numerous papers on the best linearisation schemes and time-step adaptation procedures can be found easily in the literature, partially co-authored by one of the authors of this paper, many of them also cited in the paper. Thus the main question is, if the analysis of a very special scheme is a meaningful contribution to the literature and suited for publication in HESS. As there remain a lot of questions to be addressed (see specific comments below), the paper could be accepted only after major revisions. However, I am not convinced that the contributions made by the paper will be significant even after revision.

**Specific comments:**

- equation (1) + (2): As a rigid solid matrix is assumed,  $s_0$  could only describe the

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compressibility of the fluid. As water is nearly incompressible at the pressures occurring in variably saturated soils, the compressibility term is unnecessary and should be dropped.

- line 177: "The time-adaptive algorithm consists of keeping the pressure head constant and changing the time step length." Actually, this formulation is misleading. For each tested time step a new solution for the pressure heads is calculated. Thus they are not kept constant. However, the non-linear parameters are always calculated with the solution from the old time step, corresponding to a semi-implicit scheme. Even the matrix has to be reassembled for each tested time step. Thus only the evaluation of the non-linear functions is avoided. An alternative would be the use of an interpolation table for the hydraulic functions to reduce the computational costs and still keep the accuracy high. The misleading formulation is also used in line 6 of the abstract.
- line 210: I do not understand this formulation.  $\max_i(|\Delta S_{\max}|)$  is the maximum of the actual change, how can it exceed itself? Do you mean exceeds  $(1+\lambda)\Delta S_{\max}$ ?
- line 219-226: Is this important here? If necessary at all, please move it to the introduction
- line 243: replace "superior to" by "larger than"
- line 253: "Implicit standard finite volumes" is not really a precise description. I guess you mean a cell-centred finite volume scheme for the spatial discretisation with an implicit Euler-scheme for the temporal discretisation. Actually, already in chapter 3, equation (15) the discretisation is given. Shouldn't you just refer to that section?
- line 261: "the error based on the maximum change of the state variables between two iterations" would be  $\max_{i,k} |\psi_i^{n+1,k+1} - \psi_i^{n+1,k}|$ . If your formula is correct you

are looking at "the error base on the maximal change of the state variables in the last iteration". This actually is a very bad convergence condition as it can not distinguish between "already converged" and "no convergence at all". However, it is also completely unclear to me, why the time truncation error should be a sensible stopping condition. A reasonable stopping condition is based on the reduction of the non-linear residual compared to the initial non-linear residual. This would really be related to a reduction of the error in the solution of the non-linear equation.

- equation (27) and (28): is it really necessary to write out this equations? Is it not enough to state that relative and absolute error bounds are given?
- line 269-271: Actually, not all possible combinations have been performed. You could also have tested using only the truncation error (if this makes sense).
- line 293-294: as the spatial discretisation error (though not explicitly mentioned) is addressed here: How did you check, that the grid really was fine enough? As you try to get very accurate solutions in time (down to an error of  $10^{-5}$ ), did you really make sure, that the grid is fine enough to produce changes significantly lower than  $10^{-5}$  if further refined?
- line 301-303: As you are using a mixed scheme: why did you not just calculate  $\Delta S_{\max}$  from the saturations? I am also a bit confused about notation. In equation (20)  $S_{\max}$  was a "user-defined maximum saturation change", now it is something calculated from the solution...
- line 306-315: If the mixed form of Richards' equation is used, with a (locally mass-conservative) finite volume discretisation and the linear equations are solved sufficiently accurate, why should there be mass balance at all? It is obvious from the beginning, that this could only hint to errors in your code. Thus the statement in line 314-315 is trivial.

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- line 328-330: I do not understand, why the computational costs of the time-adaptive algorithm are calculated by  $(N_{sol} + N_{param})/2$ . For each iteration step in the iterative scheme you have to calculate the nonlinear parameters and their derivatives, assemble a matrix and solve a linear equation system. For each iteration step in the time-adaptive scheme you have to assemble a matrix and solve a linear equation system, while you have to calculate the non-linear parameters and their derivatives only once for each time step. So the cost reduction depends on the number of iterations necessary (if it is always one iteration, there is no cost reduction at all) and on the relative computational cost of nonlinear parameter evaluation compared to assembly and solution of the linear equation system. Why should this result just in this simple formula?
- figure 4, 6 and 8: for the two saturation-based schemes which allow the highest precision in all three scenarios, there is often a reduced increase of precision with costs at high precision. This could be a hint that the spatial resolution was not high enough and that in this cases the spatial discretisation error became relevant. I would thus not agree with the conclusions in line 368-371.
- figure 4, 6 and 8: there is something strange with all the figures. While in the tables there are only values for four precisions given, there are always six points in the figures for the truncation based algorithms but only four points for all other algorithms. This does not make sense.
- line 342-348: As both stopping criteria for the non-linear iterations are not very adequate and a condition based on the reduction of non-linear defect should be used, I will not comment on the comparison of this non-adequate criteria.
- line 372-375: I do not agree with the last statement. As the saturation based time stepping TA\_S already produced the same precision when a precision of  $10^{-4}$  was demanded, it also had a comparable efficiency with the truncation error based algorithm for this case. The only problem was, that the error was not

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reduced with the higher precision, probably linked to a not fine enough spatial grid. A not mentioned point is, that for the saturation based time step control, there was a linear decrease of the error with the specified precision, whereas this was more erratic for the truncation based time step control.

- line 398-401: I do not understand this statement. After all, the algorithm did compute a solution, so why was the time step too long for reaching convergence? And if it did not reach convergence, how could it calculate the next time steps?
- line 405-407: Actually, the first two scenarios also had a step change of boundary conditions at the beginning and thus a "non-monotonic" change of boundary conditions. Thus this is not really completely different
- line 410: "to avoid a too rough discretisation of the upper boundary conditions": did you make sure that the times at which the boundary condition changed were reached exactly? If you did not do this, you get unnecessarily wrong solutions. This is not a question of the time stepping strategy, but of common sense and not difficult to implement. As I do not know if this was done, I am not going to discuss the further results of test case 3.
- line 447-449: this also means that most of the algorithms are not really suitable for error control. The relation between specified precision and obtained error is not linear for most of the algorithms.
- line 450-452: This is a trivial remark as a locally mass conservative discretisation scheme is used. It would be different for e.g. standard finite-elements as used in Hydrus.
- line 453-456: What should really be implemented is a convergence condition based on a reduction of the non-linear residual.

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- line 457-460: This should be formulated much clearer: The time-adaptive algorithm with the truncation based time-stepping condition did fail to produce accurate results for almost all test cases and converged to the wrong result in the first test cases. Thus it is useless. I would not expect that this will change for 2D or 3D problems. With the saturation-based time-stepping, the time-adaptive algorithm was overall comparable to the standard iterative approach. However, it always was rather costly at high precision, where the time steps are small and thus the number of iterations per time step was also small. Thus the advantage of not calculating the non-linear parameters did not pay off. This also should be similar for 2D and 3D calculations.
- line 462-468: I still do not get, why the time truncation error should be a relevant stopping condition for the non-linear iterations within one time step. Obviously the maximal change of the potential alone is not a reasonable condition, as it is linked to the fluxes and saturation changes via highly non-linear functions.

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