

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2  
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## Comment on hess-2022-153

Anonymous Referee #2

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Referee comment on "A robust upwind mixed hybrid finite element method for transport in variably saturated porous media" by Anis Younes et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-153-RC2>, 2022

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The paper under review presents a new method to obtain an upwind discretization of the advection--diffusion equation, using mixed hybrid finite element. This is a non--trivial problem, as one would like to obtain a stable and monotone method, and because the requirement for a hybrid discretization means that the balance equation needs to be local to each element. This is difficult to reconcile with the need for upwinding so as to obtain a stable method. The author reviews an existing method, and proposes a new variation. The proposed method is validated on two numerical examples.

In this reviewer's opinion, the main advantage of the proposed method would be that it avoids the need to discretize in time to derive the hybrid discretization. As argued by the authors, this enables the use of a higher order, or variable time step, discretization.

However, the paper as it is currently written suffers from several deficiencies, some major, some minor, that I would like to see addressed before publication.

### Specific comments

- **I do not understand the "derivation" of the method that is given in Section 3 (starting on page 12). Why is it allowed, or useful, or even correct, to use a steady state form of the basic mass conservation equation (12) ? This approximation is used to obtain equation (24), and then again in equation (29).**
- **As a consequence of the previous point, why would the proposed method be**

consistent ? And why do the authors expect that the method is stable (or satisfies a maximum principle) ? I understand that the paper, or even the journal, is not a numerical analysis paper, but any discussion shedding some heuristic light in the two issues of consistency and stability would be welcome.

- If no analysis is possible, then the authors should at least include a numerical convergence study, say for example 1, to quantify the accuracy of the proposed method.
- For the two examples discussed in Section 4, the proposed method is compared to the basic hybrid MFE method (that is equation (18) of the paper). But this method is already known to be non-monotone. A more significant comparison would be with the previous upwind variant from the Radu et al. (2011) reference quoted (that is the method described on page 11). This would allow the authors to discuss the possible pros and cons of their proposed method, in terms of accuracy, robustness and versatility. Even though I do not ask lightly that the authors do more numerical experiments, I feel this is really needed before the paper can be published.
- The paper under review has a significant overlap with the recently published paper (see below) by the same authors. The authors should clarify the relationship between both papers.

***A robust fully mixed finite element model for flow and transport in unsaturated fractured porous media,***

Anis Younes, Hussein Hoteit, Rainer Helmig, Marwan Fahs, Advances in Water Resources, Volume 166, 2022, <https://doi.org/10.1016/j.advwatres.2022.104259>.

**See Technical corrections in the attached file.**

Please also note the supplement to this comment:

<https://hess.copernicus.org/preprints/hess-2022-153/hess-2022-153-RC2-supplement.pdf>