

Interactive comment on “Forchheimer flow to a well considering time-dependent critical radius” by Q. Wang et al.

Anonymous Referee #1

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GENERAL COMMENTS The manuscript deals with the modeling of well flow in the subsurface. It suggests a modification of the dual domain (Darcian and non-Darcian) approach developed in the past by adopting a time dependent critical radius, which delimits the two regions. The manuscript is well written and organized, and the topic is of potential interest for the HESS reader. I have not checked all the derivations, and I assume they are correct. The innovation brought by this contribution can be considered as incremental; the method is interesting but it mainly brings some fine-tuning to the otherwise known two-region approach. Thus, I believe that the contribution is more suited to a technical note rather than a full paper. This is also somewhat suggested by the nature of the discussion, the figures and the conclusions; most of the items reproduced in the conclusions seem rather a technical check of the model rather than

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a novel finding.

SPECIFIC COMMENTS

1. It is still unclear to me why a single non-Darcian region should not work well within the entire domain. When the velocity is low, the inertial term is small and the model approximates the Darcian one. This issue is briefly touched in the Introduction, but it is not adequately discussed in my view. This point raises a question mark about the usefulness of the two-region approach, as well as the present contribution.
2. The advantages of the method are not fully clear. What are the benefits in introducing a time variable critical radius? Is the method more accurate? This is not immediate as the conceptual model adopted (two distinct regions with two flow behaviors) is anyway an approximation.
3. The interested reader may want to know when the non-Darcian models are needed: can you please provide some values regarding head gradient or flux, as compared to the hydraulic parameters?
4. Page 14100, line 20: Why Pec is set equal to 10?
5. Equation 2. The solution is given before having discussed the various approximations involved (fully penetrating well, homogeneous and isotropic formation, etc.)
6. Choice for beta in the examples: the value seems rather large to me; please provide a range of realistic values for it. Clearly, a large beta overestimates the effect of nonlinearity in the flow solution.
7. Is the convergence of the method always warranted?
8. Figure 4. The sudden change of slope and regime in the drawdown regime (two-region models) look rather unrealistic, and it indeed reflects the two-region conceptual model that is adopted here. Same for Figure 7. Can the Authors provide a sound physical justification of such behavior, beyond the model adopted?

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9. Line 14110, line 24: the choice for beta is important; please better explain your choice, beyond the Wen&al citation

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 14095, 2013.

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10, C7016–C7018, 2014

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