Hydrol. Earth Syst. Sci. Discuss., 7, C1866–C1872, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C1866/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Simple estimation of fastest preferential contaminant travel times in the unsaturated zone: application to Rainier Mesa and Shoshone Mountain, Nevada" *by* B. A. Ebel and J. R. Nimmo

Anonymous Referee #1

Received and published: 18 August 2010

General comments

This paper, which is well written and organized, aims at estimating the fastest travel time for preferential transport of solutes through the vadose zone. Generally, I would regard this topic to be of high relevance and potential interest for the readers of HESS.

The study focuses on the downward transport of radionuclides through thick unsaturated zones to the aquifer at two areas within the Nevada Test Site (NTS), where

several underground nuclear tests had been executed. Towards their goal, the authors first develop conceptual flow models for the two sites by reviewing and interpreting existing literature sources on lithology and subsurface structures. Then, they present and adopt a simple approach for quantitatively estimating the time required for transport from potential sources to the groundwater at these sites, in which continuous and intermittent supply of the preferential flow systems are differentiated.

This approach, which is termed "Source-Responsive Preferential Flow model" (SRPF) in the present paper, had already been proposed in an earlier publication in WRR by one of the authors (Nimmo, 2007). However, several aspects of this methodology and its application to the two test sites still are at least worth discussing. I would agree with the authors that having a "simple model with minimal site characterisation" to predict contaminant travel times would be highly desirable, but in my opinion the methodology of Nimmo (2007) is questionable in some aspects. Why should there be a central tendency for preferential flow velocities, such that a global "mean maximum velocity" can be defined? Why should a "universal effective rate" be globally valid? Can these "constants" be seen as variables that can be linked to some site-specific properties? How is the broad inter-site variability of subsurface materials and structures taken into account?

The authors of the present study adopt the approach as it stands, without providing critical discussion. Above all, they neither provide a validation of the SRPF models for the two study sites, nor any other discussion of the plausibility of their results, e.g. through comparison with results from other places of the NTS. The estimated travel times are thus probably too approximate for delivering meaningful results. On the other hand, the application of the approach at the two sites does not provide new data or insights that would help to further explore the methodology.

Therefore, I would not recommend publication of the manuscript in its present form, but encourage the authors to resubmit the paper after major revisions.

Specific comments

p. 3882, lines 21-22: “This work expands and clarifies issues from Ebel and Nimmo (2009) while building on that previous work.”
It would be helpful to know which issues from that work are “expanded and clarified”. The manuscript seems to be a short version of the cited work of Ebel and Nimmo (2009), which is publicly available as USGS open-file report *(http://pubs.usgs.gov/of/2009/1175/of2009-1175.pdf)*. The authors could take advantage of this by summarising the site descriptions in Section 2 and referring the readers to the report for all the details where necessary.

p. 3885, lines 6-10: Please include the distances to the saturated zone somewhere in the manuscript, for example in Table 1.

p. 3887, line 17: If much of the precipitation falls as snow (DeMeo et al., 2006), snowmelt would be a major contribution to groundwater recharge at the NTS. How would you account for the effect of snowmelt in your analysis?

p. 3890, line 4: “contaminant transport model”
Later on in the text, you are using the term “travel time model”, which seems more appropriate. As correctly stated in the introduction, in Section 3.2 and in the conclusions, the SRPF model is not a transport model that could predict concentrations or fluxes.

p. 3890, lines 19-22, and p. 3891 lines 9-19: If the travel times through the vitric tuff are short (1-6 years for 500 m), how do you come to the conclusion that preferential flow is “very limited” in this unit? The evidence you present is not so “limited” in my view, but clearly shows that preferential flow through fractures or fingers has to be expected for the vitric tuff, at least as a conservative assumption.

p. 3891 lines 19-25: “Philips et al. ... water itself”
Please consider skipping this passage, which in my opinion is not directly relevant for the study.

p. 3892 lines 4-5: “Table 3 shows the large saturated hydraulic conductivity contrast between the zeolitic tuff matrix and the overlying vitric tuff matrix, with the zeolitic tuff potentially acting as a permeability barrier to vertical unsaturated matrix flow.“
The data in Table 3 do not show a large contrast in hydraulic conductivities between the two formations. The average value would in fact be higher for the vitric tuff, although only two values are given for each formation. Thus, this conclusion is not supported by the data.

p. 3892 lines 20-28: It would be interesting if you could provide some numbers for the age observations.

p. 3894, lines 18-19: “... the properties can be approximated from the similar lithologic units at Rainier Mesa shown in Table 3.”
Given the large variability over several orders of magnitude and the small amount of data presented in Table 3, I strongly doubt that the hydraulic properties at Rainier Mesa are characterised with sufficient certainty. Even less confidence can be put in the transfer of these properties to Shoshone Mountain.

p. 3894, line 24: Where is the “upper carbonate aquifer” (or the lower, respectively) at Rainier Mesa? Is it the perched aquifer mentioned earlier in the text?

p. 3895, lines 3-5: "... (ii) stable matrix flow dominates in the vitric tuff and siliceous rock, with a very small probability of preferential flow in these units.”
The probability for preferential flow in the vitric tuff unit does not appear to be so small, see comments on pages 3890-3892 above. Given the evidence presented in Sections 2.7.2 and 2.9.2, it rather appears that preferential flow cannot be precluded for any of the formations at NTS, except for the siliceous rock at Shoshone Mountain.

p. 3897, lines 23-25: “The spread about the 1:1 line of simulated versus measured fastest V_{max} illustrates the approximate factor-of-ten accuracy of the Nimmo (2007) approach, and indicates definite uncertainty bounds.”
Please explain in more detail what you mean exactly by “approximate factor-of-ten ac-

curacy", i.e. that 85% of the predicted V_{max} fall within these bounds, and how this relates to "definite uncertainty bounds". It might be misleading to use the term uncertainty bounds in this context, as the "true" uncertainty of the SRPF estimates would not only include the uncertainty in estimating V_0 from the original dataset (which is more than 2 orders of magnitude for the entire dataset), but also uncertainties in $D_{transport}$, i_{avg}, and i_0. The term "order-of-magnitude agreement" used in Nimmo (2007) might be less misleading.

p. 3899, lines 12-13: The estimated value for V_0 seems absolutely crucial for the results of the SRPF approach. Why did you choose to take the geometric mean of the measured V_{max}?
As the objective is to estimate the fastest (worst case) travel time of a solute, would it not be more appropriate to take the maximum of observed velocities? Of the values given in Nimmo (2007), this would be 480 m d^{-1}, which were observed for a very similar lithology, namely fractured welded tuff at Yucca Mountain, located some 50 km to the southwest. Prediction with the SRPF approach for the case of continuous supply assuming 13 m d^{-1} would drastically underestimate this value and that by a factor of 36.

p. 3900. lines 10-12: "To apply the model in such cases, a universal effective rate, i_0 [L T^{-1}], is hypothesized."
Is there any physical interpretation of i_0 that would allow assessing its value with independent observations? Or is it just a fitting parameter to find some possible mathematical description of the observations by curve-fitting?

p. 3908, lines 1-2: "... demonstrates its role in the suite of models used to ask and answer difficult questions ... "
The authors do not provide enough evidence (validation data, comparison to similar sites) that the SRPF approach can give plausible answers to the question of fastest travel times at the two test sites.

p. 3908, lines 13-14: "... these data do not rule out preferential radionuclide transport through the unsaturated zone at the timescales estimated in this study."
The reverse is also true, because this also means that there is no evidence for preferential radionuclide transport through the unsaturated zone. It seems quite astonishing that there is nothing to be found in the aquifer below the U12e and U12n tunnel working points, for which the authors show that significant exposure to radionuclides exists since several decades, argue that continuous water supply is given and the fastest travel time is estimated to be around one month (more exactly, somewhere between 3 and 300 days).
In summary, there is no validation of the travel time estimates presented in this study; the evidence presented in Section 5.2 rather leads to the conclusion that the travel time estimates are far from plausible.

p. 3909, lines 16-22: "The SRPF model has the strong advantage of not requiring site-specific unsaturated hydraulic properties ... "
At the same time, a great disadvantage of the SRPF approach is that it is not possible to incorporate quantitative site-specific data, if available, in order to achieve greater accuracy.
More traditional unsaturated-zone flow models could be parameterised following the same philosophy as was done for the SRPF model, i.e. using "universal" values for hydraulic parameters determined with some "global" dataset. But with these models, it would at least be possible to decrease the uncertainty of the results by incorporating site-specific data.
In either case, tests against direct observations are essential to demonstrate the value of a theory or a model and to judge its accuracy.

Technical corrections

p. 3908, lines 11: "... definite detection limits (about 330 pCi L^{-1}) ... "
In Table 3, values of 5.3 or 8.1 pCi L^{-1} are listed. There must be some mistake.

p. 3908, line 11 + Table 2: Please consider using SI units (Bequerel instead of Curie).

p. 3910, line 22: "... must be occur ..." – Please check wording.

p. 3929, Fig. 5: This is a slightly modified version of Fig. 2 in Nimmo (2007). It should be further modified to cover the entire range of observed maximum transport velocities reported in that WRR paper (up to 480 m d^{-1}). Please remember that the material must be cited appropriately according to the AGU Usage Permissions.

References

DeMeo, G., Flint, A.L., Laczniak, R.J. and Nylund, W.E., 2006. Micrometeorological and Soil Data for Calculating Evapotranspiration for Rainier Mesa, Nevada Test Site, Nevada 2002-05. USGS Open-File Report 2006-1312. DOI: 10.2172/896768

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 3879, 2010.