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Hydrology and Earth System Sciences Discussions



# *Interactive comment on* "Detection of contaminant plumes released from landfills" *by* N. B. Yenigül et al.

**Anonymous Referee #2**

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GENERAL COMMENTS:

This paper presents an analytical model that aims to predict the probability of a contaminant leak from a landfill being detected by a downgradient monitoring network. The predicted probabilities are compared with simulations performed in homogeneous and heterogeneous aquifers using a random walk particle tracking approach. For reasons outlined below, I do not think that this paper makes a significant contribution to the field.

I have read through the two anonymous reviews posted for this manuscript, and I concur with several of their observations. I will first address the points that have not yet been discussed fully by the other referees, and then restate some of the points covered by the other reviews and with which I concur.

SPECIFIC COMMENTS:

1. The analytical model presented by the authors works well for a homogeneous case, and this is not surprising. The difference between the analytical and numerical plume shapes is puzzling, and I tend to agree with Anonymous Reviewer #3 that this is related to the “grid averaging” of the analytical solution, for which I do not see a good reason.

2. For the heterogeneous case, the analytical solution models the plume as one being transported through a homogeneous medium with a higher effective dispersion coefficient. The basic assumption that is implicit to this approach is that, if the plume is not detected, it is because the heterogeneity contributes to additional mixing (i.e. dilution), which lowers the concentration at the monitoring wells to below the detection limit. The authors clearly support this point, because they state on page 822 that a “worst case assumption for homogenization of a heterogeneous medium might be using a large hydraulic conductivity value (although still homogeneous),” because this would “result in over estimation of the velocity and extent of plume.” This statement is closely tied to the authors’ use of a homogeneous plume to describe a heterogeneous one. If the extent of a plume (defined as the locations beyond which the concentration is always below the detection limit) in a homogeneous medium is larger, then the concentration must be lower, leading to a lower chance of detection because concentrations quickly fall below the detection limits. This is again supported by the statement, on p. 837 that “The analytical model using effective (macro) dispersivities computes the mean concentration distribution, which corresponds to smoother and relatively wider plumes, consequently a much more diluted plume”

This is not at all consistent with the physical reality of plumes in heterogeneous media. If the plume width is defined as the distance between the outermost edges of the plume (at a given concentration cutoff), then this metric in not at all useful for calculating plume detection probabilities, because, unlike in a homogeneous medium, the concentration will not necessarily decrease monotonically away from the centerline of the plume. There may be areas with concentrations well below the detection limit “within” the plume, in the sense as it is defined by the authors.

In other words, dilution is not the primary cause of a monitoring network failing to detect a leak in heterogeneous formations. Concentration distributions in heterogeneous media are not necessarily any lower than those in homogeneous media. Instead, in heterogeneous media, preferential flowpaths direct most of the plume into relatively narrow meandering paths with high concentrations, and the probability of detection has more to do with the location of the monitoring wells relative to the locations of the high permeability flowlines than it does with any additional dilution that takes place as a result of heterogeneity.

Therefore, the analytical solution is completely misrepresenting the physical process that leads to most failures to detect leaks in heterogeneous formations. The in-depth discussion of absolute versus relative dispersion presented by Referee #1 is very relevant here as well.

3. Especially in the case of heterogeneous aquifers, the appropriateness of approximating the size of a well as one grid cell (p.833) needs to be addressed in light of the meandering flowpath discussion presented above.

4. I completely concur with Referee #1 that the standard particle tracking algorithm used by the authors does not warrant the long discussion presented in the manuscript. Including equations 7 and 8 and associated discussion would be sufficient. In addition, the discussion related to the number of particles used is also self-evident, well-established, and not needed (p. 834)

TECHNICAL CORRECTIONS:

p. 824, line 7 replace “block-cantered” with “block-centered”

p. 827, line 26 replace “stationary” with “steady-state”

p. 831, line 18 replace “above a given threshold” with “above a given concentration threshold”

p. 835, lines 9-10 explain why “the plume edges are not as sharply defined as in the analytical model.”

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 819, 2006.