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Interactive comment on "A decision analysis approach for optimal groundwater monitoring system design under uncertainty" *by* N. B. Yenigül et al.

N. B. Yenigül et al.

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Authors' response to the comments of Anonymous Referee #2

We very much appreciate the valuable comments of the anonymous Referee#2, who has made a thorough review of our manuscript and raised perceptive points. We have made good use of his comments and suggestions to clarify the indistinct points and to enhance the manuscript. The following is a list with the referee's comments and our response to each point.

-General Comments

This paper presents a methodology for the evaluation of groundwater monitoring system designs under uncertainty with respect to their appropriateness to detect a groundwater contamination that might be effected by a leaking landfill. A number of 171 mon-

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itoring system designs are investigated. The designs are in fact monitoring fences i.e. the individual monitoring wells are positioned in a row perpendicular to the general flow direction along a control plane. The designs under investigation differ in distance to the landfill and spacing between the individual wells. The evaluation considers (i) costs of the monitoring system and (ii) risk costs whereas the latter are quantified in terms of remediation costs required for clean-up of the groundwater volume that has been contaminated either until detection of the contamination or, in case of failure (no detection), until the end of the monitoring period (30 years).

Uncertainty in spatial distribution of hydraulic conductivity as well as with respect to the location of the leakage (assumed to be continuous point source) is considered using a stochastic framework (Monte Carlo simulations). Despite some minor issues the manuscript is generally well organized and well written. However, as discussed below, the methodology developed and applied here does not properly consider relevant issues of contaminant spreading and cost calculation. As a consequence, the results obtained and the conclusions drawn are arguable. I suggest a major revision of the manuscript, including additional detail and clarification regarding technical issues of modeling and careful review and response to the comments below.

Specific comments

Title

-The title suggests that a methodological approach for the design of an optimal monitoring systems will be presented. In fact, however, only a number of predefined monitoring systems are compared. A real optimisation is not performed.

The title suggests a decision analysis approach for the design of an optimal monitoring design. As described in this manuscript as well as in other studies (e.g. Freeze, R. A., and S. M. Gorelick (1999), Freeze et al. (1990), Gorelick et al. (1993), Massmann et al. (1991), Freeze et al. (1992), and etc.) decision analysis involves the determination of a best (optimal) alternative (that is the best values for a set of decision variables) from a discrete set of specific alternatives. Therefore the optimal monitoring system corresponds to the best monitoring system among the predefined 177 potential monitoring systems. We believe that the use of the word “optimal” is appropriate within this context and it should not cause confusion with respect to the methodology used as it means literally “the best, most favourable, best possible” and hence does not necessarily imply the performance of a real optimization in this case.

Modelling

-Some modelling issues should be further elaborated/reconsidered: Ţ Unfortunately no figure is provided that exemplifies the shape of the simulated plumes. Anyway, let’s make the following exercise: The normalized expected contaminated areas, as presented in Figure 4, are in the order of 0.2 ů ndfs (E(Ad) = ’ 0.2 for ndfs = 1, E(Ad) = ’ 0.38 for ndfs = 1.8). Taking into account the normalization (by 10,000 m2) one gets E(Ad) ’ 2,000 m2 ů ndfs. With d = ndfs ů 100 m the average plume width estimates to E(Ad)/d ’ 20 m. To me, this seems to be a unreasonable large value taking into consideration that it originates from a “point source” the width of which is 2 m. And it is an indication that dispersion is somehow overestimated. This, in turn, is what one could be expected, as “microscale” dispersivities used in the study (aL = 0.5 m, aT = 0.05 m) are quite large. Transverse porescale dispersivities are in order of (tens of) millimeters rather than centimeters (see for example Rahman et al. (2005) and Newman et al. (2005) and Cirpka et al. (2005)). Within this context, I’m quite sure that one of the main outcomes of this study . . . “The results of the extensive numerical experiments show that the reliability of monitoring systems increases with distance from the contaminant source. Since plumes begin with a small size and spread out as they migrate away from the source, systems composed of few wells are more likely to detect the contaminant plumes when they are placed away from the contaminant source. For a given distance away from the contaminant source the probability of detection increases as the number of the monitoring wells increase but once 100% of reliability is achieved by a given monitoring system additional wells would not be cost effective for improving the system reliability. The widely used 3 well monitoring system (minimum regulatory requirement) does not reach 100% reliability for any of the cases investigated in the presented study." (Page 48, line 25 to Page 49, line 8).

. . . is not representative for real aquifers. I suggest to expand the lower range of transverse dispersivity to (at least) aT = 1 mm. Furthermore, the influence of the assumptions made for contaminant spreading should be quantified not only with respect to Pd and E(Ad) but with respect to monitoring and remediation cost (see also comment on cost calculation further below).

We have carried out an extensive literature study, including the studies mentioned by the Anonymous Referee #2, for the study presented. We have seen that the dispersivities of the medium, aL and aT depend on many factors including the scale of measurements, numerical properties, model dimensions, and possibly also time and space. We have also observed a very wide range of dispersivity values (on the order of millimetre to several meters) and the ratio of transverse to longitudinal dispersivity is in general less than 1 and could be considered a constant. (See. e.g Bear, (1972), Gelhar (1986), Loaiciga, (1989), Meyer et al., (1994), Vomvoris and Gelhar (1990), Storck et al (1997), Hudak (2002), Rahman et al. (2005), Cirpka et al. (2005), and Shulze-Makuch (2005) etc). Particularly the very recent study by Schulze-Makuch (2005) provides a comprehensive review on longitudinal dispersivity data and implications for scaling behaviour. He compiled longitudinal dispersivity data from 109 different authors for different types of geological media. During our choice for the range of dispersivity values for our analysis we have considered particularly the data presented in the study of Schulze-Makuch (2005) besides the others. On the other hand to prevent the overestimation (as mentioned by Referee # 2) in determination of plume width we have used a much smaller transverse dispersivity (aT=5 cm) which is much smaller than those used in the previous studies focusing on monitoring design problem as well. Some examples of these values are : 1. Loaiciga, (1989)aT=2.8 m. This value determined based on the field data for the Butler County Landfill in Ohio 2. Smedt and Bronders (1989) aT=11 cm. This value is determined based on the field data for the Hooge Maey waste disposal site in Belgium. 3. Meyer et al. (1994) aT=20 cm and 60 cm 4. Ribeiro (2000) aT=50 cm in This value determined from the field data for the Vale de Milhacos waste disposals in Lisbon 5. Hudak (2002) aT= 10 cm

Finally, knowing that: (1) there is ongoing research in this field, (2) final agreement has not yet been reached and (3) the values we have used are within the range recognized and used by many researchers in the field we believe that that the values we used are applicable for a wide range of sites and gives us support to a wider applicability of our methodology, although we did not consider values in the order of millimetres. Hence we do not agree with Referee #2 that the outcome of our study, which is on Page 48, line 25 to Page 49, line 8 in the old version of the manuscript, is not representative for many aquifers. However we also are aware that the results with respect to reliability and contaminated area will be much lower for aquifer conditions where advective flow is more likely dominant. This conclusion can be deduced straightforward based on the sensitivity analysis in Section 4.4.1 and from the analysis of previous studies mentioned in the same section. We hope that our discussion is persuasive to explain why we did not expand the lower range of transverse dispersivity to (at least) aT=1 mm in the revised manuscript.

We have added Table 2 and Table 3 to quantify the effect of the variability in hydraulic conductivity, the effect of the dispersivity with respect to monitoring and remediation cost in addition to their influence on Pd and E(Ad) that have been presented in the previous manuscript. We hope that these new tables and the corresponding discussions included in the revised manuscript are satisfactory.

The cited references in our response that are not included in the manuscript are:

Loaiciga,H.A., 1989, An optimization approach for groundwater quality monitoring network design. Water Resources Research, 25, 1771-1782. Vomvoris, E.G., and Gelhar, L.W., 1990, Stochastic Analysis of the concentration variability in a three-dimensional heterogeneous aquifer. Water Resources Research, 26, 2591-2602.

Shulze-Makuch, D., 2005, Longitidunal dispersivity data and implications for scaling behaviour. Groundwater, 43 (3), 443-456.

De Smedt, F., and Bronders, J. , 1989, Study of groundwater pollution around waste disposal sites with a simulation model. Groundwater Contamination: Use of models in decision-making, G.Jousma et al. (eds), pp. 619-627. Ribeiro, L., 2000, The effect of an heteregenous hydraulic conductivity field on the spread of a contaminant plume in a porous aquifer: A case study in Portugal. Groundwater updates, K.Sato and Y. Iwasa (eds.), pp.153-158.

- Another major shortcoming is the resolution of the model. If a monitoring well is represented by one model cell (which is a common assumption also made in other studies) a sufficiently fine model grid is required to simulate sampling realistically. The authors should quantify how the size of the one-cell-monitoring wells does influence the detection. The discussion should include the relationship between model cell size, averaging within the cell, and magnitude of the threshold value of detection. Another point: I would have expected a spacing of the monitoring wells which is a multiple of the model discretization (= 2m). According to Table 1, however, the spacing seems not to be a discretized but a continuous variable.

We are not sure if we understand the comment correctly, however we believe that the arguments given in Section 3.1 already answer the questions related to grid size and the determination of concentration and the arguments in Section 3.3 answer the question with respect to the relationship between the model size, and the magnitude of the threshold value of detection. Since a random walk particle model is used to model the transport phenomenon the concentration in a grid cell is determined via the mass of number of particles in the grid cell at a time t. (As mentioned in the manuscript for further information related to the simulation model and the assumptions considered, the reader is referred to Yenigul et al. 2005). Use of a sufficient number of particles will minimize, or may even overcome, errors in determination of concentration values in the simulations. We have tested the concentration distribution in homogenous case with the corresponding analytical solutions and the results were satisfactory with the corresponding number of particles and grid block size. These tests were presented as poster presentation in Symposium on Soil and Water, in Zeist, The Netherlands (June 2 and 3, 2004 (Yenigul et al. 2004, Detection of Contaminant Plumes from Landfills–Assessment of Simulations by Analytical Method). However, there is no analytical solution for the heterogeneous case. On the other hand, the well diameter, which is taken 2 by 2m (a cell size), is an acceptable representation of a well in the domain to simulate sampling realistically because the actual well diameter is about 0 .5 m and the filter and the casing around the well may have a thickness of 1 to 1.5 m.

-Cost calculation I do not agree that a net present value calculation i.e. discounting of future expenditures (as suggested by equation 1) is not important in the presented study (as mentioned on page 34, line 9-17). In case of detecting the contaminant plume, the time of detection will greatly vary with normalized distance of the monitoring system to the source (ndfs).

Hence, cost-driving parameters will also vary, namely Ţ the required monitoring i.e. sampling period, Ţ the point in time when remediation cost occur

I don’t see why, under these circumstances, simple adding of C and R (equation 3) shall give the same outcome as a dynamic cost calculation. Moreover, the applied cost model utilising a unit installation and sampling cost appears to be inappropriate as sampling cost are not only a function of the number of wells but will differ with required sampling period. I suggest to separately consider investment and operation and maintenance cost and to include discounting in economic assessment.

The cost model is adjusted to include the sampling and installation costs separately. Please see Equation 4 in the revised manuscript. A net present value calculation i.e. discounting of future expenditures (as suggested by equation 1) is not important in

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the presented study because with a total number of sampling and associated costs and plume area at detection and required remediation cost the influence of time can be included in the cost estimation and this estimation is representative for a dynamic cost calculation under the assumption mentioned on page 34, line 9-17. Moreover cost analysis is indented to be a tool in portraying the solution of the monitoring problem but it is not the main concern of the study. Therefore we did not want to complicate the financial computations further. In any case, the responsible authorities may consider an insurance, or, even more modern, a hedging policy using real option theory to deal with this problem. Hence in this study including the discount rate is barely significant from the point of optimal system determined by the methodology presented and has very little influence on the results from the conceptual point of view.

Technical Corrections 1. Page 31, line 2: “Moreover, . . . lower computational effort . . . ” Lower effort compared to what?

We have rewritten the second and third paragraph on Page 30 in the old version of the manuscript to address the advantages and shortcomings of decision analysis versus optimization methods in a better way. We believe that this point is also more clear in the revised manuscript. Please see page 3 and 4 in the revised manuscript.

2. Page 31, line 11: Section title What is presented here is more than just a model. I would suggest a title like “Methodology”.

The section title has been changed to methodology please see page 5 line 27 in the revised manuscript. 3. Page 32, line 2-4: It’s not clear whether a single cell or multiple cells do represent the location(s) of leakage.

The lines 13 and 14 on page 6 are added in the revised manuscript to clarify this point.

4. Page 36, equations 8 and 9: Two different notations are used for the expected contaminated areas: E(Af(j)) and EA(fj).

It has been corrected . Please see Equations 8 and 9 in the revised manuscript.

5. Page 36, equation 9: Legend below equations is wrong, add a “no” to the legend for EA(fj).

It has been corrected. Please see Equation 8 in the revised manuscript.

6. Page 39, line 17-20: “The contaminant leak is . . . for each Monte Carlo run. Here you clarify that a point source i.e. a single cell represents the location(s) of leakage. However, this clarification should be made already in section 2.1 (page 32,see comment 3.)

Please see the response to the Comment no 3.

7. Page 41, Line 14-15: “However, . . . the common practice of 3-well monitoring system . . . ” I would not agree that a 3-well system is (in general) common practice. It might be true in some countries but not in any country.

We have considered the 3-well monitoring systems as common practice since, as mentioned in the manuscript, USEPA requires minimum 3- monitoring wells in the downgradient of a landfill to monitor the groundwater quality. USEPA regulations are considered in many countries and even have been guide in preparation of European Landfill Directive for landfills. We are certainly aware that there were and there will be the cases where more number of wells might be used depending on the available budgets, and circumstances. But in this context we are not entirely sure what we have to revise in the manuscript with respect to this comment.

8. Page 41, Line 16-28: In my opinion, this paragraph does belong to the methodology section (2.1, comp. Page 32, line 19-27) rather than to the discussion of the results.

We wanted to remind the reader about the definition of contaminated area given detection or no detection by the mentioned paragraph. Considering the suggestion of the Anonymous Referee #2 we have condensed the paragraph into one sentence in the revised manuscript. Please see page 14 line 13-15 in revised manuscript.

9. Page 42, Line 22/23: “. . . since the detected plumes reaches stationarity as

it moves further away from the source.” I wouldn’t interrelate “stationarity” and “as it further moves”. Stationarity is commonly used when a plume has reached its maximum extent.

What we meant here by “stationary” is that the size of the detected plumes do not change stay more or less the same. To avoid the possible confusion concerning the common use of the word we replaced the word “stationary” with “uniform”. Please see page 15 line 7 in the revised manuscript.

Cited Literature Ţ Rahman, A., Jose, S.C., Nowak, W., and O.A. Cirpka. 2004. “Experiments on Vertical Transverse Mixing in a Large-Scale Heterogeneous Model Aquifer” Journal of Contaminant Hydrolology, 80, 130-148. Ţ Newman, M., K. Hatfield, J. Hayworth, P.S.C. Rao, and T. Stauffer. 2005. "A hybrid method for inverse characterization of subsurface contaminant flux" Journal of Contaminant Hydrology, 81(1-4), 34-62. Ţ Cirpka, O.A., Olsson, Å., Ju, Q., Rahman, A., and P. Grathwohl. 2005. “Determination of Transverse Dispersion Coefficients from Reactive Plume Lengths” Journal of Groundwater, doi:10.1111/j.1745-6584.2005.00124.x.

Interactive comment on Hydrology and Earth System Sciences Discussions, 3, 27, 2006.