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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.

Anonymous Referee #3

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General comments

This manuscript presents a decision analysis approach to determine the optimal design of a groundwater monitoring system under uncertainty, a topic that is certainly of interest to readers of HESS.

The authors do not fully succeed to convey the novelty and contribution of their work. Some earlier works on the use of decision analysis for groundwater detection monitoring system design are cited, but it is not clearly addressed how this manuscript builds on previous works, or in what way it differs or adds new ideas/concepts.

In the abstract and introduction part of the manuscript, the authors focus on the multi-objective nature of the problem, which involves maximizing the reliability (detection probability), minimizing the contaminated area, and minimizing the total cost of the monitoring system. However, in the analysis and presentation of the results the determination of an optimal design is based on a single objective function (equation 9) that reflects the expected total cost. The authors fail to make a clear connection between the required reliability and the minimal cost of the system, and do not adequately address how a decision maker could make use of the results. For example, is there a pre-specified reliability that should always be met? The reliability is incorporated in the objective function through a penalty function. In the example shown, the minimal cost coincides with a reliability of 100%, but in the evaluation of different capital and monitoring costs (see Table 2), the optimal sampling design does not always coincide with a reliability of 100%. In other hydrogeological settings, the minimal cost optimal design will also not always (very likely not) coincide with a 100% reliable system. The authors should better address these issues.

Some results are not interpreted correctly, or in sufficient detail. An analysis of the effect of the variability in hydraulic conductivity (variance and correlation length of lnK) on the optimal design would be more informative than only showing the effect (of lnK variance) on the size of the plume and the reliability.

Specific comments

Page 30, second and third paragraph:

The advantages and shortcomings of decision analysis vs optimization methods could be better addressed. One drawback of decision analysis is the limitation to the assessment of a predefined suite of design alternatives. For the problem at hand (and the objective function adopted, see paragraph below), a genetic algorithm type of optimization could potentially be used to determine the optimal sampling design, without the limitations of a restricted decision space. Freeze and Gorelick (1999) provide an excellent overview on the convergence of stochastic optimization and decision analysis.

Freeze, R. A., and S. M. Gorelick (1999). Convergence of stochastic optimization and decision analysis in the engineering design of aquifer remediation, Ground Water, 37(6), 934-954.

Also, the authors state that an optimal solution for the detection monitoring system cannot be determined based solely on objective function values. However, in the following analysis and discussion of results, this is exactly what has been done? The optimal design is based solely on the values of the objective function defined in equation 9, which comes down to minimizing the expected total cost. In the example shown, the minimal cost coincides with a reliability of 100%, but in the evaluation of different capital and monitoring costs (Table 2), the optimal sampling design does not always coincide with a reliability of 100%. For example, when the optimal design involves an interwell spacing of 0.2 and a standardized distance from the landfill of 0.7, the reliability of the system is approximately 0.85. Should the actual reliability be taken into account when making a decision about the design, or should the optimal design be based solely on the expected cost, irrespective of the actual reliability of the system (although it incorporated in the penalty term)?

Page 30-31:

The authors do not clearly state what is the novelty or contribution of their work. The authors cite some earlier works on the use of decision analysis for groundwater detection monitoring system design, but no further details are provided about these works. Also, the authors do not clearly address how this manuscript builds on previous works, or in what way it differs or adds new ideas/concepts.

Page 31, line 16 (reference to Figure 1):

In Figure 1, should the box with ‘Monitoring system reliability model checks whether the contaminant plume is detected Ě’ not be before the box with ‘Determine the plume size cumulative distributions given detection Ě’?

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Interactive Comment

Page 31, line 18-20: ‘A simulation-based model Ĕ coupled with Ĕ is used to determine the optimal monitoring system.’

Not clear what is meant here by a ‘simulation-based model’. Also, I suggest replacing ‘is used to determine the optimal monitoring system’ by something in the line of ‘is used to simulate the contaminant plumes’.

Page 32, line 21-22: ‘Therefore, rather than producing a single contaminant plume every time a system detects a plume Ĕ’

Bad phrasing. Every time a system detects a plume only one plume size is determined (realizations are looked at individually), but the size may differ for different plumes detected by the same design.

Page 36, equation 7: Should be double integral and integration limits are not correct. I presume that in the x-direction the integration limits are the right edge of the landfill and the right boundary of the field (x = 500 m). However, in the y-direction, it is not clear, neither here nor later in the manuscript, if the integration limits are taken as the boundaries of the field (i.e., y = 0 and 400 m), or as the edges of the landfill (i.e., y = 150 and 250 m). This actually has a consequence on the interpretation of the results.

Page 36, equation 8 and line 6-7:

Inconsistency in symbols E(Ad(j)) - EAd(j). Line 7: Should read ‘expected contaminated area’ instead of ‘contaminated area’.

Page 36, equation 9 (line 10):

Last term of equation 9 should be ‘expected cost given failure’.

Page 36, line 12:

Definition of ‘best’ system is not fully correct. The best system is the system that minimizes the total expected costs (number of monitoring wells is reflected in the cost term and detection probability in the penalty term). Minimizing the cost does not necessarily imply the smallest number of wells, and, even though for the example case the minimal cost system has a reliability of 100%, the best system as defined in the manuscript does not warranty a maximum reliability.

Page 38, line 1-3: ‘Ĕ involve major assumptions on the dispersivity of the medium.’

Not clear what is meant here. What major assumptions? Should an ideal monitoring design not account for the uncertainty in the dispersivity of the medium?

Page 38, line 8-28 and Figure 2:

At first reading it is not clear from the text and the figure that each monitoring system (i.e., with a fixed number of monitoring wells) is evaluated at different distances from the landfill (the figure might suggest that each monitoring system is evaluated only once at a different distance from the landfill). This becomes more clear later in the analysis, but it would help to clarify this here (and in the caption of Figure 2).

Page 40, line 14-15:

Definition of best design system. See earlier comment.

Page 41, line 10-14: ‘The analysis showed that Ĕ with nws greater than 0.08 there is a ndfs at which 100% reliability is achieved.’

Figure 3 clearly indicates that only for nws  0.10 a reliability level of 100% is obtained at a certain distance from the landfill. For larger nws values, at some distance from the landfill the reliability seems to level off with increasing ndfs and there seems to be a threshold reliability level. For increasing nws, the threshold reliability level gradually decreases and a larger ndfs is required to approach the threshold level. This means that, for nws $>$ 0.10, there will always be plumes that remain undetected, due to preferential flow paths that bypass the monitoring wells, or large spreading of the plume and consequent dilution below the detection limit. It follows that this phenomenon is observed more frequently with increasing nws.

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Page 42, line 8-11:

Even though the effect is less pronounced than for the ndfs, there is some noticeable effect of the nws on the expected size of the plume, and this should be addressed. When nws increases, one can expect that the size (width) of most of the detected plumes gets larger, as smaller (less wide) plumes will go undetected more easily. This could explain the increase in expected plume size with increasing interwell distance.

Page 42, line 12-16:

Should E(Af) not show a decreasing trend with increasing ndfs and nws? For example, for a given interwell spacing, plumes that go undetected passed a monitoring system close to the landfill will have more chance to spread out over a larger area than those that go undetected passed a monitoring system located far away from the landfill (because the width of an undetected plume is smaller than the interwell distance at the time it passes a monitoring system). It also seems logical that a larger interwell spacing allows wider plumes to go undetected?

Page 42, line 19-27:

Wrong definition of coefficient of variation, and misinterpretation of the results. Firstly, the coefficient of variation represents the ratio of the standard deviation to the mean, and not the other way around. Secondly, the high values for the coefficient of variation close to the landfill are due to the high standard deviation relative to the mean plume size, mainly caused by the variation in release location. The effect of the unknown release relation relative to the mean size of the plume diminishes when moving further away from the landfill. Thirdly, why would the plumes become stationary further away from the landfill? The variability encountered does not change, nor does the background hydraulic gradient.

Page 45, Section 4.4.1:

This paragraph presents straightforward results and has little added value. Can be condensed into 2-3 sentences.

Page 46, Section 4.4.2:

Figures 12 and 13 show that, although the (expected) plume size increases with increasing variance of the lnK field (larger spreading of the plume), the reliability or detection probability decreases. This can be attributed to several factors, some of which may result from the chosen setup. Firstly, the effect of preferential flow paths, i.e., transport may be concentrated in high conductivity zones that bypass the monitoring locations. Second, larger spreading of the plume causes dilution of the concentration. It is not clear whether for the determination of the plume area (through a binary transformation of the concentration field) the same threshold is used as the detection limit of the monitoring wells. If this is not the case, this could partly explain the results. Thirdly, the lower detection probability with increasing variability in hydraulic conductivity is also likely caused by a larger number of plumes that migrates out (north or south) of the region covered by monitoring wells. The first factor is likely most pronounced close to the landfill, whereas the effect of the other two factors likely increases with the distance from the landfill. The authors should better address these issues.

Page 46, Section 4.4.2, line 18-20:

More interesting would be to see if and how the optimal pumping strategy changes with respect to changes in the variability in hydraulic conductivity.

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