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Interactive Comment

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 #3

We would like to thank the anonymous Referee #3 for his thorough review of our manuscript and for his comprehensive comments. We believe that amending these points in the manuscript will increase its clarity, and hence will lead to an immense improvement of the manuscript. The following is a list with the referee's comments and our response to each point.

General comments

- 1) 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 nov-

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elty 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.

Our study differs substantially from previous studies. Please see page 4 (Line 30-34) and page 5 (Line 1-20) in the revised manuscript. We hope that this new discussion is satisfactory.

- 2) 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.

As addressed in the revised manuscript, in a multi-objective (multi-criteria) decision making problem one usually considers a set of alternatives, which are valued by a family of objectives (criteria). Assessment of such a set of overall preference of an individual decision maker leads to aggregation of all objectives into a unique objective, called a multi-attribute utility function. Eq.(9) in the manuscript represents such a multi-attribute utility function. Therefore, although the evaluation of each monitoring system

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alternative using Eq.(9) results in a numerical value of total cost (this value is known as utility in decision making approach) that indicates the degree of preference, which is namely the combination of three objectives (maximizing the detection probability, minimizing the contaminated area, and minimizing the total cost of the monitoring system) considered in this study, yields on that alternative. So we do focus on the multi-objective nature of the monitoring problem, not only in the abstract and introduction part of the manuscript but also in the whole manuscript.

As we described in the Section 2.3 an optimal system does not necessarily enables 100% reliability or does not require a certain pre-specified reliability level. It is defined by the cost associated with possible maximum detection probability and possible minimum contaminated area by using the least number of the wells. Despite the fact that the optimal design is based on expected total cost the actual reliability is incorporated in the risk term of Eq.9 (as Referee#3 mentioned). See also our reply to the general comment no.2

- 3) 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.

The correlation length in the example presented in this paper should be between 8 m and 16 m in order to obtain meaningful results while keeping a balance between the level of discretization and the computational expense with respect to findings from the studies Gelhar (1986), Ababou et al. (1989) and Bellin et al., (1992). We have performed analysis also for correlation length of 8 m and 12 m. The results showed that there is no distinguishable effect on the optimal design.

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 first version of the manuscript . We hope that these new tables and the corresponding discussions included in the revised manuscript are satisfactory to present the effects of the variability in hydraulic conductivity and the dispersivity of medium on the optimal design.

Specific comments

- 1) 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 ex-

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pected cost, irrespective of the actual reliability of the system (although it incorporated in the penalty term)?

We have rewritten the second and third paragraph and added the reference mentioned by Referee#3 in the revised manuscript. Please see the revised manuscript page 3 and page 4

With our statement “an optimal solution for the detection monitoring system cannot be determined based solely on objective function values” we mean that: the solution of a multi-objective optimization problem yields an infinite number of optimal solutions referred to as Pareto optimal solutions that are equally good optimal solutions. This solution set is usually large and gives rise to two main problems. First, identifying one solution for implementation can be quite tricky and secondly, because the objectives are usually non-commensurate, finding a preferred point as a compromise or satisfying the solution with a rational procedure can be quite a challenging task. On the contrary, the solution of a multi-objective (multi-attribute) decision analysis gives a single numerical value corresponds to the best solution. For the answer of the question: “ 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)?” Please see our response to general comment no 2.

- 2) 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.

Please see our response to general comment no1.

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- 3) 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 ˇ E’ not be before the box with ‘Determine the plume size cumulative distributions given detection ˇ E’?

This point has been corrected. Please see Figure 1 in the revised manuscript.

- 4) Page 31, line 18-20: ‘A simulation-based model ˇE coupled with ˇE 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’.

The sentence is rephrased. Please see page 6, line2-4 in the revised manuscript.

- 5) Page 32, line 21-22: ‘Therefore, rather than producing a single contaminant plume every time a system detects a plume ˇ E’. 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.

The sentence is rephrased. Please see page 6, line31-32 and page 7 lines 1-2 in the revised manuscript.

- 6) 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.

This pint has been changed in the revised manuscript (Please see Eq. (7). Probably the referee has been confused by the mistake we made in the lower limit: it should be 0 instead of -ě. Still the functions that are integrated are functions of the one-dimensional

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variable A. - 7) 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’. These are corrected. Please see Eq.(8) and Line 3 on Page 10 in the revised manuscript..

- 8) Page 36, equation 9 (line 10): Last term of equation 9 should be ‘expected cost given failure’.

It is corrected. Please see Eq.(9) on Page 10 in the revised manuscript.

- 9) 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.

Please see our response to general comment no 2 and Section 2.3 in the revised manuscript.

- 10) Page 38, line 1-3: ‘ ˇE 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?

For very low dispersive medium or for the cases that consider advective contaminant transport, the plumes originated from the ends may not be as likely to be detected as those originated from the center of the facility. In such cases, a longer line of wells that extends beyond the length of landfill may be slightly better. This is what me meant by major assumptions on the dispersivity of the medium. An ideal monitoring design should consider all the possible uncertainties involved in the problem. The ground-water flow rate and direction, chemical characteristics of the contaminant, subsurface heterogeneity, the size and the location of the leaks, the screen length, the depth of the

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monitoring wells, sampling frequency etc., are the uncertainties that make the success of a monitoring system itself uncertain. However, computational considerations limit the number of parameters included in the Monte Carlo simulation. Hydraulic conductivity field and the leak location are random inputs. On the other hand the relative importance of dispersivity and other additional parameters such as sampling frequency, are investigated in the framework of sensitivity analysis. The spatially varying hydraulic conductivity field captures the larger scale mixing process, while dispersion on a scale smaller than the discretization of the numerical model is described by the micro-scale dispersivity coefficients.

- 11) 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). The explanation indicating that each monitoring system (i.e., with a fixed number of monitoring wells) is located at different distances from the landfill is added to the text and in the caption of Figure 2. Please see line 1-3 on page 12 and caption of Figure 2 in the revised manuscript

- 12) Page 40, line 14-15: Definition of best design system. See earlier comment.

Please see our response to general comment no. 2 and Section 2.3 in the revised manuscript.

- 13) Page 41, line 10-14: 'The analysis showed that ˇE 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

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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.

We agree with Referee#3 that Figure 3 indicates that for systems with nws $>$ 0.10 there will be always plumes that remain undetected, since the spacing between the wells are mainly less than the plume width at any ndfs considered here or due to preferential flow paths. As we mentioned in Section 4.4.1 and 4.4.2, the subsurface heterogeneity and dispersivity are the parameters controlling the spreading of plume and therefore the reliability and the location of the monitoring systems. The analysis showed (although not presented in the Figures 10 and 11 only to avoid crammed presentation of the results) that for transverse dispersivity aT=0.12 a monitoring system with nws=0.12 (8 well) achieves 100% reliability at ndfs=1.8. On the other hand, whenthe variance of lnK equals to 1.2 only the monitoring systems with nws$<$0.08 can detect all the plumes. For instance a 12-well monitoring system can detect maximum 95% of the plumes at ndfs=1.9.

14) 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.

We do not agree that when nws increases, the size (width) of most of the detected plumes gets larger because nws is not a parameter that effects the spreading of the plume and accordingly the size (width) of the plume. Plume size is controlled by dispersivity of medium, heterogeneity of the medium and the distance that it travel away from the contaminant source. Therefore, the detected plume gets larger due to the increase in ndfs and not due to the increase in nws. Hence the slight increase in expected plume size with increasing interwell distance occurs when the plume travels further away from the source (see Figure 3). As the Referee #3 mentioned plumes will go undetected more easily as the spacing between monitoring wells are more likely to be larger than the plume size (width) when nws increases and this already indicates that nws is not a parameter that effects the plume size but a parameter that has influence on detection probability of a given monitoring system.

15) 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?

In this study, E(Af) is defined as the area of contamination when the plume remains undetected at the end of the monitoring period of 30 years (please see Section 2.3). At the end of the monitoring period the plume size will be more or less the same regardless of the distance from the landfill or interwell spacing. Therefore E(Af)does not show a decreasing trend with increasing neither ndfs nor nws but remains almost constant with respect to both parameters.

- 16) 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.

Although the values used Figure 5 represents the correct definition of coefficient of variation the typing mistake in the definition of the coefficient of variation in the text (Page 42, line 19-27) caused an inattentive misinterpretation of the results. These are corrected in the revised manuscript. Please see revised manuscript page 15 line 1-9. What we meant here by “stationary” is that the size of the detected plumes do not change and stay more or less the same. To avoid the confusion we replaced the word “stationary” with “uniform” as the common use of “stationary” indicates that the plume has reached its maximum extent.

- 17) Page 45, Section 4.4.1: This paragraph presents straightforward results and has little added value. Can be condensed into 2-3 sentences.

Although the paragraph presents the straightforward results it explains the points in special comment no. 13 and we refer to Section 4.4.1 in our response (Please see our response to Special comments no. 13). Therefore for clarity we prefer the paragraph stays the same.

- 18) 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 fac-

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tor 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.

We are not entirely sure for what the Referee#3 asks here but we think that first and second points mentioned by Referee#3 actually contain the explanations already given on Page 18 Line 9-20. For the second point related to 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 in order to determine the plume area. Please see the description of detection and contaminated area given in Section 2.1, Figure 1 and in Section 4.4.2 on page 18 line 8-18.

- 19) 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.

Please see the response to general comment no.3.

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