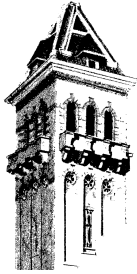[Type here]

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December 29, 2014

SUBJECT: Re-Submission of Manuscript #2013WR014667 to *Water Resources Research.*

Ximing Cai, Editor in Chief
Water Resources Research
American Geophysical Union

Dear Dr. Cai

Please find attached an electronic copy of the revised manuscript #2013WR014667, titled "Blended near-optimal alternative generation, visualization, and interaction for water resources decision making", by myself as the sole author with I am resubmitting for publication in *Water Resources Research*.

Below, please also find a listing of the numerous revisions that incorporate and address the suggestions and comments of the 3 reviewers, associate editor, and yourself. Should you need to contact me, please do so via email at david.rosenberg@usu.edu.

Sincerely,

David E. Rosenberg (the corresponding author)
Associate professor

Attachments
- Revised manuscript
- List of revisions and response to reviewer comments

<div align="center">

Response #2 to Reviewers' Comments
December 28, 2014

</div>

General Comments

Again, thank you for this opportunity to further revise and improve the manuscript. The most significant changes include to:

1. Expand Section 2 to include formulations for the prior MGA methods used and against which the new tools are compared.

2. Expand Sections 1, 2, 6, and 7 to demonstrate improved scalability over prior MGA methods:

    a. Present problem sizes in sections 2 and 3 and runtime performance in Section 6.2 and a new Table 1.
    b. Observe in Section 7 that the stratified-sampling method solves a larger number of smaller optimization problems to identify decision variable maximum extents, whereas the MGA distance-based methods solve fewer, larger problems to maximize distance between alternatives. Further, the stratified-sampling maximum extent objective is linear whereas the MGA distance metric is non-linear and discontinuous and must be resolved with more computationally intensive global search or evolutionary algorithm methods.
    c. Also observe in Section 7 that the MGA problems grow in size with the number of generated alternatives, whereas stratified sampling simply adds Monte-Carlo Markov chain samples within prior-identified maximum extents.
    d. Additionally in Section 7, that the new tools take less time to generate more numerous alternatives that more completely show the near-optimal region—even for the Echo Reservoir problem with 39 decision variables.

3. Add an appendix that lists the model formulation for the Echo Reservoir phosphorus removal problem.

4. Edits to improve the clarity of the writing and figures and shorten text.

Below, I elaborate on these changes, list reviewer comments in *blue italic*, and indent my further responses in plain black text.

Responses to Reviewer #1 Comments

The paper proposes a novel methodology to generate and communicate the structure and the extent of the near-optimal region to address un-modeled issues in solving optimization problems. The method relies on (1) the stratified Monte-Carlo Markov Chain sampling to generate alternatives in the near-optimal region, (2) parallel axes plot to visualize objectives and decision variables, (3) interactive controls allowing the users to explore the near-optimal region and the

set of alternatives identified. The approach is demonstrated on a linear water quality problem aiming to reduce the phosphorus load in the Echo Reservoir, Utah. The author was able to improve the paper with respect to the original version. However, I suggest a second revision of the paper according to the following points:

> Thanks, I feel the above comments summarize the paper contributions very well and I am able to make the changes suggested below to further improve the paper.

- in the introduction, the author frames the paper within the systems analysis umbrella, searching solutions to "large, uncertain, real-world" problems, with the paper introducing a method to address the challenges of (1) generating alternatives that address un-modeled issues and (2) tractably communicate them. Since the application of the methodology is on a highly simplified linear problem, I would say that the paper main contributions are methodological. Yet, in order to demonstrate the effectiveness of the proposed method wrt points 1 and 2, in my opinion the paper needs a real interaction with the users and decision-makers. Since the method is not only identifying the set of near-optimal solution, which can be extremely large (i.e., 2,500 in the application presented) and the user might be overwhelmed, the interaction and the response of the real users is essential to validate the effectiveness of the method (as also pointed out by reviewers 2 and 4).

> The revision now no longer mentions "large, uncertain, real-world" in Section 1. Text added in Sections 1, 2, 6, and 7 also now further explain the problem size and scaling aspects (see summary point #2 on page 1 of this response letter) as well as emphasize the methodological contributions as suggested in the above comment. This is includes a new paragraph in Section 7 startling a line 632 which points out the methodological differences to prior MGA work.

> I agree that a demonstration of the effectiveness of the interaction tool remains as an important follow-up work; this need is mentioned prominently in Section 7 (lines 664 to 682) and the conclusions (line 720). At the same time, I believe there are multiple methodological and management contributions contained within the paper that derive from the generation, visualization, and demonstrated interactions. These contributions include to generate more numerous alternatives faster, more completely show the near-optimal region, and help elicit a larger set of un-modelled issues than by prior MGA methods. Also to identify more numerous and flexible alternatives to manage the system and maintain close-to-optimal performance.

- the concept of set-valued solutions (lines 144-146) is a bit confused. Set-valued optimization means to identify multiple solutions that provide the same performance in the long run. It does not relate to Pareto-optimality and to provide a large number of Pareto-optimal solutions instead of a single one, whereas it means that the same point in the objective space is associated to multiple points in the decision variables space. For example, in your application, a set-valued solution can be the couple (apply practice P1 in watershed W1) and (apply practice P2 in watershed W2) if their costs are the same. In this way, the decision maker can freely choose the solution that he considers the best with respect to the un-modeled issues, still obtaining the same optimal performance without the need of accepting a tolerable deviation. In my opinion, the search of these solutions can be included as the first step of the proposed method. In the case set-

value optimal solutions do not exist, the decision maker will be more prone to accept a near-optimal performance.

> Yes, the idea of set-value was confused in lines 144-146 in an attempt to review several things in the same sentence. The review of set-value work now appears at line 147 "These challenges similarly confront narrower searchers of control policies for the multiple optima that have the same value in objective space but different locations in decision space [*Aufiero et al.*, 2001; *Nardini and Montoya*, 1995; *Orlovski et al.*, 1983])" and are then followed by the review of pareto search methods. Agreed that search of set-value optimal solutions should be part of the method. It is as the new tools generate alternatives continuously between the optimal objective function value and tolerance level (as shown in Figures 3-6) and more specifically emphasized with the color ramping of the removal cost objective in Figure 7. The darkest green band in Figure 7 shows the multiple optima vary implementation levels of select decision variables by a small amount and are nearly identical in solution structure. This similarity motivates search for near-optimal alternatives shown in lighter shades of green that have a removal costs higher than the optimal cost. The text in section 6.5 now makes several of these points.

- the methodology is presented on a a linear and a mixed-integer problems. However, it requires to solve several optimization problems and the scalability to more complex problems (e.g., real-number decision variables, non-linear models and objectives, stochasticity, dynamics models) can be a significant limitation. I would suggest to anticipate the limitations of the method (currently discussed only in the conclusions). In the present form, the method indeed is not able to contribute to the systems analysis goal of searching solutions to "large, uncertain, real-world" problems.

> Again, I have removed "large, uncertain, real-world" from line 81 in the introduction. The manuscript now more holistically addresses the issue of scalability in Sections 1, 2, 6, and 7 (see also point #2 in the summary of changes on page 1 of this letter) to explain how the new stratified sampling tools take less time to generate more numerous alternatives that more completely show the near-optimal region than prior MGA methods. This comparison shows the new tools better scale than prior MGA methods. And while other MGA methods have been applied to larger, non-linear problems, it is important to recognize that those applications--at best--generated a very small number of near-optimal alternatives that gave a limited view of the region and provided a very small basis to elicit un-modelled issues. Thus, the new tools are an improvement. The new tools may not be a panacea owing that they have, up to now, only been demonstrated on the medium-sized Echo Reservoir problem that has 39 continuous-valued variables. With this in mind, many of the contributions in the abstract, discussion, conclusions, and significance sections have been re-worded to emphasize the comparative advantages of the new pools over prior MGA methods.

- the scalability of the method should be also discussed with respect to the possibility of effectively visualizing a large number of decision variables and, potentially, of objective. There are works (e.g., Miller 1956) stating a limit for the information that human beings are able to hold equal to "seven plus or minus two". Although the parallel axes plot are easily scalable (Fig.

6 has 41 axes), I'm concerned about the effectiveness of showing 2,500 solutions over 41
dimensions.

> Yes, I am very familiar with the rule of "seven plus or minus two." It's a great rule and one I hold dear. So I'm unclear… what is the concern about effectiveness? Effective for achieving what goal? To me, the purpose of the parallel coordinate plotting tool is not that someone has to consider the 2,500 alternatives separately or individually (an impossible task), but rather to amass all the sampled alternatives and visualize them together so that the user can instead see them as part of a single, larger construct which is the near-optimal region. In this way, the near-optimal region extents and other features serve as a reference frame (background layer or base map if you will) to guide the user's subsequent interactive exploration. To clarify this point, I have re-written Line 674.

- although the multi-objective reformulation is promising and provides interesting results, the
practical implementation of the method is only partially a multi-objective optimization. As far as
I have understood, when 2 objectives are considered, the method samples the first objective in its
near-optimal range and, given this, it moves to the second objective. Such an approach seems to
be similar to the well-known lexicographic method, which, however, allows computing few
points of the Pareto front. Then, the near-optimal extension increases the number of identified
solutions. However, in my opinion, the multi-objective analysis would require a benchmark
comparison with a truly multi-objective optimization (as done with the MGA).

> Based on this comment, I have revised the text in Section 3.3 to further clarify the modification for multi-objective problems and how the method resembles but is also different than the Lexicographic method.

> The manuscript does provide a benchmark comparison (Section 6.4 and Figure 5)! Obviously, the comparison was not clear in the prior draft so I have added text in Section 6.4 and the Figure 5 caption to describe the constraint method as the standard multi-objective solution method used to generate the pareto solutions that form a main part of the example in Section 6.4. Additionally, I fixed a typo and now refer to the pareto tradeoff as black triangles and black lines. In sum, the example in Section 6.4 already provided a benchmark comparison to a multi-objective method (the constraint method) and now improved phrasing should draw more attention to the multi-objective method used in the comparison.

- at lines 425-426, the author compares the ranges explored by MGA and by the proposed
method: is it fair to compare the explored ranges by 5 MGA alternatives and 2,500 near-optimal
ones? I mean, was it possible to generate more alternatives with MGA? If yes, I guess that they
have to be generated; if no, such a limitation of MGA should be clearly defined.

> Yes, the comparison was fair, MGA-Hop Skip Jump reached its stopping criteria of no new non-zero decision variables entering the decision set. This criteria is the same criteria that stopped MGA-HSJ in the Figure 1 example. Sections 2 and 6.2 that describe the MGA-HSJ results now also present the stopping criteria that led to the results.

Please additionally note, in further testing, I found the # of alternatives generated was sensitive to the decision variable scaling in the problem formulation. For example, MGA-HSJ terminated at 22 alternatives when all decision variables entered the non-zero set for the Echo Reservoir model formulation presented in the Appendix. Only five alternatives were generated for a second model formulation that instead used differently scaled decision variables of land area and stream bank length.

Given this sensitivity, the manuscript now first introduces HSJ results from the run that generated 22 alternatives, mentions the results for the second formulation, additionally presents results from MGA-Serial and MGA-Simultaneous and adds discussion in the first 3 paragraphs of Section 7 to explain how the new tools outperform each of the MGA methods for scaling, objective criterion, and methodological reasons. The first paragraph of Section 6 also explains the number of alternatives for the MGA-Serial and MGA-Simultaneous runs was set to make runtimes commensurate across methods.

MINOR COMMENTS
- line 76: I don't get the meaning (and need) of the citation

OK. Deleted. The "best is the enemy of the good" is one of the motivating rationales for near-optimal work. True, this paper is more focused on how to do good better.

- line 238: "several" is actually two classes (i.e., linear and integer), plus the multi-objective extension

Yes. The text now says "different classes of optimization problems."

- Section 6, lines 364-375: there is still a large use of the first person

I have edited the text here to remove several of the first person "I"s.

- line 440, : the figure has a single panel

Thanks, this is an error. I have removed the reference here to Figure 4. The subsequent reference remains a few lines below.

- line 506, : I guess it should be figure 7

Yes, correct, changed.

Responses to Reviewer #2 Comments

The editor did not provide comments from a Reviewer #2.

Responses to Reviewer #3 Comments

The reviewer provided his/her comments as edits and additions to the original response letter. Nearly all the comments were a simple OK to indicate the original changes and responses satisfactorily addressed the original comments. Here I reproduce and respond

to only the comments that the reviewer indicated still need to be addressed. When needed and to help the reader track the progression of multiple responses, I provide the original comment in blue, my first response in black, and the reviewer's second reply comments in red. My second response appears indented at the bottom in black. Note in some instances, the reviewer intermingled his/her 2nd comments with the first response.

The reviewer also provided several general and specific comments regarding the style of the first response letter which I address together at the end. In general, the reviewer's comments greatly improved the quality of the work and the aim for the first and new responses is to acknowledge the comments, explain how I interpreted them, and describe changes made in the manuscript to address the comments.

Responses to specific comments that still need to be addressed

The author uses "I ... (do)" in many locations in the paper. See Abstract, line 1 and 4, also and later in the paper: Is "I present" "I demonstrate" acceptable style?
This comment was also raised by two other reviewers. I have revised to reduce the use of first person narrative (but not in this response letter :). Still too many to my taste, but I will let it go and leave it for the Editors to decide. E.g., see lines 371-372 in the new MS, where it says "… a single-objective linear program for Echo Reservoir [*Alminagorta et al.*, 2013] that I helped develop". The author of this MS is one of the authors of *Almingorta et al.,* so it should be is obvious that he contributed to that paper.

> Yes, several uses of first person at the start of Section 6 are not necessary and I have removed them. See lines (now) 396-401.

99: If there are only 2 decision variables, how many optimization problems have been solved to generate the circles and triangles in figure 1?
None. The points on the figure are extreme points which were generated by enumerating basic and non-basic combinations for the primary decision and slack variables (associated with the optimization program constraints). Then testing each combination to see if it is feasible. These steps are part of prior enumeration methods and not central to the nearoptimal method and tools proposed in this work. Thus, as noted in the response to line 93, I have removed these points from the figure. The MGA triangle is hard to discern in the figure – try changing the MGA text to bold/black or to white or move away from the purple area.

> Thanks. Figure 1 now shows the MGA-HSJ marker as a black square. The labeling text has been removed and the figure now includes a legend to differentiate the MGA methods tested.

100-148: Has the author added something new to the parallel coordinate visualization method, or possibly to the interactive solution? If so: state this explicitly. If not: there is no justification for the explanation. This part is less important than the exposition of the case studies, which has been skipped (see above).
Yes, there are several new contributions: a) Use of parallel coordinates to visualize solutions to a high-dimensional optimization problem Is this actually new? I must admit that I am not fully familiar with parallel coordinate display but doing it for high dimensional problems must have been at the basis of the method, no?, b) simultaneously

link objective and decision spaces on a single plot OK, and c) and place interaction controls on the plot to allow managers to explore the near-optimal region. I have revised and reordered the writing in this section to emphasize these contributions and how they relate to the broader near-optimal method. OK, except see the comment above.

> Yes, the use of parallel coordinates to visualize solutions to a high-dimensional optimization problem is new. Parallel coordinates were developed to visualize high-dimensional data sets and several water resources applications have used the plotting technique to visualize discrete points in the objective space of an optimization problem. Here the contribution is in visualizing solutions and alternatives through the linked decision and objective spaces. I have further revised the statement in the introduction to read:
>> A new Parallel Coordinate plot [*Inselberg*, 2009; *Wegman*, 1990] places axes for all objectives and decision variables side-by-side on a single page to show generated alternatives across the linked decision and objective spaces.

190-196 and Figure 4: The text that relates to Figure 4 is not clear. Why show the (suggested) disadvantage of uniform vs. stratified sampling?
The intent is to justify the stratified sampling approach. I have removed this figure and the discussion in the revised manuscript. The review in the introduction simply describes this limitation. OK. See below my comment on Figure 7, after a response by the author to the results demonstrated by this figure. Is there a confusion in the revised paper with Figure 5?

> Sorry, there was a typo. The reference in Section 6.5 to Figure 6 has been changed to "Figure 7".

I could not find the word "Figure 7" in the text, where the text would explain what it stated in this response.

> Again, this comment seems to be in response to the same typo noted and fixed in the prior response.

It might have been interesting to indicate the combination of variables that generates the max (110%) value of the objective function. The one that generates the min (100%) is the black line. Comparing these two on the graph may reveal something.

Great comment, and yes it does! Color ramping the results (Figure 7) now allow the reader to simultaneously compare the optimal and 110% levels, as well as many more progressive levels between 100% and 125%., all on a single plot! Please also see my responses to your two prior comments. But where is this explanation in the (revised) text?

> The explanation was and remains in section 6.5. Sorry that the same typo noted in prior two comments (now corrected) caused this additional confusion.

<u>Responses to comments on the style of the first response letter</u>

Many of the responses by the author tend to be explanation delivered to the reviewer, explaining to her/him details of the method and of the results. This is not what I normally expect to see in a response/rebuttal. If the author has to provide me with lengthy explanations this remains between him and me, and does not get to the reader.
For example, see on page 3 the author's response to the comment that begins with "It seems that the only novel …".

A response by the author should basically point to the changes made in the text (where and what) and provide a short explanation why it has been done. A dialogue between author and reviewer does not serve the readers – unless it reflects changes/improvements made to the paper.

Finally, consider the extra burden it places on the reviewer, having to review the rebuttal as if it were another piece of the paper itself.

Still, I have endeavored to avoid asking that the rebuttal be re-written, and have gone through the points I raised and the responses provided by the author. See my responses to each point.

Both the reviewer's 2nd set of comments (including the one above) and my 2nd (this) response letter significantly tone down the 1:1 dialogue as text sticks to referring either to the work or comments about the work.

I wholeheartedly agree that response letters should be as brief as possible and limited to clarify how the author interprets a comment (if needed), describe how the comment is addressed in the revised manuscript, and justify why the change was made in the way it was. The first response letter was long because there were a large number of reviewer comments, the comments were substantial, and I made numerous global and local changes throughout the manuscript to address the comments (including reorganizing the methods and rewriting most of the manuscript text). This 2nd response letter is shorter because there are now fewer comments and associated modifications. I provided all responses in the first letter—including those which may seem like 1:1 dialogue such as quoted at the bottom of page 7 of this letter—with the aim to show changes/improvements to the manuscript and assist reviewers to track where and how particular comments were addressed without having to re-read both the original and revised manuscripts. This dialogue can still benefit readers of the final published manuscript… as I have posted all the materials (original and revised manuscripts, reviewer comments, my responses) on the project's GitHub repository at https://github.com/dzeke/Blended-Near-Optimal-Tools.

Admittedly, there was extensive use of the word "you" and "your" scattered throughout the 1st reviewer comments and my 1st responses (that refer to either me as the author or the reviewer) that further 1:1 dialogue but do not advance the goal of showing improvements in the work.

Responses to Reviewer #4

Overall, the manuscript has been improved to much more clearly communicate the contribution of the work and the methodology itself, which is appreciated.

Thank you.

The correct formulation for the MGA approach is not provided in the description of what is MGA, starting at line 174. The new problem formulation should have a new objective function that maximize differences between the first identified solution and new solution.

Yes, correct. Section 2 now includes formulations for the MGA-Hop, Skip, Jump (MGA-HSJ) and Serial (MGA-Serial) methods as noted in major change #1.

Can Figure 2 be done using the same modeled problem shown in Fig. 1? That would communicate all ideas well. If not, the mathematical model for Fig. 2 should be included.

Great idea. Done. And made easy because there is a parallel coordinate plotting tool.

The mathematical model for the case study should be included. Especially the MGA formulation should be included because that will drive what kind of MGA solutions are obtained.

The Echo Reservoir model formulation is now presented as Appendix I. No change/modifications beyond the formulations presented in Section 2 were needed to run the MGA methods on the Echo Reservoir problem.

There are still grammatical errors and problems with the style. It seems that the author is trying to introduce a new style for journal publications, since he is not following commonly accepted rules for style (by using first person and imperative tense). This is distracting in some places and makes the manuscript more difficult to read. For example, at line 581, it is not easy to read the transition between tenses in a discussion paragraph: "Then provide interactive tools for users to further explore the features of most interest." In addition, at line 466, "Say, for example, the manager explains that their preference is motivated by a belief that the TMDL phosphorus..." (should be his/her, not their) The informality here and in use of first person sacrifices professionalism.

No, the intention is neither to introduce a new style nor make the manuscript difficult to read. Rather the opposite and the text examples listed by reviewer are simply imperfect grammar which I appreciate the reviewer pointing out so that I can correct. The sentence formerly at line 581 is missing a subject; I have deleted this sentence entirely. The sentence formally at line 466 has been shortened, the dual gender pronouns substituted, and the address made more formal. Elsewhere I have further reduced use of the first person (see also response to a comment from Reviewer #1).

I disagree with the contention that all use of first person is to be banished from scientific or technical writing. This banishment does not serve readers. In certain instances it is shorter, more succinct, and clearer to write in first person to explain who did what.

Fig. 3. Because the MGA approach should be run to identify (ideally), one extreme point in decision space, then the next extreme point, etc., MGA should identify the same extreme points as generating the near-optimal region. That is to say, one might expect that the MGA identifies the outline of the ranges shown in Figure 3 by the green lines. Even if the number of MGA runs were more to give a better representation, the solutions that have been identified do not stray from the center point of each decision variable, except for a few decision variables. Can the manuscript comment on why this is true? Is this a characteristic of this problem (and problems like it)? For example, if a second and third MGA solution were generated for the problem shown in Figure 1, the next point that would be generated would at the corner of the near-optimal region (about (3,1.5)) and then at the mid-point of the curved side of the near-optimal region. These points would comprise the range of X1 and X2 that are covered by generating the near-optimal range. Highlighting why the near-optimal solutions cover a larger range than the MGA solutions is important to understand if that is a true contribution of this work or if that is an artifact of this problem, the number of MGA solutions selected by the user, or the specific MGA formulation that was used - such as the weighting of different decision variables when they are summed for the new objective function. For example, if decision variables are normalized then different answers may emerge. If generating the near-optimal range identifies solutions that are not identified (or cannot be identified) by the MGA, then that is an important contribution.

Yes, thank you for this excellent insight and prompting. I too was surprised by the observed MGA-Hop, Skip, Jump (MGA-HSJ) behavior and found with some further testing that the behavior is real. MGA-HSJ reached one of its stopping criteria (no new non-zero decision variables) so it is not possible to generate more alternatives. MGA-HSJ cannot always identify the full extents of every decision variable because of a limitation of both MGA-HSJ and more generally to MGA optimization-based approaches to generate alternatives to address un-modelled issues. The revised manuscript now includes multiple changes and additions in sections 2, 6, and 7 to (i) present the MGA formulations used, (ii) characterize the observed behavior including stop criteria, (iii) discuss why the behavior occurs, and (iv) state how the new near-optimal tools improve on them. Specifically, see the new paragraphs that start at Lines 202, 214, 494, 606, 619, and 632.

To summarize, for MGA-HSJ, the problem is in its objective function. Minimizing the sum of non-zero variables does not regulate which and how many previously zero-valued variables enter at an iteration and at what levels. One or more variables can enter at small or medium values—values need not be at their full extents. Once entered, MGA-HSJ will drive those variable values back to zero on successive iterations and never revisit to find their true extents. More generally, 5 (or now 22 with a different model variant, another issue entirely) HSJ generated alternatives represent a very small fraction of the total extreme points for the problem, give a limited view of the near-optimal region, and are not necessarily points of management interest that an analyst can use to elicit un-modelled issues and update the problem formulation. If an analyst had information about which extreme points were preferred (even partial info), then the underlying issue can be modelled—simply update the HSJ generation criteria (or the objective functions for any of the other MGA optimization generation approaches) to better direct alternative generation towards preferred extreme points. But we don't have this information and this

paradox exposes a limitation of using optimization to generate alternatives to elicit un-modelled issues (i.e., that are unknown prior to alternative generation).

I think the problematic MGA-HSJ behavior has gone unnoticed up until now because there have not been tools to generate and visualize the entire near-optimal region and against which to use to compare results from MGA or other methods. People assumed MGA-HSJ generated the full extent of the near-optimal region… but it does not. The more correct description is MGA-HSJ generates a few select alternatives at extreme points of the near-optimal region and these extreme points may—or may not—be of management interest. (Similarly, other MGA variants generate alternatives at select other extreme points which may or may not be of management interest). As the reviewer notes, the comparing the results from the new tools to MGA-HSJ identifies a short-coming of MGA-HSJ which is an important contribution and now stated in the abstract, discussion, and conclusion. In my view though, this contribution derives from two even more important contributions which are that the new tools generate and visualize the full extents of the near-optimal region. Again, thanks for prompting me to look more carefully at this issue.

In section 6.4, how are the new MGA solutions generated? Having the mathematical model to refer to would be helpful

Again, text in Sections 2 and the first paragraph of section 6 now explains how the MGA alternatives are generated.

Associate Editor (Remarks to Author):

The revised manuscript "Near-optimal alternative generation, visualization, and interaction for water resources decision making" has received 3 detailed reviews. Overall the reviewers all agree that the authors have been highly responsive and that the manuscript has been substantially improved. That being said, 2 of the 3 reviewers still recommend Major Revision in the second round of review. My own review of the revised manuscript raises two very significant concerns: (1) the simplicity of the test case strongly limits the generality of claimed contributions and (2) the core methodological contributions for the near optimality approach are minor extensions of well established concepts. The overall scalability of the approach and its actual use in actual decision making is not well articulated or treated in the manuscript. The discussion of the decision making is not fully convincing in terms of the value and insights in a complex decision making environment.

Thanks. I have used these comments to guide the second round of revisions. The resulting changes are listed at the top of this letter. These changes:

- Establish the major methodological contributions of the new blended tools to stratify sample to generate and visualize the full near optimal region. This approach differs significantly from prior near-optimal work which uses optimization to generate a few alternatives at select extreme points of the region.

- Show that the new blended tools perform much better in run time, # of alternatives generated, and coverage than multiple prior near-optimal methods (now MGA-Hop Skip Jump, MGA-Serial, and MGA-Simultaneous). Also that the new tools better scale.
- Emphasize that as a result of the above, the new tools can better elicit un-modelled issues, improve model formulations, and, in turn, improve decision making.
- Use all of the above to demonstrate and support the paper's main contributions and findings.

I have also removed mention in the motivating introduction of "large, uncertain, real-world." Yes, the demonstration example is still a linear problem of moderate size and important work remains to demonstrate use of the new tools on more complex problems. This need is acknowledged prominently in the discussion and conclusions. But linear and moderate sized should not be automatic disqualifiers. The paper's central themes of near-optimal and un-modelled issues apply to problems of all sizes and complexity. Further, the deficiencies of the MGA methods now more clearly identified in the paper necessarily carry over to more complex problems (and likely will be exacerbated for those problems). Also, the problem size and complexity of the example are comparable to prior near-optimal work and it's this commensurate size which permitted making meaningful comparisons to prior near-optimal methods and show the contributions of the new tools in terms of methodological approach, # of alternatives generated, region coverage, computation time, scalability, etc. Even though the example problem is linear and of moderate size, the paper offers multiple useful ideas, tools, and results for people working on more complex problems.

I believe the suggested Major Revisions are beyond the scope of what is acceptable for the second round of review. Moreover, I think the scope of this work and its simplified case study may be more appropriate to a more applied journal.

Thank you for the opportunity to revise this paper.