

## ***Interactive comment on “A multi-objective optimization tool for the selection and placement of BMPs for pesticide control” by C. Maringanti et al.***

**C. Maringanti et al.**

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We take this opportunity to thank all three reviewers for the time taken in carefully reviewing and suggesting edits to improve the manuscript. The suggestions and modifications made by the reviewers have greatly enhanced the quality of the work presented in the manuscript. We present a detail response to the comments made by each reviewer as follows:

Response to the comments of Reviewer # 1 Specific comments

Comment #1: I am not convinced that the SWAT has been properly calibrated to produce any reliable result. As you can see clearly from figure 5, there seems a linear trend

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between actual and simulated result, but the simulated results are not much close to the actual one. What was the r-sq for the pesticide result? Did you use any optimization method to find the model parameters? Report the parameter values and describe fully how the calibration was done. The SWAT model should be properly calibrated before any kind of analysis.

Response: This is a very important comment made by the reviewer. We have explained in detail the calibration routine in section 3.2 (page 16, lines 9-20 of the revised manuscript). The various calibration parameters for flow and pesticide are detailed in Table 1 of the revised manuscript.

Comment #2: Is the goal of the optimization to meet the maximum pollutant reduction at the watershed level or at the farm level? If the goal is to have the maximum deduction at the watershed level, I strongly disagree with your 1st assumption that was used for developing the BMP tool. How can you assume that the pollution reduction with BMP at the watershed scale approximates that at the field level? Can you explain your assumption using the physics of the transport and fate of pollutants in the watershed system? Can you show the simulation results provide reasonable outputs to make this assumption? How do you account for the spatial heterogeneity in finding the location and selection of BMP using your method?

Response: We welcome the comment made by the reviewer, and have incorporated an additional figure (#12) that shows the SWAT simulated pesticide loads at the watershed outlet and the HRU area weighted loads in the watershed. The figure 12 describes how the BMP reductions are similar (but not same magnitudes) for both the strategies. We have also revised the wording to clarify this point (lines 12-17 on page 18, revised manuscript). We agree that such an assumption does not consider the in-stream processes and channel routing which would simplify the optimization model and improve the computation time considerably leading to possibility of searching for larger number of generations using the heuristic MOEA algorithm that guarantees a near optimal solution but not necessarily a global optimal solution. However, if the in-stream and

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channel routing processes are highly important in a watershed, the optimization would require a full model that incorporates a dynamic linkage of the watershed model with the optimization model instead of the BMP tool to estimate the pollution reductions for every new solution obtained during the various stages of optimization.

Comment #3: I also disagree with the 2nd assumption that were used for BMP tool. The BMP performance varies temporarily under different hydrological conditions and land use practices. It cannot remove certain percentages of the pesticide (for example 45 % as you shown in table 3) for all cases. I am guessing if you made the 2nd assumption based on the average BMP performance estimated from the SWAT results. Was the BMP tool based on the results of the continuous simulation from a specific time period? How these values varied temporarily? Clarify how the SWAT simulation was done to develop the BMP tool. Please provide descriptions of the hydrological conditions and the land use scenarios in detail.

Response: It is true that the BMP reduction estimates have been obtained through simulations of the SWAT model. The reviewer is correct that the BMP performance varies with hydrologic, meteorology, and land use conditions in the watershed. We have clarified this point by revising this statement (lines 18-19, page 18). The BMP performance indicates average reduction over the period of simulation which was 5 years (2000-2004) in this study. We have clarified this point in the revised manuscript (page 17, lines 10-17). The BMP reductions are dependent on the land use of the particular HRU. However, since atrazine is applied only in corn fields, we did not apply BMPs in other land use areas. Page 17; line 20-page 18 line 10 of the revised manuscript detail the development and assumptions of the BMP tool.

Comment #4: I cannot fully follow how BMP tool was developed. In section 3.3., you mentioned that allele set was prepared only changing land use practices and BMPs on corn fields. The 2nd sentence in section 3.4, you mentioned that you have chosen all the HRUs that have a common land use. What is a common land use? Is it a corn field? The 4th paragraph described that how you chose different BMPs one by

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one ("one BMP at a time is allotted"). How did you evaluate the effects of mixture of different BMPs? Again, how does the BMP performance vary by place to place? How does your model consider that?

Response: We have revised the description of the BMP tool (Page 17; line 20-page 18 line 10) in the revised manuscript. In this study, since atrazine was pollutant of concern and it originates only from corn fields, BMPs were applied only in corn areas. However, it is not a limitation of the BMP tool. If one is interested in other water quality parameters, various BMPs specific to different land use conditions can be implemented through the BMP tool. This includes selection of more than one BMP in a particular HRU in a particular BMP placement scenario. Corn being the major crop (~40%) in the watershed and the only crop that is applied with atrazine as pesticide, was selected for the placement of BMPs to reduce atrazine. An example provided in page 19, lines 6-9 clarifies how multiple BMPs can be possibly placed in a single HRU in the watershed.

Comment #5: How does SWAT simulate the atrazine leaching to the groundwater? Does SWAT fully simulate the groundwater flow? How atrazine reduction is related to the application rate? What was the application rate? Are you considering any changes in that? 6. Does your model only concern atrazine? Are you only optimizing the BMPs for the corn fields? Please clarify the scope and purpose of your optimization upfront.

Response: SWAT does not simulate the groundwater flow completely. However, the SWAT model can simulate any pesticide of interest at a watershed level considering the routing, degradation, and in stream processes during the movement of the pesticide. The SWAT model can simulate transport of any pesticide. However, as atrazine is a major pesticide applied in the corn fields in the watershed, it was used in this study. A uniform atrazine application rate of 1.46 kg/ha, based on data reported for this and other watersheds, was used in the HRUs that grow corn where the optimization algorithm searched for BMP placement to reduce atrazine.

Comment #6: How your BMP tool reflect the BMP performance if locations and

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amounts of corn fields changes or if there are any land use changes in the watershed?  
Can you perform any kinds of sensitivity analysis to address this issue?

Response: As the BMP tool requires simulation of the SWAT model, any changes in the land use and land cover can be addressed with the watershed model. Also, if there is any change in the corn acreages in the watershed, the SWAT model would be able to simulate the changes considering the spatial heterogeneity and the application amounts, if any, during the land use change.

Comment #7: How did you choose the width of buffer strip ? The 20, 27, and 30m are a very similar type and would give a similar performance (as you showed in Table 3). Why not try to test a wide range of buffer strips?

Response: This is a very good observation made by the reviewer. We wanted to keep the buffer widths realistic with current BMP implementation in the watershed. Federal rules require that buffer width be at least 60 ft in order to receive any cost share assistance for its implementation. Our experience with the stakeholders in this and other agricultural watersheds is that farmers are unlikely to implement buffer strips wider than 100ft as it takes agricultural land away from production. The values simulated in this study represent real world conditions in this and other agricultural fields in this region.

Comment #8: Pg 1830. the last paragraph. I do not understand what it means by "the near optimal solution reached as close as possible to the global optima"? As you stated earlier, the MOP provides a set of efficient solutions (pareto-optimal), but cannot provide a single solution (called global optimal one).

Response: Here the global optima refers to the real Pareto optimal solution front in a two dimensional space. The optimal front and Pareto optimal front are described in more detail in page 10; lines 8-15 of the revised manuscript.

Comment #9: Pg 1831, Section 2,1,6. the 2nd paragraph. I do not understand "crowding distance is half the perimeter of the maximum hypercube allowed around a solution"

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and think an average reader could not understand this either. Without clarification, it is difficult to follow how the elite set and the next generations set are used to find the optimal solution sets.

Response: The definition of the crowding distance has been modified in page 11, lines 6-8, as follows: "The crowding distance is defined as the sum of the side lengths of the cuboid that touches the neighbouring solutions in case of the non-extreme solutions and is infinite for the extreme solutions (Coello et al., 2005).

Comment #10: P. 1833. Section 3: the 2nd sentence: Specify what "the variables" mean.

Response: The variables that are used for optimization and the coding of the variables is described in detail in page 20, lines 10-14 of the revised manuscript.

Comment #11: P. 1834 How can you guarantee that the solution would converge before the maximum number of iterations? What is the maximum number of iteration? How many iterations are needed before convergence on average? Have you done any numerical assessment?

Response: NSGA-II, like any other heuristic based algorithms does not guarantee to provide a global optimal solution set. This is a compromise that needs to be made for the advantage the NSGA-II model provides in searching a very large solution space with hundreds of variables. It is difficult to generalize the number of iterations needed for the convergence of the multiple objectives and needs to be evaluated on a case by case basis. We have performed a sensitivity analysis to address this issue for this study.

Comment #12: P 1835 last sentences. Clarify what the baseline scenario is? How does it affect your optimization (in other words, how the optimal results are dependent upon the base scenarios)?

Response: The baseline scenario is described in page 15, lines 18-19 of the revised

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manuscript.

Comment #13: Section 3.6 & 4.1 How does the population size is related to the number of decision variables? Isn't it fixed by the number of corn fields? How is it related to the SWAT input? Why do you perform sensitivity analysis on the population size?

Response: The genetic algorithm uses the concept of population to find optimal solutions effectively in a highly distributed search space. The population size is not related to the number of decision variables. Each population consists of a chromosome string, whose length is equal to the number of HRUs in the watershed. The population size provides different chromosome strings that will be used during the various stages of the genetic algorithm to undergo genetic modifications to produce optimal solutions for the problem.

Comment #14: Perhaps, you should explicitly explain why the pareto-front moves toward the origin is preferred.

Response: This is a very good suggestion provided by the reviewer. Based on this suggestion we have incorporated the following in page 23, lines 23- page 24, line 3 of the revised manuscript: "As the front moves towards the origin, it is ensured that the magnitude of the objective functions get reduced in both the directions. Therefore, the closer the front gets to the origin the better the solution is to minimize the two objective functions".

Comment #15: Figure 11- show the current agricultural (and other land use) practices in the study watershed.

Response: Based on the reviewer's advice, the corn areas where the BMPs were selected for placement was highlighted in Figure 4 of the revised manuscript.

Editorial comments

In addition to the specific comments, all the editorial comments have been addressed in the revised manuscript.

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## Response to the comments of Reviewer # 2

Comment #1: It will be good to differentiate between simple GA and MOEAs.

Response: We appreciate this suggestion. We have modified how the genetic algorithms are divided into SGA and MOEAs along with their difference as follows (page 7; line 13-20 of the revised manuscript): "The important development during the last decade is the extension of the simple genetic algorithm (SGA) into a multi-objective evolutionary algorithm (MOEA) that finds solutions from conflicting objective functions. The SGA is a single objective genetic algorithm that solves the solution space to find a single solution that is a near optimal solution for the given objective function. However, in MOEA there is not a single optimal solution for the given problem, instead the interactions of conflicting objective functions yield a range of non-dominated solutions known as Pareto-optimal solutions (Deb, 2001)". Also, the major modification that has been made during the development of MOEA is described as follows: (Page 8; Line 5-8) "The major modification and strength of the MOEAs when compared to SGA lie in the usage of non-dominated sorting and elitism properties during the selection to maintain a diverse solution set that has a good spread in all the objective functions (Zitzler et al., 2000)."

Comment #2: It is not clear why you selected the NSGA-II algorithm and not the SPEA-II for your work. And It will be good to include a brief description of the NSGA algorithm and how it works. Also you may include a formal definition about what makes a Pareto optimal solution (change the place of information in section 2.1.5.)

Response: It is a very interesting observation made by the reviewer and we have incorporated this comment through mentioning the advantages of using NSGA-II in the revised manuscript (page 8; lines 12-14). Drawbacks of using NSGA have been described in page 8, lines 9-12 of the revised manuscript "The NSGA algorithm, like most of the MOEAs, was highly criticized for being computationally expensive with a complexity of  $O(MN^3)$  (Deb et al., 2002), where O stands for 'order of', M stands for the

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number of objective functions, and N stands for the population size." Also, as suggested the information provided in section 2.1.5 of the old manuscript has been brought in front of the section 2.1 and also description of the Pareto front and Pareto-optimal front has been mentioned in page 10, lines 8-15 "The objective of multi-objective optimization is to search for solutions that would help in achieving the global pareto-optimal region (i.e. optimal for all the objective functions) and to achieve solutions that are as far apart as possible from each other in the non-dominant front."

Comment #3: In the discussion about the coding of the solutions it is not explained what is represented by each 0 and 1 in the representation (used/not used?) you choose and why this representation was selected.

Response: We are sorry for not mentioning how the binary coding is used to code the variables. This has been now described in section 2.1.1 (Page 9; Line 5-7) of the revised manuscript "A binary coded GA for optimization is utilized in this study using three bits per chromosome in an individual population; for e.g. a BMP number 6 would be represented as 110 in the binary notation using three bits."

Comment #4: In the explanation about the mutation operator there is not need to explain in detail the mutation procedure for real coding, since you don't use it.

Response: Based on the reviewer's suggestion, the detailed description about the real coded mutation has been deleted from the revised manuscript.

Comment #5: In the explanation about the crossover operator there is not need to explain in detail the crossover procedure for the real coding.

Response: The detailed description about the real coded crossover has been deleted from the revised manuscript.

Comment #6: It is not explained how NSGA-II uses the elitism concept and how it is used in your work.

Response: More details about the elitism concept and how NSGA-II uses it have been

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detailed in page 11, lines 1-4 of the revised manuscript.

Comment #7: It is not clear what means [Pesticide] in equation (12)

Response: Pesticide in equation 12 describes the pollutant used for optimization. However, in the present study we used only one pollutant (pesticide) and therefore the subscript has been deleted in the revised manuscript.

Comment #8: I think that the contents about the computational platform presented in section 4.2 may be presented before 4.1.

Response: We respectfully disagree with the reviewer. Section 4.1 details the results from the sensitivity analysis that is required before the final optimization run results are presented.

Comment #9: Fig. 2: it is not clear what is shown.

Response: Fig. 2 shows an example for uniform crossover in which 50% of the genes in the chromosome string (at locations 2, 3, 5, 7, and 9) of one population are randomly and uniformly crossed over into the chromosome string of the other population. This detail is added in page 11 lines 17-21 of the revised manuscript.

Comment #10: Fig. 3: try to improve the figure, may be changing the orientation. The flow chart is of a simple GA, I suggest to put the flow chart of the NSGA-II.

Response: We are sorry for not including the non-dominant sorting and elitism components of the NSGA-II algorithm into the flow chart. The updated flow chart in the revised manuscript incorporates these components in the selection module.

Comment #11: Pag. 1822. Line 26. pareto-optimal -> Pareto optimal

Response: pareto-optimal is replaced with Pareto optimal in the revised manuscript.

Comment #12: Pag. 1823 Line 4. in USA -> Considers REMOVE.

Response: "in USA" has been removed in the revised manuscript.

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Comment #13: pag. 1826 Line 24 (NSGA-II;( -> (NSGA-II; Line 26 Use the same convention in naming the MOEAs, i.e. capitalize the first letter in Non-dominated sorted genetic algorithm. I think that it is not necessary to repeat the acronym, and the references to the NSGA-II in this line, since it is done two lines before.

Response: This is a very good point noticed by the reviewer. We have updated the naming of the MOEAs into a common convention in the revised manuscript. Also, the repeated acronym has been removed in the revised manuscript.

Response to the comments of Reviewer # 3 Simulation part: Comment #1: Application and calibration of the SWAT simulation model should be much better documented. Which parameters were calibrated, what were initial and final values, how were other parameters estimated? Practically no information is given.

Response: Table 1 details the various parameters that were used during the calibration of flow and pesticide along with the optimal values obtained after the auto-calibration of the SWAT model.

Comment #2: Calibration results for streamflow in figure 5 indicate that the simulation model is not very accurate, which casts doubt on the rest of the analysis in this paper, which relies on a proper simulation of watershed processes.

Response: This is a very useful suggestion provided. We have updated the calibrated model after performing further calibrations of the flow and pesticide in the watershed. The current R2 and RNS2 for the model lie in the range of 0.67-0.72 and 0.64-0.68 respectively at a daily scale and this is a good calibration for the SWAT model and is similar to results reported by others for this model (Santhi et al., 2001, Gassman et al., 2007).

Comment #3: Calibration results for pesticide concentrations on the other hand look comparatively good (figure 6), even though flow is not simulated well, and even though annual average pesticide concentrations were used as calibration target (it seems to

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me that the model should be calibrated to peak concentrations).

Response: The amount of data available was very sparse to perform point calibrations for peak concentrations. It is not uncommon to have pesticide concentration measured at a very coarse resolution using a few grab samples. Therefore, although possible, we believe that the model should not be calibrated for specific data points, especially when the number of data points is relatively small. Similar calibration procedures have been reported by others for SWAT model and atrazine and other pesticides (Vazquez-Amabile et al., 2006; Quansah et al., 2008).

Comment #4: Results of the simulation model are summarized into a simplified BMP tool. In general, this is a good approach; however the description on page 1836 makes it sound like only homogeneous BMP scenarios were evaluated (see assumption 1 on page 1836). That does not make sense, and it should be fixed or clarified.

Response: We are not sure what the reviewer means by homogeneous BMP scenarios. An example provided in page 19, lines 6-9 should make clear how multiple BMPs can be possibly placed in a single HRU in the watershed. As detailed in these lines of the revised manuscript, any BMP scenarios or combination of scenarios can be implemented through the BMP tool to evaluate water quality benefits at HRU level.

Optimization part Comment #5: The most important step in any optimization problem is specification of an appropriate objective function. I have two questions in this respect. First, since maximum pesticide concentrations are federally legislated, why not use that as a constraint and minimize cost in a single-objective formulation? Second, the pesticide control objective measures annual average pesticide concentrations. I think this is flawed, as one is interested in peak concentrations, rather than averages.

Response: This is an excellent point raised by the reviewer. It is possible to use federal concentration limit for atrazine to perform a constrained single objective optimization. However, the pesticide concentrations are not measured daily and therefore it is required to minimize the average concentrations of the pesticide in the watershed. Also,

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if the maximum pesticide concentration value is modified to have more stringent controls for atrazine, it would be easy to find the corresponding solutions from the Pareto-front than finding the solution after performing an additional optimization in the single objective problem.

Comment #6: Section 2 (theoretical background on the optimization algorithm) should be summarized and significantly shortened, and then included as a subsection under 'methodology'. That should generate space for a more extended description and discussion of case study, calibration, and results.

Response: The theoretical background section has been reduced in the revised manuscript. In addition, more discussion about the calibration and the results is provided in the revised manuscript.

Response to the comments of the Editor Comment #1: An optimisation algorithm should be tested on theoretical problems, in order to be confident that it is able to find the Pareto-front of the problem. If the algorithmic parameters are difficult to tune, this means that the algorithm is not suitable for the problem at hand (and the population size should also be considered as an algorithmic parameter).

Response: We agree with the editor's comment that the genetic algorithm parameters are sensitive. This is a limitation with the algorithm as it is a population based search technique and searches a large solution space to find optimal solutions. Therefore, the model parameters that are important for the genetic modification of the population and convergence of the algorithm are sensitive. However, the sensitivity analysis performed could explain how sensitive these parameters were to solve the multi-objective optimization problem.

Comment #2: I suggest paying more attention to the wording: the Pareto-front is a property of the optimization problem, the optimization algorithm tries to find it. Formulations as 'goodness of the front', or 'improvement of the front' are thus misleading.

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Response: This is a correct observation. Pareto-front represents the plot of the non-dominated solutions that the multi-objective optimization algorithm tries to obtain. Based on this suggestion, modifications have been made in page 22: lines 19-23 of the revised manuscript.

Comment #3: What is a dynamic linkage between an optimizer and a simulation model?

Response: A dynamic linkage of the watershed model with the optimization model is used instead of the BMP tool to estimate the pollution reductions at a watershed level for every new solution obtained during the various stages of optimization.

Comment #4: Pesticide calibration: the text implies that the sum of differences between observed and simulated pesticide concentration was minimized. This is not a good criterion as negative and positive differences can compensate.

Response: We thank the very good observation commented by the editor. The model actually uses the absolute value of the differences during the optimization and therefore the positive and negative differences do not cancel each other. This has been made clear in page 16; lines 15-17, of the revised manuscript "Therefore, pesticide calibration was performed in such a manner that the absolute difference between the total annual average of pesticide measured and simulated was the least".

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1821, 2008.

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