FEMTO-ST Institute, UMR 6714 CNRS
University Bourgogne Franche-Comté
IUT Belfort-Montbéliard, BP 527, 90016 Belfort Cedex, France.

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On the decision for the article
"Multiround Distributed Lifetime Coverage Optimization
Protocol in Wireless Sensor Networks"
by Ali Kadhum Idrees, Karine Deschinkel, Michel Salomon, and Raphäel Couturier

Dear Editor,

We were disappointed by the rejection of our article named: " Multiround Distributed Lifetime Coverage Optimization Protocol in Wireless Sensor Networks " for publication in the AD HOC NETWORKS journal. Indeed, we've started the submission process since September 2014. We obtained the comments of only ONE reviewer during July 2015. Its suggestion were very helpful and we incorporated them in the revised article submitted in September 2015. In particular, we have stopped the resolution of the Branch-and-Bound method after a time threshold empirically defined and we retain the best feasible solution found by the solver, as it was suggested by the reviewer. We made our best to carefully address the issues raised by the referee and revise your paper accordingly.

So, we would like to clarify some of the points raised by the reviewer, since some of its remarks seem to be not relevant.

1. The authors have partially taken into account the comments of my previous review. Additional content has been added, but these additions are sometime confusing.

ANSWER: The reviewer does not clearly indicate which addition is confusing.

2. The answer that the authors have provided to my comments should have been inserted into the paper, this is not always the case (i.e., my comment about the duration of the rounds).

ANSWER: We clearly indicated in section 3.2 that the rounds are of equal duration and we explained that this parameter should be set according to the types of application (see our answer in part "minor comments" in our previous answer).

3. In Section 3.1, all nodes are assumed to be "homogeneous from the point of view of energy provision". Why is such an hypothesis necessary? This assumption is likely to be satisfied only when the WSN is deployed for the first time, with new sensors. But after using the network for the first time, the aforementioned hypothesis is not likely to be satisfied again.

ANSWER: Maybe the honourable reviewer misread the sentence in section 3.1. : "We assume that all nodes are homogeneous in terms of communication

and processing capabilities, and heterogeneous from the point of view of energy provision.

4. The last paragraph of Section 3.1 is very confusing. It seems that the author attempt to propose area coverage instead of target coverage, but instead of defining "primary points" inside the area to be covered, they define points inside the sensing range of the sensors, that are obviously covered when the corresponding sensor is active. There exists works in the literature for area coverage, the authors should read them.

ANSWER : This remark is rather offensive. Of course we read literature for area coverage. We use the metric "Coverage Ratio" (defined in section 4.3.) to measure how much the area is covered. But the optimization process to decide which sensor has to be active or not in each round is based on the coverage of only a specified set of points called primary points. So the area coverage problem is transformed to the target coverage problem.

5. In Section 3.2, the reason why a subregion is defined in such a way that the distance between any two sensors in the same subregion is less than 3 hops is not justified. The reader is not told if the proposed protocol works better when the number of subregions is low or high. No algorithm for defining the subregions is given. It pertains to a clustering problem, for which a large number of algorithms exists, but without a sound definition of what is a 'good' partitioning for the proposed protocol, the reader cannot select a clustering algorithm.

ANSWER : ????????????

6. The ILP of Section 3.5 aims at addressing a bi-objective problem. Since full coverage is required, the ILP should first ensure total coverage, and then minimize overcoverage. The current ILP is not a proper formulation for reaching this objective, as undercoverage can be compensated by overcoverage. The primary and secondary objective are then mixed up into a single objective function with no interpretable meaning. Of course, an objective function value of 100 is better than an objective value of 101, but one cannot tell if a gap of 1 makes a major difference or not. The objectives should be distinguished, and the problem should be addressed as a multiobjective optimization problem.

ANSWER : As mentioned in the paper, the ILP of section 3.5 is based on the model proposed by F. Pedraza, A. L. Medaglia, and A. Garcia ("Efficient coverage algorithms for wireless sensor networks") with some modifications. The originality of the model is to solve both objectives in a parallel fashion: maximizing the coverage and minimizing the overcoverage. Nevertheless the weights w_θ and w_U must be properly chosen so as to guarantee that the number of points which are covered during each round is maximum. By choosing w_U much larger than w_θ, the coverage of a maximum of primary points is ensured. Then for the same number of covered primary points, the solution with a minimal number of active sensors is preferred. It has been formally proven in the paper mentioned above

that this guarantee is satisfied for a constant weighting w_U greater than $|P|$ (when w_θ is fixed to 1).

6. The content of Section 4.3, where different metrics are proposed to assess the solution quality, is a sign of a ill formulated problem: how to comment on the performance of an algorithm on a criterion (say network lifetime) if the ILP does not take this objective into account? The performances with these additional objectives are likely to be related to the ILP solver used: many optimal solutions to the ILP of Section 3 may have a very different impact in terms of these additional metrics. Hence, measuring them is pointless.

ANSWER : We disagree this remark. It is quite possible to optimize a criterion to have an impact on another one. In the problem formulation proposed here, the number of active sensors is minimized in each round for a maximal level of coverage. Limiting the activation time of each sensor has a direct impact on its lifetime and consequently on the network lifetime as shown in our experimental results. For example, such an idea is used in the models developed for brachytherapy treatment planning to improve the quality of a dose distribution ("Comparison of inverse planning simulated annealing and geometrical optimization for prostate high-dose-rate brachytherapy", I-Chow J. Hsu1, E. Lessard, V. Weinberg, J. Pouliot, *Brachytherapy* Volume 3, Issue 3, 2004, Pages 147-152): the objective in the problem formulation is to minimize a weighted sum of the differences between prescribed doses and obtained doses in reference points, whereas many criterion (like dose-volume histograms, conformal index COIN) are used for quantitative evaluation of dose plans.

We hope that these observations allow you to revise your decision concerning our manuscript. If possible, we would like to have comments of an additional reviewer.

Best regards
The authors