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Dear Editor and Reviewers:

Please find attached our revised manuscript entitled “A geographic routing approach for IPv6 in low-powered and lossy large-scale wireless networks”, previously entitled “A routing protocol for an urban IoT based on a smart street lighting wireless sensor network”. The change of the title was motivated by a valuable comment of one of the reviewers of the previous version of the paper. In the revised paper, we have attempted to address all the comments and questions raised by the reviewers of the previous version.

Most sincerely,

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**Reviewer #1:**

The authors have revised their manuscript, answering all questions and comments from all reviewers in the first round.

R: Dear reviewer, we thank you for the time taken to review our manuscript and for the valuable suggestions that helped us to improve the quality of the paper.

Without the focus on street light systems, the contribution of this research work should be accepted for publication. It improves routing protocols for random P2P communications in wireless sensor networks. However, the authors explicitly design and evaluate their routing protocol for street light systems, and hence the contribution in this manuscript should be evaluated by reviewers in this particular context, not in the general context of wireless sensor networks.

In the particular context of street light systems, the authors do not justify or define the concrete application that would require a street light system to send messages randomly from any street light to any other street light. In this context, the appropriateness of the simulated evaluation cannot be asserted. If the application is not related to the lighting system (eg, air pollution or traffic management), then the authors assume that the lighting system is only used by the application for its readily deployed infrastructure, not its capacity of lighting the streets. But in that case, the reference to the street lighting system should be anecdotal, somewhere at the end of the paper to justify how easy it could be to deploy a wireless sensor network in urban zones, based on the existing lighting system.

In any case, if the authors insist that their protocol is designed for lighting systems, then the design and evaluation should exploit the characteristics of street lighting systems. What is the difference between this context and a randomly deployed wireless sensor network in a rural area (eg, for military or agricultural purpose)? Wouldn't the routing protocol work with the same performance? Did the authors really design that protocol based on characteristics of street lighting systems? In that case, why are the buildings not taken into account for the signal propagation, instead of an idealistic disk radio coverage? Especially for a protocol involving geographical routing, the closest node geographically behind a building might not be considered as a neighbour, whereas a much further node in the same street might be the only neighbour, because of the better signal propagation in the same street. The proposed routing protocol should be evaluated in this realistic and particular context, since it is claimed to be designed for it.

R: Dear reviewer, thank you for such valuable comments. Indeed our intention to use the context of street light systems was to present a possible case for the application of the proposed routing method. However, as you pointed out, it should be applicable also in a randomly deployed wireless sensor network, not being specifically designed to the street lighting system. Therefore, in order to address your concerns, we have added to the text a note explaining that the context of street light systems used in the paper should be taken as one possible application where the proposed algorithm could be used. Also, we have changed the title of the paper in order to better reflect the main contribution of the paper. In this way we think to have addressed your concerns and hope you agree.

**Reviewer #2:**

All suggestions have been considered and now the paper is much more clear and interesting.

R: Dear reviewer, we thank you for the time taken to review our manuscript and for the valuable suggestions that helped us to improve the quality of the paper.

**Reviewer #3:**

The authors have satisfactorily addressed the majority of the concerns of my original review. I feel that they have addressed the concerns previously raised.

R: Dear reviewer, we thank you for the time taken to review our manuscript and for the valuable suggestions that helped us to improve the quality of the paper.

The detail added on the simulation environment used is somewhat a surprise - why use a custom simulator when so many already exist designed for this kind of thing. Ideal MAC and PHY layers are a luxurious assumption when designing the layer that depends upon and interacts with both of them.

R: Dear reviewer, thank you for such a valuable comment. Indeed our choice for using a custom simulator and the ideal MAC/PHY layers was motivated by the need of simulating a large-scale network in a reasonable time. Therefore, at this time, we have not investigated other performance metrics that would require more realistic MAC/PHY layers (e.g. packet delivery ratio), which we have left for a future work.

If accepted, the title MUST be reviewed, at present it does not make any grammatical sense. Also, it doesn't really reflect the contribution of the paper - it's not really a new routing algorithm, it's a combination of RPL and GOAFR (or an automated run-time selection between the two).

R: Dear reviewer, thank you for the suggestion. We have changed the title in the revised version in order to better reflect our contribution.