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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 manuscript presents a new routing protocol to be used for communications between urban street lights. The routing algorithm, GeoRank, mainly uses geographic routing but also exploits elements from RPL. The authors show simulations results outperforming RPL and geographic routing, depending on the radio range. The idea is sound and well explained. However it is not clear why a street lighting system would need every random light to communicate with another random light in the city, for example. The authors should present a clear application to motivate this scenario, and simulate this application to evaluate the routing protocol.

R: Dear reviewer, indeed it is not clear in the original text the need for P2P communication. Therefore, we try to clarify such issue in the section “Requirements and benefits of a smart street lighting system”. In addition, the P2P messages were considered in the simulation scenarios, in order to represent a completely random scenario of such messages, as described in the referred section.

Note that the title is misleading. This work only proposes a routing protocol, that could be used for lighting system (this has to be proven) or probably for other contexts as well. The title implies that the work is about a whole smart lighting system, whereas it is only about a routing protocol for wireless sensor network.

R: Dear reviewer, we have changed the title in the revised paper in order to better describe our contribution. Thank you for this contribution.

There are some typos and English mistakes to fix in the paper, for example:

- "On the other hand" instead of "By the other hand"

- "RPL provides" instead of "RPL provide"

- "in order for an actuator to make" instead of "in order to an actuator make"

- etc.

R: Dear reviewer, the typos have been corrected in the revised paper.

**Reviewer #2:**

-- Is it not clear what is the meaning of acronym CRI. Second paragraph

R: Dear reviewer, the meaning of CRI (Color rendering index) has been included in the revised paper.

Figure 2 is not clear at a glance. I guess the label mean cost,parent but this is not clear. Maybe if the LBR was located on the top part it would be more clear. And I think the letter in the labels are redundant. You could also explain in plain text in the previous paragraph, after referring the figure.

R: Thanks for the valuable comment. We embrace the suggestion of removing the redundant label as well as the suggestion to position the LBR at the top of the figure. Also, a little bit of plain text was added in order to better explain the cost label.

Section 4 should be much more "verbose", with plain text that explain both the algorithm and the diagram (figure 4). Algorithm 1 should be converted into pseudo-code. It is not clear what are the input and output variables of your algorithm. Step 3, for instance, is confusing, it should be split into more sentences or lines of pseudocode.

R: Dear reviewer, thank you for pointing these problems. Indeed, there are some mistakes in the previous version of the text in section 4. At first, we extended the plain text of this section. Second, we removed figure 4, which really confuses even more the understanding. Finally, we completely rewrote the algorithm based on some definitions (which elucidate the algorithm inputs) as well as we increased the sentences used to describe it.

Diagram of Figure 4. It is not clear the different between face and greedy GOAFR. It is not even clearly defined in the text. Please, clarify this point somewhere.

R: Dear reviewer, as previously explained, we completely rewrote the algorithm in order to clarify each step of the algorithm. We hope to have achieved this goal.

It is not clear which type of simulation was performed. If you developed your own simulator you could specify more details specially related to wireless link quality and ETX values for RPL protocol.

R: Dear reviewer, we have included more details about the simulations in the revised paper, including details about the simulator, wireless links model and RPL protocol implementation.

-- Finally, graph for Figure 6 does not show any higher order statistics, only the average values. You should include standard deviation or confidence interval if different seeds were used in your simulations.

R: Dear reviewer, confidence intervals have been added to the graphs in the revised paper.

You should also consider implementation of a real test case (small, no need for large testbeds) using real hardware and OS such as ContikiOS or TinyOS. This could contribute give more effective contributions to our community and confirm your results out of the controlled environment of simulators.

R: Dear reviewer, we thank you for the valuable comment. Indeed, we have initially implemented some parts of our routing protocol using the ContikiOS and experimented with it and its implementation of RPL using the Cooja simulator (the official Contiki simulator). However, we decided to not commit to this path, because we thought the contribution would be minor and too specific to a given implementation. Also, the results could be much more prone to errors due to programming bugs and flaws of the implementation. In our current setup, we could be sure that no errors of programming affected the reported results. Nonetheless, we have a plan to implement the proposed routing protocol using an OS, such as ContikiOS in the future. However, we think that the lack of the implementation in a real HW and OS does not devalue the routing protocol proposed as a contribution and hope you agree.

**Reviewer #3:**

The abstract identifies three novel contributions:

1) GeoRank finds shorter routes than RPL with high link densities, and GOAFR in low link densities.

2) GeoRank avoids bandwidth-consuming control messages required in RPL

3) GeoRank is more scalable in terms of memory usage than storing-mode RPL

However, it is hard to find results that substantiate these claims. (2) is not substantiated and quantified in the results; hence there is little evidence of this. I also cannot see any results providing evidence of (3).

R: Dear reviewer, we have included simulations results that provide evidence of (3) in the revised paper. Also, we have changed the item (2) to “GeoRank avoids DAO control messages required in RPL”, which is correct by design, because DAO control messages are disabled in the implementation.

The requirements of street light networks are not expressed. What demands do these put on communication networks? What kinds of topologies do they have? What kinds of traffic patterns do they exhibit? How often are packets transmitted? Discussion is had around "an example of the necessity of P2P messages is a scenario where actuators require data from sensors to decide when and how to act." What are the actuators and sensors in a street light network? Later in the paper it says that "approach is mainly suitable for large scale and heterogeneous networks, such as a network of wireless streetlight nodes" - why are street light nodes heterogeneous? Simulations are made conducted with "1000 random source-destination pairs": why would a streetlight in one street want to randomly communicate with a streetlight in a different street?

R: Dear reviewer, thank you for such valuable contributions. The answer for most of your questions are not in the original text indeed. Therefore, we try to clarify such questions in the section “Requirements and benefits of a smart street lighting system”. The main point is to demonstrate that this network is not only intended to be used in the street lighting application, but also in many other public services. However, the question “How often are packets transmitted” is highly dependent of these applications. What we and several other researches seek is to improve the P2P performance of the RPL based algorithms, which is currently the most promising approach to implement IoT applications at city levels.

Likewise, the detail in the experimental setup and results is lacking. What was the simulation setup: tool used, radio model etc? If there are only 5 different sink node locations, why pick 10 random locations in the network (rather than 5 actual locations)?

R: Dear reviewer, we have included more details about the simulations in the revised paper, including details about the simulator, wireless links model and RPL protocol implementation. Also, we have included an explanation about the locations for the sink node.

Figure 6: is the 'mean' the best metric to present here? Is the RPL implementation compared against the original RPL, or the enhanced version proposed by Winter et al., 2012? Why is GeoRank compared to RPL and GOAFR rather than the improved CDF technique?

R: Dear reviewer, we think that the “best metric” is highly dependent of the applications that will use the network. Therefore, in order to make a comparison possible, we have decided to use the mean number of hops because it is one of most common metrics found in the literature for comparing routing algorithms. However, the best metric will be application dependent.

In the revised paper, we have also included confidence intervals to the graphs, more details about the simulations, including also more details about the RPL implementation and more results regarding the worst case memory requirements of the algorithms for storing routing tables. However, because a comparison of CDF with GeoRank will require much more space and this paper is already long, we decided to leave it for a forthcoming paper.

What other routing schemes exist/are suitable for streetlights? Why is IPv6 needed/a good candidate? It is not clear to me why IPv6/6LoWPAN is needed/suitable for streetlight systems - the only real motivation given is "The IP protocol is an open, mature and field proven technology, with free and well documented standards, and thus it is very reasonable to reuse it for WNSs instead of creating new proprietary protocols". This is not a clear argument. Also, in this case, why do the authors propose using a proprietary routing algorithm - why not use something like AODV?

R: Dear reviewer, thank you for the valuable comments. In the revised paper, we have rewritten this part of the text and we have included the motivations for using the IPv6/6LoWPAN protocol. Please note that in the paper we are supporting the IPv6/6LoWPAN protocol deployment because it is also a tendency towards the standardization of the “Internet of Things” according to the references that we have added in the paper in support to this view. Nonetheless, the use of IPv6/6LoWPAN standard is optional and the proposed routing protocol does not depend on it. Moreover, regarding the use of AODV, indeed there is already a proposal for a protocol derived from it targeted to LLNs, known as LOADng (LLN On-demand Ad-hoc Distance vector routing protocol). However, recently published papers, like this one (Proactive versus reactive routing in low power and lossy

networks: Performance analysis and scalability improvements (<http://www.sciencedirect.com/science/article/pii/S1570870514001243>) have reported results showing that such protocol has worse performance when compared to RPL according to several metrics, including average delay, control overhead and memory requirements for storing routing entries. Nonetheless, we have included in the revised paper a reference to LOADng protocol and to some papers that have reported comparison results of RPL with LOADng.

There is a need for more interaction with the literature in some sections, for example there are none to support the 'drawbacks of PLC technology'. It is claimed that "the scalability and reliability of cellular networks are questionable", but this is not referenced or substantiated. Cellular networks have done pretty well for themselves, I think this is a little dismissive!

R: Dear reviewer, we agree that in some cases we seem to be a little dismissive. However, we correct some affirmations based on literature references and, in other cases, we argue about the possibility to integrate different types of networks in order to provide a more comprehensive network.

What is the problem of adapting RPL in streetlighting systems? Page 8 claims that RPL exchanges very few DIOs when the topology is stable. Street light networks are very stable - they're not going anywhere!

R: Dear reviewer, thanks for the valuable comment. The fact that streetlights are static is one of the motivations to use the position of the nodes for routing. In this way, it is possible to reduce memory consumption for storing routes and bandwidth consumption for the transmission of DAO messages. However, geographic routing may use longer routes than RPL, thus it makes sense to propose GeoRank for reducing the length of the routes using the RPL DODAG whenever it can be helpful. Thus, GeoRank tries to join the best of both methods (i.e. RPL and geographic routing). We have included more details about this in the revised paper.

In section 4, low and high link density is mentioned, but it is not clear how to differentiate a low link density to a high link density and how it is modelled during the simulations. The last paragraph of the results section explain that "in a smart street lighting [network], there may be some places where the link density is very low due to obstacles". Were obstacles simulated in the simulation environment? How low is very low? Generally, a street light will have visibility of some other nodes in the same street?

R: Dear reviewer, the link density is modelled in the simulations by the radio range, the distance between neighboring nodes, and the constrained placement of nodes, as the nodes are not allowed to be in the regions enclosed by the streets. Therefore, the obstacles have been simulated by the constrained node placement. Very low density is the density obtained when the radio range is set to the minimum value required to keep the network connected. The network is connected when, for any two nodes in the network, there is at least one path connecting them. We have explained this in the revised paper.

The last sentence of the conclusions is speculation.

R: Dear reviewer, that sentence has been removed in the revised paper.

The abstract has a lot of acronyms in it, which are obviously undefined. Are they all needed?

R: Dear reviewer, we have improved the abstract in the revised paper. Also we have included the meanings of the acronyms.

The English needs a \*lot\* of work, if accepted, as there are numerous spelling and grammatical errors including:

"…streetlighting system based on AN ipv6-enabled wireless…"

"…the main drawbacks of THE rpl routing protocol…"

"It is responsible FOR guaranteeing 'the society security and living during nighttime'…" (part in '' makes

little sense)

"mesopic visual conditions", "PLC", "RFCs" and "CRI" are not defined/explained on first use

"…to reuse THE existing ipv6 protocol for THE networking layer…"

etc etc

R: Dear reviewer, thank you for pointing the errors. Those errors have been corrected in the revised paper.