Dear Mr. Rager,  
  
We regret to inform you that your paper:  
  
Scalability and Satisfiability of Quality-of-Information in Wireless Networks  
  
paper#  1570139233, cannot be accepted for publication as a full paper at The 12th IEEE International Conference on Mobile Ad-hoc and Sensor Systems (IEEE MASS 2015).  
  
The conference received 188 submissions of which only 49 full papers were accepted. The reviews are now available on EDAS. We hope you will find them helpful, and will attend IEEE MASS 2015 in Dallas, TX (USA).  
  
While your paper cannot be accepted in full, you might want to consider submitting your work to the poster and demo sessions. Links for submitting poster and demo abstracts will be posted on the conference website shortly. (These are 2-page submissions of the same format as full papers, describing the results briefly.) We would like to encourage a submission on the topic of your paper.  
  
We thank you for your interest in the conference and look forward to meeting you in Dallas.  
  
Sincerely,  
  
Mingyan Liu  
IEEE MASS 2015 TPC Chair  
  
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======= Review 1 =======  
  
> \*\*\* Recommendation: Your overall rating.  
Borderline (top 30%) (3)  
  
> \*\*\* Contributions: What are the major issues addressed in the paper? Do you consider them important? (Pls. comment explicitly on the relevance of the paper to MASS topics, the technical depth and the importance of the problem addressed.) [Be brief]  
  
The major issue addressed in this paper is to increase the scalability of networks by taking into account the completeness and timeliness of information.  
Taking into account Quality-if-Information is certainly an interesting approach from a theoretical point of view, but may be hard to realise in real-life situations.  
  
> \*\*\* Strengths: What are the major reasons to accept the paper? [Be brief]  
  
The paper is well structured. The problem is well-formulated and the analysis is clear.  
  
> \*\*\* Weaknesses: What are the most important reasons NOT to accept the paper? [Be brief]  
  
Although an interesting problem, the application in real-life situations is doubtful. An experimental Proof-of-Concept validation is missing. The simulations (considered as experiments in this paper) use a very simplistic model for wireless communication.  
  
> \*\*\* Detailed comments: Please provide detailed comments that will be helpful to the TPC for assessing the paper, as well as feedback to the authors.  
  
- The introduction can be improved. While the remainder of the paper is well-structured, the introduction is more chaotic. I recommend to split the introduction in 2 parts. In the first part the main motivation + contributions can be described, while the second part should then explain organisation of the paper in different sections.  
  
- often the term 'network instance' is used, while it not explained what is meant by instance. It becomes clear while reading the paper. It would increase the readability to explain 'network instance' when it is introduced for the first time.  
  
- The font in some of the figures is quite small and hence not readable when printed  
  
- Please don't use the term 'experiments' when you refer to simulations  
  
- The numbering of the nodes in Figure 2 is confusing. Apparently the numbering is not referring to nodes, but to time slots. Please clarify in the text or figure caption  
  
- section IV B: a drawing of the different network topologies (+ indication of time slots) would improve readability. Now only reference is made to previous work. It would be certainly helpful to better understand the parameters in Table 1.  
  
- the authors claim that experimentation is a difficult task. Nowadays many large-scale open testbeds are available for experimental validation (see for example [http://www.ict-fire.eu/getting-started/fire-testbed-search.html](http://www.ict-fire.eu/getting-started/fire-testbed-search.html" \t "_blank)), reducing the complexity of experimentation to the level of simulation. A proof-of-concept validation in a realistic wireless setting would be more convincing, as the theoretical analysis and the simulations makes many simplistic assumptions. Through experimental validation it would be more easy to compare TDMA versus DCF MAC.  
  
======= Review 2 =======  
  
> \*\*\* Recommendation: Your overall rating.  
Borderline (top 30%) (3)  
  
> \*\*\* Contributions: What are the major issues addressed in the paper? Do you consider them important? (Pls. comment explicitly on the relevance of the paper to MASS topics, the technical depth and the importance of the problem addressed.) [Be brief]  
  
This paper identifies completeness and timeliness as quality of information (QoI) metrics in WSN applications, and proposes a model that can be used to 1) predict if QoI requirements are achievable given a network of a certain size, as well as 2) what are the regions of QoI requirements and network sizes that are achievable. The paper uses simulation to validate that the proposed model is realistic for simple types of network topologies, such as cliques, lines and grids.  
  
> \*\*\* Strengths: What are the major reasons to accept the paper? [Be brief]  
  
- model agrees with simulation results (this is validated in the context of simple network topologies)  
  
- model can be applied to different application scenarios (though only for simple network topologies)  
  
- interesting topic that connects satisfiability of quality of information and scalability  
  
> \*\*\* Weaknesses: What are the most important reasons NOT to accept the paper? [Be brief]  
  
- model only applies to networks using TDMA protocols; contention-based protocols which are far more common are not covered  
  
- unrealistic assumption: model assumes that the completeness quality of information metric directly reflects to a number of messages (either from the same node, or from different nodes) that is easy to estimate; this is not the case in realistic settings and this could vary over time  
  
- the analysis is specific to the topology and does not easily extend to arbitrary topologies (lack of generality)  
  
> \*\*\* Detailed comments: Please provide detailed comments that will be helpful to the TPC for assessing the paper, as well as feedback to the authors.  
  
This is an interesting paper on how to connect satisfiability of quality of information and scalability. This is an important problem since it allows network engineers to estimate if certain quality of information requirements are satisfiable given a certain network, as well as what type of network would be required to be able to satisfy certain quality of information requirements. Also, achievable regions that combine quality of information and scalability attributes are explored.  
  
Although this is an interesting attempt to address an important problem, the proposed model is very specific to the network topology. Hence, it is not clear if it can be readily applied to the real world where topologies are not simply cliques, or lines or grids.  
  
Also, it is demonstrated only in the context of TDMA protocols which are not as common as contention-based protocols. It is not clear how the analysis can be extended to contention-based protocols or not.  
  
Finally, it is assumed that it is easy to infer the number of messages needed to satisfy a completeness requirement (this information is provided by a node, or assumed to be known by the network). In practice when a sensor network is monitoring a spatial field, the number of nodes that need to be queried to satisfy an accuracy requirement depends on the distribution of nodes, their density in different areas and the fluctuation of the physical phenomenon over space and time.  
  
There is a plethora of work on sensor set selection, which is not even mentioned in the related work, but which is highly relevant. Typically such papers use information theoretic metrics such as mutual information, and so on. Also, there are recent works that combine the sensor set selection  and the scheduling problems and try to optimise both at the same time. The assumptions in this paper are more naive than in state of the art papers on sensor set selection and scheduling, and it would be useful to significantly extend the related work to refer the reader to such studies.  
  
======= Review 3 =======  
  
> \*\*\* Recommendation: Your overall rating.  
Weak Reject (top 50%) (2)  
  
> \*\*\* Contributions: What are the major issues addressed in the paper? Do you consider them important? (Pls. comment explicitly on the relevance of the paper to MASS topics, the technical depth and the importance of the problem addressed.) [Be brief]  
  
The paper proposes the term Quality of information as a measure to evaluate performance in wireless networks.  
  
> \*\*\* Strengths: What are the major reasons to accept the paper? [Be brief]  
  
The proposed metrics are somewhat new, i.e. it was not yet proposed exactly as it is now.  
  
> \*\*\* Weaknesses: What are the most important reasons NOT to accept the paper? [Be brief]  
  
The relation to QoE is minimal. Important related work areas are ignored. The motivation for QoI is not convincing. poor performance evaluation.  
  
> \*\*\* Detailed comments: Please provide detailed comments that will be helpful to the TPC for assessing the paper, as well as feedback to the authors.  
  
Why do we need QoI. Why is QoE not sufficient ? in particular, there are many metrics to evaluate picture / video quality, e.g. SSIM, VQM, PSNR etc, i.e. using objective QoE measures. How to measure certain criteria mentioned on page 1 left column, bottom? Why is bandwidth and delay not sufficient / equivalent to {C, T} ?  
Completeness should not be the only criterion since it matters a lot which data will be delivered and which not, consider different importance of I, P, B frames in pictures. QoE work does consider that in contrast to the paper !!  
The purpose and meaning of QoI regions remains fuzzy.