Paper Review

Paper: Coupons: A Multilevel Incentive Scheme for Information Dissemination in Mobile Networks

Familiarity: Some knowledge

Recommendation: Definite accept

Strengths:
1. Presents an original idea to solve a known problem
2. Provides a simple but robust solution to the problem
3. Promises to have a significant impact on future applications in the field
4. Excellent evaluation and shows that the solution performs well
5. Very well structured and easy to understand

Weaknesses:
1. A bigger testbed implementation would have helped
2. Implementation on mobile phones would have much more insightful

Detailed comments:

The main goal of 'Coupons' is to explore the 'human factor' aspects of mobile networks and to facilitate opportunistic interaction and information dissemination between users over wireless medium. It incorporates adaptive algorithms for determining the broadcast frequency and a new incentive scheme to encourage participation of nodes in the network. The main contribution of the paper is the idea of introducing multiple levels in a coupon (a level is added with every forwarding) and then rewarding the users who participated in forwarding, according to the . I think the paper provides a novel and elegant solution to the known and often attacked problem and the authors have highlighted clearly the differences between the solutions proposed earlier and what they lacked. Furthermore, as the authors mention it, "the concept can be applied to a whole range of new applications" which highlights its importance.

The algorithms proposed have been presented extremely well with the static algorithm (forwarding done with a predetermined fixed probability) described first and then the two adaptive algorithms (Traffic based and ACK-based), leading to a good understanding of the same. I particularly like the way the algorithms were derived from the established SIR mathematical model of describing the spread of an epidemic disease. This assures us that the algorithms proposed would be based on an already established baseline. It also indicates that good effort has been put in to throughly investigate the possible algorithms. The algorithms proposed are simple, easy to implement and efficient (in case of ACK-based scheme, as proven by the evaluation).

Apart from the simple algorithms suggested in the paper, their evaluation is excellent as well. First of all, the authors have described clearly the methodology, goals, assumptions and parameters of their evaluation which helps in understanding it and putting it in context with the idea. Secondly the authors have been very thorough by doing both simulations (on GloMoSim simulator) as well as later implementing on a testbed. The measures chosen in the simulations: network efficiency and spreading efficiency, are perfect to analyze the system and the results show that the the ACK-based scheme proposed has the lowest network load (and hence high efficiency) and relatively high spreading efficiency (if not the highest). However, I think it would have been helpful to also include a metric of memory footprint in the simulations. The idea of 'Coupons' holds high potential for use in mobile phones and memory footprint is a bigger issue in that domain as compared to a normal mobile host (like a laptop). Also, on page 8, the authors mention that one disadvantage of the two schemes is that they need to store the coupons received previously in order to detect duplicate coupons and that limiting the number of stored coupons on a node will not help performance. An evaluation describing the performance of the system with respect to memory constraints would have been insightful.

Also, as I mentioned before, the proposed scheme would be more helpful for mobile phones than laptops which people generally do not carry around with them. Also, mobile phones would have a different mobility pattern than laptops. Hence I think a testbed implementation for mobile phones would have been very useful. It would however have required an implementation of the schemes in the mobile phones which undoubtedly is more complex and maybe would have needed more time.

Paper Review

Paper: Real-Time remote rendering of 3D video for mobile devices.

Familiarity: Novice

Recommendation: Likely accept

Strengths:
1. Provides a novel solution to a new problem
2. Contains a very sound technical part
3. Provides a good background and related work to build an understanding of the system

Weaknesses:
1. Lacked detailed explanation of some technical terms which makes the paper hard to understand for someone not well versed with the subject.
2. Evaluation section needs more work

Detailed comments:

The paper presents a novel approach to render 3D video in real time for mobile devices with a reduced user interaction delay. The system proposed in the paper builds upon TEEVE (Tele-immersive Environments for EVErybody): a system to capture a 3D video, reconstruct it from the real world and render it in real time. This work extends the idea of TEEVE to mobile nodes addressing the challenges that come with a mobile wireless network. The paper describes the TEEVE system to extensive details, thus laying down the groundwork to understand the ultimately proposed system.

The authors have clearly identified the problems that are associated with the mobile wireless medium (bandwidth bottleneck and computation power) and propose to use proxy based remote rendering to address

them. I particularly liked the background and related work information provided in the paper which helps us understand the concepts behind 3D video and remote rendering. However, I think at several places, a better explanation of technical aspects would have helped. For example, it will be rather hard to understand for an outsider (not well versed with the area) what the authors mean by 'convex attribute' on page 4. Also, in some places the explanation is a bit fuzzy, for example on page 3, the authors mention that interaction delay becomes an issue if the proxy only sends the rendered 2D images. No clear reason was provided to support it.

The image based approach to constraint the user interaction delay has been explained very well. Again, the background necessary for the approach has been described well in detail (3D warping). However, the evaluation methodology should have been detailed out a bit more, explaining the parameters, measures and assumptions in more details. Also, evaluation should have been performed on more than one mobile phone device to generalize the results.

Confidential comments to the TPC chairs:

I do not have much idea of research in this field and hence was not aware of many technical terms and concepts required to deeply understand and appreciate the paper. Despite my lack of knowledge, I firmly believe that the idea is new and solves an important problem in the field, thus contributing significantly in research in the same.