

S. Khan, et al., "Application-Driven Cross-Layer Optimization for Video Stream Over Wireless Networks," IEEE Communications, vol. 44, no. 1, January 2006.

This paper starts off with a good introduction to the problem the authors are attempting to solve. The authors explain that with the expanded use of mobile devices and increased demand for multimedia from these devices, a change is needed to the standard layered approach which is traditionally used in networking since the approach is "no longer adequate to meet the challenges of next-generation mobile systems". The authors state that multimedia communication is extremely challenging due to the "time varying transmission characteristics of the wireless channel and the dynamic quality of service (QoS) requirements of the application". Following this, there is a brief discussion of different attempts at cross-layer design (CLD) and the authors' proposed CLD (a combination of the top-down and bottom-up approaches).

The introduction was fairly straight forward except for a few things. First, the authors mention beyond third-generation wireless networks without any information as to how these are different than traditional wireless networks. I thought this section could have benefited from such information. Second, I believe the authors should have included a "related work" section rather than attempt to describe sources [6]-[13] with one sentence each. Had they included this, it would have been much clearer to the reader why a new design is needed to solve the aforementioned problem. Lastly, it is mentioned that this study only includes application, data link, and physical layers. The authors should have briefly mentioned why they felt the other layers were irrelevant.

The next several sections of the paper dive into the details of the paradigm that the authors are proposing. It is explained that abstractions are used for the different layers' parameters. This is done in order to reduce overhead at the optimizer which would occur because of the numerous parameters contained in each layer. Another reason for the abstraction is to make the design of the paradigm more general such that it is possible to use the same optimizer across different systems. Following a short description of some of the parameters, the authors discuss cross-layer optimization for wireless video streaming. This section starts with an analysis of distortion including a chart that shows the mean square error experienced by a group of pictures depending which frame in the group is the first one that is lost (figure 2). This analysis serves as somewhat of a build-up to one of the key components of the proposed CLD – dynamic resource allocation across multiple users. The paper then goes into more detail about the abstraction that happens at the different layers of interest, followed by some details on cross-layer optimization.

In general, I felt that these sections were lacking a lot of detail and took too much for granted. The description of the parameters in the cross-layer architecture section did not seem to provide much insight as to the usefulness of the parameters and the examples provided did not clear up any of the confusion. Additionally, even though the explanation of distortion and mean square error was good and figure 2 helped the reader understand this concept in more detail, the authors failed to differentiate between the three trend lines. It would have been interesting to know what about the different video sequences made for the differences in their MSEs. Also, the authors make a pretty bold statement in this section: "Applying cross-layer optimization to multiple layers, including the application, network,

data link, and physical layers that directly interface with the dynamically changing environment, allows for optimal adaptation of the network". I feel like some justification was needed to convince the reader why this is a legitimate statement.

The subsection titled "video streaming scenario and architecture" failed to provide any useful information. I do not believe that the cross-layer optimizer had been explained well enough up to this point to be included in an example. This subsection also mentions a "the transition probabilities of a two-state Markov packet burst loss" but does not explain what this is even though it claims it will do so. The authors should have referred the reader to a source that could explain this concept appropriately. Also, the use of figure 3 in this subsection is completely useless. It does not help the reader understand what the authors are trying to depict in any way. Finally, in the "cross-layer optimization" subsection, it is said that the CLO "selects for each GOP the optimal parameter values that maximize the expected user-perceived video quality." I am wondering why instead of explaining how this parameter is selected, the authors instead discuss a calculation of distortion.

The final few sections of this paper talk about results of a simulation performed by the authors as well as an analysis of performance and cost. I thought these sections were a bit confusing and failed to solidify what the authors were attempting to show. Figure 4 in particular is very hard to decode and the explanation of its various sections is not detailed enough. Though the inclusion of figure 5 was a nice way to demonstrate the performance analysis, I felt like more comparisons should have been provided for the reader to truly understand the gains attainable by implementing such a paradigm. Discussing only one experiment is not nearly enough to draw concrete conclusions about anything. I did appreciate the paper including a cost analysis section. I think that this is an area that is often overlooked in other papers. It is nice to suggest ideas and write papers about them but when it comes down to it, people are interested in making money so if deploying such a paradigm proves to be too costly, it will be dropped no matter what performance gains it promises.

Overall, I thought this paper was well written and clearly had the potential to make a large contribution to the field. In general, it was lacking more detail about the different aspects of implementing the paradigm – details that I feel would help the reader understand the value of this idea much more. The paper also should have done a better analysis of the paradigm or at the very least talked about more downsides to it since in reading the paper, it seems like the proposed solution is flawless.