

# CS290F - Paper Reviews for 2010.02.09

## Review for "Application-Driven Cross-Layer Optimization for Video Streaming over Wireless Networks"

**S. Khan, Y. Peng, E. Steinbach, M. Sgroi, and W. Kellerer. *IEEE Communications Magazine*, January 2006.**

Two things struck me as I read "Application-Driven Cross-Layer Optimization for Video Streaming over Wireless Networks": first, the article appears to have put very little thought or effort into identifying a target audience; second, there doesn't *really* seem to be a major contribution made by the article. Despite these (fairly major) flaws, the paper does serve as a decent starting point for further investigations into cross-layer design, due mostly to the many references included in the short article. The remainder of this review serves to address each of these claims in turn.

Before attempting to address the claim that the paper doesn't identify a target audience, let's consider *why* a paper should identify a target audience: most importantly, it helps the author(s) structure the paper with a consistent theme, which makes the paper accessible to the audience reading the paper, as ideas from the beginning of the paper are tied into ideas from the end of the paper. Also, clearly identifying a target audience helps the paper gain an audience interested in reading the paper -- without a clear identification of the audience, the paper doesn't manage to attract readers that are guaranteed to be interested in the topic and readers from outside the field don't approach the paper with the proper mindset.

In this particular paper, the abstract concentrates on concepts such as the various layers involved in application communication and the introduction to the paper continues to support these ideas, making the paper seem to be targeted towards network researchers, but as soon as any details are approached, the paper quickly dives into video encoding and video architecture concepts much more agreeable to those involved in multimedia research.<sup>1</sup> This lack of self identity makes the paper extremely difficult to approach for a reader more familiar with networking topics by forcing the reader to re-evaluate the paper from a different perspective after reading a section focused on multimedia encodings.

The lack of a self-identity in a paper may make the paper less-widely read or less accessible to readers, but it doesn't necessarily mean that a paper isn't entirely without merit (it just makes the paper difficult to read and less likely to provoke interest or further research). What makes a paper worthwhile is the contribution made by the paper. For this reason, it is imperative that a paper clearly identify the contribution it makes: which this article ultimately fails to do.

The abstract and introduction of the article claim that the article introduces a novel cross-layer optimization approach to wireless video streaming. However, the explanation of the cross-layer design falls apart as it is buried under the abstraction of a "cross-layer optimizer". Essentially, the entire contribution of the paper is this optimizer, and yet, all that

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1. Interestingly, this happens to make it fall right into the "Topics in Multimedia Networking" arena, but the fact that it happens to nicely fit a special topics graduate course at UCSB doesn't make the lack of an identified audience a good thing.

this optimizer seems to be is an application-level routine that tunes parameters in the network stack based on how much error is introduced into the application data stream presented to the end user. The software industry has been using similar techniques (updating transmission rate or frame size, etc.) for a long time when delivering content to end users. There may be something to the particular combination of parameters used to react to video encoding/decoding problems, but the idea of dynamically tuning a connection doesn't seem particularly novel or inventive.

Finally, despite containing a reasonable set of reference, the paper frequently makes claims that are only weakly supported by some esoteric hand-waving, if at all. To pick one representative example, the claims made in the introduction that "the layered approach has been widely used in the past, but is no longer adequate to meet the challenges of next-generation mobile systems" is followed by some of the particular problems in mobile computing, but no evidence is given that a layered approach is in any way *inadequate* to solve these problems. In fact, it seems altogether *likely* that these problems will be better and better addressed as the network-hardware and protocol *layers* continue to improve in performance and ability.