

Commentary on the study “The overrated power of background music in television news magazines: A replication of Brosius’ 1990 study”

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This is the first time that I have had the chance to write a commentary on a replication study. I want to applaud the editors of *Musicae Scientiae* for their willingness to give replication studies more room. Everybody talks about the necessity of replicating scientific results, but hardly anybody does these studies (and gets them published).

The original 1990 study was driven by the idea that “real” and perceived information gains were influenced by different cognitive mechanisms. Music contributes to a positively perceived atmosphere and gives people the impression that the makers of the film/tv news want to help them digest the information. This possibly reduces the cognitive effort invested by recipients. “Raw” information is perceived as less supporting but actually demands and gets more cognitive processing on the side of the recipient. My later conclusion – not explicitly discussed in my paper – was that music is not the only element having a negative influence on information processing and a positive influence on perception of the film and on subjective feelings of being informed. The graphic design of a PowerPoint presentation may serve as another example. Students like the moving, even exploding, 3-dimensional graphical elements which do not carry a lot of information. I remember vaguely having read studies about that. Unfortunately, I have never pursued these lines of thought any further.

The replication study partly supports the general notion that music is *perceived* as being supportive of information but actually *isn't*. The study shows how communication and psychology research have developed higher methodological standards. The study is more carefully designed in terms of (1) power analysis to determine sample size, (2) systematic selection of different types of music, (3) carefulness of the experimental design and procedure, and (4) advanced statistical procedures. The study also has a more solid theoretical framework. My original study collected different pieces of evidence for the hypotheses; the present study uses a pre-existing theoretical framework, namely the Elaboration Likelihood Model of Petty and Cacioppo, which I was not familiar with when I conducted the study, although I could have been.

The replication study differs from the original study in two important aspects. First, the experiment was conducted using an online survey. Obviously, this was not possible in 1990.

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According to the shift in modality, the reception situation was different. My recipients were seated and tested in small groups in a semi-public setting; Kopiez et al.'s recipients were tested individually in a private setting. One can only speculate about the effects of these differences. Second, I used information films as stimuli, while Kopiez et al. used mass-mediated TV news. These two types possibly vary in terms of their relevance, actuality, external validity, and the credibility of the experimental variation. Again, one can only speculate about the effects of these differences.

It can easily be concluded that much more research is needed to clarify the effects of all possible intervening variables. Most likely, more than one replication study would be necessary. How much research is invested in such an area like music psychology is the result of both individual decisions of researchers and societal attribution of relevance to the topic (as, for example, reflected in funding decisions).