

**On Wober's "Televised Violence and Paranoid Perception:
The View from Great Britain"**

WE welcome J. M. Wober's interest in and contribution to what he calls our "major new perspective" in television approach. Wober's study (*POQ* 42:315-21) claims to summarize and replicate our research on television effects, and fails to confirm our findings—or, we feel, to disconfirm them.

We are surprised by the murky reasoning and dubious comparability of his study. We are even more struck by the haste with which this article was rushed into print without bothering to obtain the methodological details that Wober claims were missing, or additional data that might have answered some of his questions. A simple inquiry could have elicited our Technical Report which was listed in the publications Wober had used. We could also have sent him the manuscript of our most recent major report which was published in the Summer 1978 issue of the *Journal of Communication* under the title "Cultural Indicators: Violence Profile No. 9."

As Wober seems to be less than adequately informed of many aspects of the research he claimed to have replicated, we shall limit this review to a few of the most obvious errors.

Our research deals with responses to questions about social reality by heavy and light viewers (controlling for demographic characteristics) of American network dramatic programs in which about 8 out of 10 contain some violence. Wober's replication involved programming in Britain, where, according to his article, only some 15 percent is occupied by American programming—the kind of material that is responsible for our findings, and only 10 percent of shows are "violent."

The violence counts he reports in Britain are based on "Gerbner's violence definition, the description of each item in the program journals, and the advice of experienced program administrators." This is hardly comparable to our method, inasmuch as our definition is applied by pairs of trained coders (not administrators) to videotapes of actually broadcast materials (not listings). The lack of an appropriate content study—his or another researcher's—clearly reduces the strength of his assertions. His misconceptions are also revealed by his claim that our hypotheses rest on "the absolute levels of violence viewed." This is simply wrong; we have never said this nor done this type of analysis. Moreover, even if only 15 percent of British screen time is filled by "American programming," what of the other 85 percent? If the vast majority of British TV is indeed "nonviolent,"

then it is certainly not likely that TV would have a "paranoid effect on viewers," and his findings may actually support our hypotheses.

In any case, Wober's fundamental misunderstanding of our theoretical perspective is evident in his implications that we say watching television makes people "paranoid about violence." Although most of our published work has been related to violence, it is a small part of our overall approach. We see television's messages as a system, containing and cultivating coherent images of life and society. Our message data archives have enabled us to conduct a large number of studies of viewers' conceptions of many aspects of social reality, such as beliefs about aging, images of occupations, political socialization, and the roles of women, children, and minorities.

His replication of the cultivation phase of our analysis has its own puzzles. What is the *N* of the diary sample? The data used for Table 2 are of problematic origin. The national sample seems to have been divided; one group was asked an "unsafe" and a "trustworthy" question, the other a "safe" and an "untrustworthy" item. He reports that distributions were similar for the first subsample, but not the second, so only the first was used. Why? This would reduce the *N* from 1,113 to about 550. Yet the *N* in Table 2 *seems* to be only 257 (he tells us that *df*=256). He also never tells us how many heavy and light viewers there are or defines light viewing. Further, if our questions are reworded (to make them "comprehensible to British subjects"), they should be reported, verbatim. His question which focuses on robbery may tap an entirely different dimension from ours, which concerns violence in general.

Finally, we agree that our correlations are small but they are statistically significant and consistent across samples of children, adolescents, and adults. That other variables may account for more variance than TV does not deny television's independent contribution. While we would not ask "journalists in Britain"—or anyone else—to accept our conclusions "uncritically," Wober's piece of wishful thinking is not a straightforward replication on either methodological or theoretical grounds; it neither supports his allegations of flaws in our work nor negates our findings.

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Editors Note: A reply by J. M. Wober will appear in the Summer Issue.