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Dr. Gerbner:

I am a graduate student in the sociology department here at Princeton. I have been interested in your work for some time now and thus read with great interest J.M. Wober's Public Opinion Quarterly article reporting his effort to replicate your work in Great Britain. I have also read the exchange of replies that followed in that same journal. In general I would agree with many of the reservations you expressed about the Wober research, though there is an important criticism of the article that I fear you may have overlooked. If you are not aware of it, please allow me to call it to your attention.

The clearest indication that Wober is probably tapping an entirely different dimension from yours is the fact that according to his "security scale"(See Table 2 of his article) lower class respondents on the whole feel more secure than their upper class counterparts. This is contrary to what your data show, and is also substantially at variance with a wide range of stratification findings(especially in the social class and mental health literature.) It would also come as quite a surprise to those who have closely studied and reported on how members of the British working class perceive the world(e.g. Richard Hoggart). If Wober's questions measure what he says they do, his results would probably qualify as an important finding in stratification research. It is more likely, however, that they reflect a fundamental flaw in his measurement instrument. On this ground alone, I believe Wober's work must be considered seriously suspect.

Let me add here that my own analysis of the 1976 election data has lead me to the conclusion that the cultivation effects of television are, especially with regard to mistrust, even stronger than your published work suggests. Yet while this analysis convinces me that the effects you discuss are real, it(along with Doob and MacDonald's recent findings) also lead me to the conclusion that exposure to television violence per se is probably not the most important factor underlying it. As one part of a dissertation now in progress, I am working on a somewhat different explanation for why heavy experience with television drama might be linked to the cultivation of such effects. Much of my approach grows out of E. Goffman's work on the origins of interpersonal suspiciousness, and also involves a close look at certain structural features common to most television dramas. I think you might find what I am working on quite interesting, and more importantly that it nicely complements your efforts to identify the coherrent images of social life disseminated by television drama. If all goes well I will be finished sometime this Fall, and if you would like will forward you a copy.

Finally let me take this opportunity to express my admiration for the work of you and your colleagues. It is clear to me that the effects you are uncovering are both real and extremely important.

Sincerely,

Thomas J. Neville

Thomas J. Neville

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 research. Wober's study, published in the Fall 1978 issue of POQ under the title "Televised Violence and Paranoid Perception: The View from Great Britain," 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 has 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 British programming where, according

to his article, only some 15 percent of screen time 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 "non-violent", 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 1113 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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