

Comments and Letters

On the Limits of "The Limits of Advocacy Research": Response to Hirsch

IN "Some Additional Comments on Cultivation Analysis" (POQ 44:408-10) we offered some corrections to Michael Hughes's "The Fruits of Cultivation Analysis . . ." (POQ 44:287-302). While we were disappointed that our comments appeared some 120 pages after the Hughes article (without being noted either in the Table of Contents or at the conclusion of Hughes's piece), we were more than surprised to find the publication of tendentious and *ad hominem* remarks by Paul Hirsch immediately following (POQ 44:411-13). Hirsch's "The Limits of Advocacy Research" (a title as irrelevant to our work as his remarks are to our comment preceding it), under the guise of helping to "reassert scientific standards," is little more than presumptuous horn-tooting for a then still unpublished, and severely flawed, "reanalysis" of our research.¹

To recap briefly for latecomers, both Hughes and Hirsch re-examined some data, largely from the 1977 General Social Survey conducted by the National Opinion Research Corporation (GSS/NORC), which we reported in Violence Profile No. 9.² Both authors demonstrate that when multiple controls are applied to these items (as well as other questions not reported by us because they were either unavailable at the time of publication or unreliable), some apparent relationships between amount of television viewing and various conceptions of social reality disappear. In some cases, under simultaneous controls, the resulting patterns even become curvilinear. The evidently unassailable conclusion, therefore, is that our theory that television cultivates insecurity, mistrust, and alienation (and perhaps by extension, other conceptions of social reality derived from heavy exposure to television content) has no empirical basis.

¹ Hirsch's exposition appears in the October 1980 and January 1981 issues of *Communication Research*.

² George Gerbner, Larry Gross, Marilyn Jackson-Beeck, Suzanne Jeffries-Fox, and Nancy Signorielli, "Cultural indicators: violence profile no. 9." *Journal of Communication* (Summer 1978) 28:3, 176-207.

In our latest report,³ also summarized in our response to Hughes, we argue that the implementation of multiple controls can obscure nonspurious and theoretically meaningful relationships *within* specific subgroups. We find that many of these conditional associations can be explained by a phenomenon we call "mainstreaming," which implies that there may be a convergence of outlooks among the heavy viewers in "otherwise" disparate and heterogenous groups. Differences deriving from other factors may tend to be reduced or even eliminated among heavy viewers, and relationships may be enhanced for those groups who are least likely to share a given attitude cultivated by television. While assessing "overall effects" is important, stopping there can be inadequate and misleading.

Hirsch's analysis does stop at that point, but his rhetoric continues. He purports to provide "an indictment of the research community for permitting the widespread promotion of the cultivation hypothesis—stated as a scientific finding—to go unchallenged for so long."⁴ He contends that one of our most reprehensible failings is that our analyses of light, medium, and heavy viewers do not separate out so-called "nonviewers" and "extreme viewers" (eight hours or more per day). In fact, analyzing the responses of nonviewers and extreme viewers adds little to understanding the most pervasive consequences of television because both are tiny and bizarre groups. Extreme viewers, for example, constitute as little as 3.7 percent of the GSS/NORC data base (in 1977); and even when combined, these marginal groups always represent less than 10 percent of the samples. Furthermore, nonviewers in particular manifest a contradictory and inconsistent profile, in terms of various demographic characteristics and attitudinal measures.⁵

³ George Gerbner, Larry Gross, Michael Morgan, and Nancy Signorielli, "The 'mainstreaming' of America: violence profile no. 11." *Journal of Communication* (Summer 1980) 30:3, 10–29.

⁴ The first published cultivation analyses appeared in George Gerbner and Larry Gross, "Living with television: the violence profile," *Journal of Communication* (Spring 1976) 26:2, 172–99. At least two critiques appeared *in print within two years* of that publication. See J.M. Wober, "Televised violence and paranoid perception: the view from Great Britain," *POQ* (1978) 42:315–21; Horace Newcomb, "Assessing the violence profile of Gerbner and Gross: a humanistic critique and suggestion," *Communication Research* (1978) 5:264–82. For opposing views, see George Gerbner, Larry Gross, Michael Morgan, and Nancy Signorielli, "On Wober's 'Televised violence and paranoid perception. . .'" *POQ* (1979) 43:123–24; Thomas Neville, "More on Wober's 'Televised violence. . .'" *POQ* (1980) 44:116–17; and George Gerbner and Larry Gross, "Editorial response: a reply to Newcomb's humanistic critique," *Communication Research* (1979) 6:223–30.

⁵ See Marilyn Jackson-Beeck, "The non-viewers: who are they?" *Journal of Communication* (Summer 1977) 27:3, 65–72; and J. W. Tankard, Jr. and M. C. Harris, "A discriminant analysis of television viewers and non-viewers." *Journal of Broadcasting* (Summer 1980) 24:3, 399–409.

Thus, while “nonviewers” may indeed be *more* likely than are light viewers to give “television answers,” they cannot be used to disconfirm the cultivation process because they probably differ from viewers on relevant, uncontrolled, third variables. More important, they do not even provide compelling evidence that cultivation relationships are nonmonotonic. If Hirsch had bothered to perform *tests* for linearity, he would have found that only one item out of 22 shows a significantly nonlinear association with amount of viewing, while 77 percent show significant linearity beyond the .05 level.

“The story gets worse” indeed when one scratches beneath the surface of the rhetoric and realizes that some of Hirsch’s items are irrelevant to our cultivation analyses, because they have no discernible basis in our (or anyone else’s) TV message analysis. We are unable to see anything in known content data which would predict heavy viewers’ predilections toward suicide.

Hirsch’s final criticism, that our operational definitions of light, medium, and heavy viewers vary across different samples, is transparently simplistic. These categorizations may differ because the univariate distribution of amount of viewing may differ in samples of different ages (e.g., children vs. adults) and locations; it also depends upon question wordings and sampling techniques. That is a basic limitation of secondary analysis. In any case, we use these groupings as relative, not absolute rankings—a way to compare those who watch a lot of television with those who watch less.

Many other problems pervade Hirsch’s “reanalysis,” and these are discussed elsewhere.⁶ Our intent here is neither to reiterate the lengthy and detailed corrections his work requires, nor to rehash our disagreements with Hughes. We simply felt the need to set the record straight for POQ readers.

We are engaged in a long-range, diverse, and complex research effort on television and perceptions of social reality. The research community needs and we welcome serious scientific replication and criticism; if anything, there is too little of that. But a narrow analysis on selective grounds cannot do full justice to years of cumulative findings supported by independent investigators in the U.S. and abroad.

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⁶ George Gerbner, Larry Gross, Michael Morgan, and Nancy Signorielli, “A curious journey into the scary world of Paul Hirsch.” *Communication Research* (January 1981) 8:1 (in press). See also Robert P. Hawkins and Suzanne Pingree, “Television influence on constructions of social reality,” in National Institute of Mental Health, *Television and Behavior: Ten Years of Scientific Progress and Implications for the 80’s* (in press).