

ON THE LIMITS OF "THE LIMITS OF ADVOCACY RESEARCH": RESPONSE TO HIRSCH

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In "Some Additional Comments on Cultivation Analysis" (POQ 44:408-410) we offered some corrections to Michael Hughes' "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' piece), we were more than surprised to find the publication of tendentious and ad hominem remarks by Paul Hirsch immediately following (POQ 44:411-413).

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the guise of helping to "reassert scientific standards," is little more than presumptuous horn-tooting for a then still unpublished, and severely flawed, "reanalysis" of our research.<sup>1</sup>

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.<sup>2</sup> Both authors demonstrate that when multiple

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

<sup>2</sup> 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.

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.

In our latest report<sup>3</sup>, also summarized in our response to Hughes, we argue that the implementation of multiple controls can obscure non-spurious 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 heterogeneous 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

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<sup>3</sup> 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.

a scientific finding -- to go unchallenged for so long."<sup>4</sup> 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 "non-viewers" and "extreme viewers" (eight hours or more per day). In fact, analyzing the responses of non-viewers 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, non-viewers in particular manifest a contradictory and inconsistent profile, in terms of various demographic characteristics and attitudinal measures.<sup>5</sup>

Thus, while "non-viewers" 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 importantly, they do not even provide compelling evidence that cultivation relationships are non-monotonic. If

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<sup>4</sup> 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, 172-199. 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) 42:315-321; Horace Newcomb, "Assessing the violence profile of Gerbner and Gross: a humanistic critique and suggestion." *Communication Research* (1978) 5:264-282. For opposing views, see George Gerbner, Larry Gross, Michael Morgan, and Nancy Signorielli, "On Wober's 'Televised violence and paranoid perception....'" *POQ* (1979) 43:123-12 ; Thomas Neville, "More on Wober's 'Televised violence....'" *POQ* (1980) 44:116-117; and George Gerbner and Larry Gross, "Editorial response: a reply to Newcomb's humanistic critique." *Communication Research* (1979) 6:223-230.

<sup>5</sup> 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.

Hirsch had bothered to perform tests for linearity, he would have found that only one item out of 22 shows a significantly non-linear 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 discernable 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 towards 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.<sup>6</sup> 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

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<sup>6</sup> 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 Pin-gree, "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).

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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FINAL REPLY TO HIRSCH

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## FINAL REPLY TO HIRSCH

By George Gerbner, Larry Gross, Michael Morgan, and Nancy Signorielli

Hirsch's "Rejoinder" (in the January 1981 issue of this journal) -- as well as his interpretation of our previous "comment" (in the same issue) -- contains an impressive number of inaccuracies, errors, and convolutions of our statements and findings. His illusions about our "concessions" are particularly puzzling. He raises very few new points and seems oblivious to numerous issues we presented, choosing instead to rehash his "findings" and "conclusions" ignoring contrary explanations and evidence.

If we do not respond here to some of his trivial points, it is not because, as he presumes, we "concede" them; rather, it is in order to prevent this discussion from bogging down in irrelevant minutiae. We will focus on the few new concerns he does present and areas which represent a markedly stepped-up attack.

### On Our "Rejection" of the General Social Surveys

One of the most troubling aspects of Hirsch's "rejoinder" is his imputation that we have "dismissed" the NORC General Social Surveys. According to Hirsch, we have suggested that the GSS "is itself unreliable" and "is less worthy than the non-public and unreanalyzed surveys [we] claim show 'stronger' results."<sup>1</sup> Our so-called "attack" on the GSS has entailed "a remarkable number of charges," which imply that we believe "this entire dataset should be rejected." After Hirsch reported his "reanalysis," we "quickly announced that these data are flawed." Finally, Hirsch says that our "understandable effort to maintain credibility under duress" is "insulting to the national research community of social scientists."

Hirsch has concocted a scenario without foundation in fact. In case we have inadvertently led anyone besides Hirsch to this interpretation, let us make it clear that we acknowledge and appreciate the value and quality of the General Social Surveys. While they have not, as Hirsch claims, been the backbone of our research, we have used and are continuing to use many items from this data set. Invoking such accolades as "nationally acclaimed" and "prestigious," Hirsch charges us with blasphemy.

The basis for his allegations is that we expressed reservations about the reliability and/or validity of two items; one deals with "fear," and the other with "approval of violence," and they will be discussed below. For now, let us repeat categorically that our caution about two questions should not be construed as evidence that we believe the entire data set is flawed in some fundamental way. Only by the wildest stretch of the imagination, or by deliberate distortion, can it be said that we have "rejected" the GSS/NORC data. The fact that these controversies sparked our reassessment of the quality of these particular measures provides no grounds for complaining that we had not expressed these doubts sooner. Despite Hirsch's puzzling proclamation that greater sample size necessitates greater reliability, we do not believe that the reliability and validity of each and every item can be taken for granted. This hardly stands as a total "rejection" of GSS/NORC.

#### On Our "Cover-Up" of Damaging Results

The two items ("fear" and "approval of violence") about which we voiced reservations loom large in Hirsch's "reanalysis," notwithstanding the disclaimer that "neither is at all central to the conclusions reached in my article." We had reported (Gerbner, et al., 1978) that when two samples of

junior high school students were asked, "How often is it all right to hit someone if you are mad at them for a good reason?"<sup>2</sup>, a significantly higher proportion of heavy than of light viewers answered, "almost always." Both adolescent and adult heavy viewers also were more likely to report being afraid to walk alone at night.<sup>3</sup>

Yet, Hirsch claimed to find no evidence to support our conclusions when he analyzed GSS/NORC data for adults. The GSS/NORC question asked adults, "Are there any any situations that you can imagine in which you would approve of a man punching an adult male stranger?" Hirsch's results regarding this "approval of violence" item -- which show adult heavy viewers less likely to condone the use of violence -- are said to be "especially damaging to the cultivation hypothesis." In "one of the very few relationships whose statistical significance remains after the imposition of multiple controls," the sign of the association "runs directly counter" to what he claims cultivation theory would predict. Hirsch also charges us with selective reporting of this item, and with presenting only those data which are consistent with our viewpoint.

These charges, and the data regarding the relationship between television viewing and "approval of violence," deserve detailed discussion; in the process, we will address many of his other points. Strictly speaking, there is nothing in our message system analysis which implies anything about whether or not violence is "approved of" in the television world. We asked adolescents, "How often is it all right to hit someone if you are mad at them for a good reason?" ("almost always" or "almost never"), largely because of the commonly-voiced fear that television desensitizes young people to violence, making them more willing to use it. As we have noted, there is evidence that, for adolescents, heavy viewing tends to go with a greater likelihood of saying "it is almost always all right."

There are two critical questions here. First, why are the findings different for adults and adolescents? Second, why did we not report the results for adults from GSS/NORC?

We believe that the apparent discrepancy between the results for adolescents and those for adults do not cast doubt on our theory for two major reasons:

- (1) There is substantial evidence to indicate that the GSS/NORC question asked of adults is unreliable;
- (2) Regardless of whether or not the GSS/NORC question is indeed reliable, it is simply not comparable to the question we asked of adolescents.

As we noted in our previous comment, the series of questions measuring approval of violence (we have no idea why Hirsch analyzed only the lead-in question) produces a less-than-marginal estimate of reliability. Hirsch argues that an "item's reliability in scaling has nothing to do with its value as a single item." While that may be generally true, it hardly seems likely in this case. Reliability (in the sense of internal homogeneity), "accuracy" of individual responses, and random or systematic procedural biases (coding or punching errors, etc.) are all quite different things, and Hirsch has muddled them. We feel there is reasonable doubt about the item's value as a measure of a clear and unambiguous dimension. Again, this is decidedly not a suggestion that we believe that lack of quality control, "cheating" or similar errors mar the General Social Surveys.

In any case, the argument that the GSS/NORC question is problematic becomes even more compelling when the lead-in question is examined in light of the follow-ups (not mentioned by Hirsch), which provide five specific situations in which the respondent might or might not "approve of a man punching an adult male stranger." We find it notable that that vast majority of all adult respondents who said "no" to the lead-in (i.e., they were unable to imagine a situation) answered "yes" to at least one of the follow-ups:

for 1978 and 1980 combined, 985 people said "no" to the lead-in; 842, or 85.5% of the 985, subsequently cited at least one situation in which they would approve of a man punching an adult male stranger.<sup>4</sup> Interestingly, heavy viewers are more likely to show this discrepancy, by a difference of ten percentage points ( $\gamma = .13$ ,  $p < .0001$ ). Thus, being unable to imagine a situation spontaneously is quite different from approving the use of violence in specific situations -- especially for adult heavy viewers.

Furthermore, it seems quite likely to us that the question asked of adults taps a rather different dimension than the one we asked of young people.<sup>5</sup> The adolescent question asks how often is it all right (a value judgement about frequency of approval, not a yes/no dichotomy) to hit someone if you are mad at them for a good reason (providing a justification for the act, dealing with the respondents' own projected behavior, establishing some level of interpersonal familiarity between assailant and victim). The GSS/NORC questions asks are there any situations that you can imagine (more abstract and partially dependent upon the respondent's ability to imagine) in which you would approve of a man punching an adult male stranger (the assailant is someone besides the respondent, and the victim is unknown to the victimizer).

We also noted in our previous comment that the GSS/NORC item shows counter-intuitive relationships with background variables, with higher SES respondents more likely to be able to "imagine a situation." Hirsch rejects this reasoning, simply attempting to dismiss our contention by relegating "counter-intuitive" to quotation marks, and claiming that the unexpected relationship represents "reason to report, rather than suppress the finding." This might be true if our central research interest were to determine what factors have an impact on respondents' ability to "imagine a situation." But since we are concerned with understanding the contributions of television to theoretically meaningful dimensions of attitudes and behaviors, such a finding is irrelevant, at best.

Finally, our adolescent item and various indices of "approval of violence" used by Dominick and Greenberg (1972), McLeod, et al. (1972), and McIntyre and Teevan (1972) all show negative associations with SES, while the GSS/NORC item, again, is positively associated with social class variables. Even if there were no doubts as to the GSS/NORC items' reliability (and there are many), this validation discrepancy suggests that different dimensions are being tapped by the respective items.

If one were either to defend the GSS/NORC item's reliability, or to argue that the adolescent and adult questions are conceptually comparable, there remain several other possible explanations for why adolescents and adults show different results. These include: (1) the question we asked of adolescents is bad; (2) our adolescent samples are bad; (3) what's true for adolescents may not be true for adults; and (4) cultivation theory is wrong. As we shall see, none of these seems to be a strong possibility.

(1) Problems with the Adolescent Question. This seems quite unlikely, particularly since our findings were similar to those of a number of earlier studies. Dominick and Greenberg (1972) found consistent relationships between exposure to television violence and willingness to use violence, perceived effectiveness of violence, and (for girls) suggesting violence as a solution to conflict. For boys, they also found that exposure interacts with social class and family attitudes in terms of approval of aggression. While they used more questions, our results parallel theirs.

In addition, McLeod, et al. (1972) found modest positive correlations between approval of aggression and both overall viewing time and violent viewing. Particularly strong associations were found between approval of aggression and self-reported amount of violence viewed three or four years earlier. They note that "aggressive attitudes are rather closely related to

both self-reported and peer-reported aggressive behavior, and they are associated with various viewing and family variables in a manner similar to the measures of aggression" (p. 265).

Finally, McIntyre and Teevan (1972) even found that adolescents whose favorite programs (both the single favorite and particularly the average of the four most favorite) were more violent were more likely to approve the use of violence. This pattern was not altered by controls for perceived realism of television violence, social class, age, and "insulating factors" which represent strength of ties to the social structure (e.g., aspirations, school grades, integration into school activities, and relationships with parents and peers). While none of these studies used precisely the same question wording as we did, they tend to be much closer to ours than to the GSS/NORC question (except for McIntyre and Teevan's question about adult violence, discussed below).

(2) Problems with the Adolescent Sample. Hirsch chooses to discount any findings derived from so-called "convenience" samples. We feel this is unwarranted, if only for the paucity of national probability samples of children and adolescents in social science research. Our panel of New Jersey adolescents is probably one of the best of its kind, combining six questionnaires over three years, in-depth personal interviews, and parents' questionnaires. Hirsch ignores our longitudinal findings completely, presumably because they are not drawn from a national sample. In that case, he might as well reject much research in social or experimental psychology, and even sociology. To say that we "equate" GSS/NORC with a "proprietary" sample of 116 New Yorkers is silly, and a facetious distortion of our statement that our conclusions are based on consistent patterns observed across a range of samples, while his come from one.<sup>6</sup> That is not to attribute "equal weight" to each, but to call for appropriate caution and restraint when a

large number of other samples (national probability, quota, and school children) show contrary results.

Finally, in this case, a number of different studies, which used samples of children and adolescents of different sizes and from different geographic areas, produced essentially similar results. This clearly provides added support for our samples and findings.<sup>7</sup>

It is also worth noting that Dominick and Greenberg (1972) found numerous interactions between violence exposure and social class in terms of willingness to use violence, approval of aggression, perceived effectiveness of violence, etc. This takes on added import in light of the negative relationship between SES and the dependent measures: exposure to television violence is a much more predictive measure of attitudes among middle class children than it is among lower class children. There is less relationship with viewing for lower class children -- who have higher approval levels, regardless of viewing. This certainly fits our "mainstreaming" perspective -- the association is enhanced for those otherwise less likely to share what is arguably the television perspective.

(3) Adults and Children are Different. It is possible that the divergence in results stems neither from problems with the adolescent sample (as Hirsch argues) nor from problems with the GSS/NORC question (which we believe we have convincingly demonstrated), but that both relationships are valid. One could argue that children learn that violence is an appropriate or (at least) common means of resolving conflicts or achieving goals, while adults learn that "crime does not pay," and transgressions are punished. Certainly, social class has differential implications for these attitudes for different age groups, so television could as well. Yet, given the manifest differences in demographic correlates of response patterns between the

two, the likelihood of reliability problems with the GSS/NORC question, and the lack of conceptual comparability, this proposition cannot be tested.<sup>8</sup> It could well be that what holds for children need not hold for adults, but we do not think that this is the primary explanation for the differences.

(4) Cultivation Theory is Wrong. It might seem, a priori, that there is a simple reason why the opposite, "damaging" association holds in the GSS/NORC data: that the theory, which predicts that heavy viewers should be more likely to condone the use of violence, is faulty. Clearly, that is Hirsch's conclusion. He goes even further and suggests that this "damaging" negative association with television is one of the very few in GSS/NORC which persists under multiple controls.

Yet, our own reanalysis of this item over three years of the GSS (1975, 1978, and 1980; the only years where it co-occurs with the television viewing question) provides no evidence whatsoever to support his argument that heavy viewers are less likely to "approve of violence" under multiple controls. Controlling for sex, age, education, race, and income, the fourth-order partial correlations for each year are .02, .04, and .02, respectively (all obviously n.s.).

If, as Loftin and Lizotte (1974) suggest, "privileged groups" were more likely to respond affirmatively to the "imagine a situation" GSS/NORC question in 1973, it is not surprising that it looks as if heavy viewers (who tend to be of lower SES) do not. Thus, it is also not surprising that this relationship turns out to be spurious, contrary to Hirsch's superficial analysis.<sup>9</sup>

Thus, quite apart from the questionable characteristics of the GSS/NORC question, and the results of Dominick and Greenberg (1972) and McIntyre and Teevan (1972) (not to mention the number of other items, across many data sets, including other questions from GSS/NORC, which support cultivation), we reject his conclusion.

All this having been said about why the GSS/NORC results differ from those obtained from other samples, the question remains why we never reported these "damaging" results. The "offending" publication is Violence Profile No. 9 (Gerbner, et al., 1978). While Hirsch dismisses our claims of unreliability (and turns them into a wholesale rejection of the General Social Survey) and rejects its "counter-intuitive" association with background variables, he disregards the fact that (as stated in our previous comment) the 1978 GSS/NORC data were not yet available when we released Violence Profile No. 9. In all frankness, internal disagreement about the validity and meaning of the item led us not to report the data for 1975. The so-called "suppression" derived from conflicting viewpoints over what the item actually measured, given its clear conceptual divergence from the question asked of adolescents.

In sum, the GSS/NORC item about "approval of violence" is by no means "especially damaging" to our position. We have discussed this item extensively because it demonstrates that many of Hirsch's charges -- his accusation that we "reject" the GSS, his condemnation of our other samples, our "suppression" of the results, etc. -- are tendentious and misleading and that his "re-analysis" is severely flawed.

#### Relationship Between Message and Cultivation Analysis

Another area of confusion throughout Hirsch's discussions is the relationship between our message system analysis and cultivation analysis. Specifically, Hirsch has charged that cultivation analysis has no manifest connection with message analysis.

A basic premise for our research is that cultivation analysis begins with the patterns found in the "world" of television drama -- a world that

presents coherent images of life and society. The basic question we are concerned with is how these images are reflected in viewers' assumptions and values.

We do not expect (nor have ever expected) heavy viewers to exhibit a one to one correspondence between what they see on television and what they believe or do. The television world is a fictional world in which details are selected with care and for a purpose. Its people do not live or die but are created or destroyed to tell a story. Television drama presents stories about how things work, how people behave, what it means to be a man, a woman, a child, an older person, a cop, "bad," "good," and so on. Television also conveys information about risks and power as well as information about the range of opportunities and activities that are available and acceptable for its characters. Most of what television tells us about life and people cannot be translated into discrete facts but can be construed to present potential lessons about life and society. Thus, we need not expect that viewers will specifically identify with characters who are "most like themselves" demographically.

The design is further complicated when we conduct secondary analysis of existing survey data, using questions that were specially designed to answer some other primary research objective. These analyses include all of our work with the GSS/NORC, the CPS election survey, and the NCOA "Myth and Reality of Aging" survey. In these cases we had no control over question design and often stretched existing response questions and response categories to meet our framework. As we noted above, the problems with the GSS/NORC questions asking respondents to "imagine a situation in which you would approve..." are a good example of the difficulties that one must expect in secondary analysis. Thus, a considerable portion of our research has used less than optimal questions (and response categories) for testing our ideas

and/or expectations. In some cases we probably have been overly cautious in not using certain questions, while in some other cases, we probably have not been cautious enough.

Over the years we have had many graduate students and collaborators working on this project. We have tried to accommodate their interests while, at the same time, maintaining a common thread throughout the research. It is out of this variety of interests that the inclusion of the "isolationism" and "expectations for a world war" questions emerged. These items were originally included because they seemed, to some of our collaborators, to add another dimension to understanding alienation and because we saw some correspondence in message system findings (specifically, the extreme U.S. nationalism of the television world).

#### Mainstreaming and Resonance

Apparently, we have not convinced Hirsch that "mainstreaming" and "resonance" are valid theoretical formulations, supported by empirical evidence and conceptual justification. Yet he does acknowledge (and in doing so contradicts himself), in Part II, that his "examination of the NORC data suggests that much of the 'effects' found at both ends of the viewing spectrum" hold up for "high-status, high-income, and high-education individuals who view television heavily." This sounds like "mainstreaming" to us.

We have been engaged in a long-term, ongoing, and flexible effort to develop a coherent, data-based theory of television's impact on society. There has been no similar sustained and broad-based effort. Our project has evolved and our theories developed over time. Hirsch's greatest objection seems to be that we do not define some specific problem, formulate rigid hypotheses, collect the appropriate data, determine whether they support or disconfirm the hypothesis, all in a one-shot effort, and then move on to something else. That, however, is the major shortcoming of much social research.

As Sherlock Holmes put it, "It is a capital mistake to theorize before one has data. Insensibly one begins to twist facts to suit theories, rather than twist theories to suit facts." While that sentiment rubs Hirsch the wrong way, it almost certainly captures the actual manner in which most science is conducted. Research is a continual process of interaction between theory and data, expectations and results, predictions and findings.

Hirsch makes much of what he construes as inconsistencies and contradictions in our theoretical refinements over the years. In fact, the refinements developed a steady and consistent line of theory-building, and will continue to do so. Contrary to Hirsch's implications, "mainstreaming" and "resonance" were not "dreamed up" for our response to his criticism. Passages from our earlier publications, cited in our previous response to Hirsch, suggesting that "a more refined analysis" of "differential cultivation patterns is a task of our continuing research" (Gerbner, et al., 1978, pp. 205-206) were deleted (without our knowledge or approval) from the published version of our response. Hirsch persists in confusing his reformulation (that cultivation will be most pronounced among the real-world counterparts of television's most victimized groups) with ours, claiming cultivation theory is disconfirmed by the failure of his (insufficient and superficial) test.

After examining the conceptual and empirical underpinnings of "mainstreaming" and "resonance," Hirsch rejects them as "speculative, nonpredictive, unspecified, post hoc, and irrefutable" (p. 79). Let us examine these charges.

"Speculative." By "speculative," we assume that he does not mean "involving, based on, or constituting intellectual speculation" or "marked by questioning curiosity," because these hardly seem objectionable. "Mainstreaming" and "resonance" were indeed developed in response to "questioning curiosity," in that they reflect a paradigm designed to explain the intriguing systematic regularities we observed. Even most methodology textbooks frame theory-building as "invention, not discovery," and stress the roles of curiosity and

casual observation in the development of theory.

Because these usages provide no conceivable basis for rejecting our refinements, he might mean "speculative" as in "theoretical rather than demonstrable." If so, this represents a puzzling inverse tautology, where "mainstreaming" and "resonance" are not good theory because they are theoretical. As to their not being "demonstrable," we can only point to the consistency with which they appear in the data, for a wide variety of cultivation topics (see, e.g., Gerbner, et al., 1980b; forthcoming; Gerbner, 1980; Signorielli, 1979; Morgan and Gross, 1980; and also Hirsch's statement that "the more interesting findings" in GSS/NORC appear primarily for high-income, high-education, high-status respondents). Strictly speaking, even his absurd interpretation of the effects of controlling for astrological sign show that they can be "demonstrated"; unlike the specifications we have presented, however, there is no theoretically intelligible reason for the resulting patterns.

Moreover, the findings that originally led us to these observations have already been supported by independent investigators.<sup>10</sup> In addition to the work of Dominick and Greenberg (1972), Lull, et al. (in press) found a significant interaction with education in the relationship between amount of viewing and feminism. In a sample of 523 Santa Barbara adults, among the highly educated, greater viewing goes with less feminism; better educated heavy viewers are more "sexist." But among those with less educated (who are "otherwise" less likely to endorse feminist beliefs), the high feminists are the heavy viewers. As in our analysis (Signorielli, 1979), greater viewing means less sexism for the most sexist, and more sexism for the less sexist.

Impressive independent confirmation of our findings about school achievement (Morgan and Gross, 1980) comes from the California State Department of Education's (1980) massive statewide assessment program of over 510,000 sixth

and tenth graders. In this study, negative relationships between amount of television viewing and scores on achievement tests of reading, written expression, and mathematics are most pronounced for high SES students, while the lowest SES students (who generally score lower) show some positive associations. Even more striking evidence for "mainstreaming" emerged under controls for English fluency. Light viewers with limited English skills score quite low, but the more television the low achievers watch, the higher their reading scores. Parental education, time spent on homework, and amount of extra-scholastic reading all produced the same results, particularly for sixth graders. Among light viewers, more parental education, more time spent doing homework, and more outside reading all go with better reading skills; among heavy viewers, these factors make almost no difference in scores.

Finally, Werner (in progress) reports that Norwegian adolescents' attitudes towards the United States provide evidence of "mainstreaming," in that stronger positive relationships with viewing were found in outlying areas; in central areas, attitudes were more positive regardless of viewing levels.<sup>11</sup>

"Nonpredictive." Whether "mainstreaming" and "resonance" are "non-predictive" is yet to be seen. When the concepts were introduced, we stressed that they

are still being developed and investigated. Although the number of empirical instances of each is rapidly growing, too few have been accumulated to allow for prediction of when one or the other -- or neither -- will occur. Nonetheless, we believe that the results we will report here suggest that these concepts merit serious consideration (Gerbner, et al., 1980a:16).

The fact that such instances did occur at all, always with highly plausible implications for interpretation (and particularly in data that Hirsch and others claimed show "no relationships"), led us to offer these concepts as new ideas worth pursuing.

Hirsch's confusions about what "mainstreaming" and "resonance" might

predict in the first place underlie this issue. Not only does he persist in presenting his reformulation (again, that cultivation will be strongest in groups whose television counterparts are most likely to be victimized) as: (a) our expectation and (b) contradictory to "mainstreaming" and "resonance," but he also asserts that the lack of support for this proposition (again, based on an inappropriate test) disconfirms "mainstreaming" and "resonance" along with the entire theory of cultivation. As with the previous charge, his argument relies on a specious inverse tautology: proposition X (which contradicts propositions Y and Z) is not borne out by the data, so propositions Y and Z should also be rejected. Specifically, he claims that his test shows that "there simply are not consistent patterns in any direction among the critical subgroups to which 'mainstreaming' and 'resonance' are presented as most applicable" (p. 83; italics in original) -- in terms of a formulation which allegedly contradicts our refinements. And yet he is surprised that we "assert that the failure (sic) of the audience data to support this clearly formulated hypothesis (sic) casts no doubt on the logic of cultivation theory."

Thus, according to Hirsch, "mainstreaming" and "resonance" are "non-predictive" because the results of his faulty test of his quite different hypothesis say so. Meanwhile, back at our ranch, the elaboration of our refinements into a predictive framework has been progressing in a variety of areas, and the shape they are taking is quite consistent with our early expectations. In Violence Profile No. 11, we suggested that "mainstreaming" is a more general process and "resonance" deals with special salience of specific issues to specific groups at certain times.

In other words, the ability of television viewing to over-ride or reduce the influence of demographic factors, with heavy viewers of "otherwise" divergent perspectives sharing a "television" view, reflects "mainstreaming," or the cultivation of common conceptions of social reality. When real-life experience or other dispositions increase the congruence between environmental and television messages, we should get "resonance."<sup>12</sup>

"Unspecified." In some ways, this charge resembles the previous one ("nonpredictive"), in that it relates to our alleged failure to "specify" which groups will show evidence of "mainstreaming" or "resonance." His criticisms which elaborate that point continue to demonstrate and affirm what we said in our previous comment: that Hirsch makes a consistent effort to oversimplify our theory into a mechanical concept. This is seen here in two ways: first, he would like to see explicit statements about which specific groups will show these conditional patterns; second, he believes that the "mainstream" is some specific, fixed point.

As to the first, we contend that the conditional influence of other factors is not invariant across all subject areas in which we examine television's contribution to conceptions of social reality. While demographics (and particularly education) generally illuminate "mainstreaming," this need not always be the case. But the dispositions and experiences which should generate "resonance" are even more likely to vary across cultivation topics. The differential appropriateness of different controls for different analyses makes it unfeasible -- even logically impossible -- to set forth rules about which specific groups should "resonate." As Hornik, et al. (1980) note in connection with their "distance" theory of susceptibility to media effects<sup>13</sup>,

the "myriad of specific instances with quite varied characteristics" makes the specification of regularly vulnerable groups difficult. Our reluctance -- even, refusal -- to propose groups which will always show "mainstreaming" and "resonance" in all issues stems from our sensitivity to the exigencies of each analysis. What remains to be developed is a more comprehensive system of discovered specifications that may, in the future, suggest some dynamic process of specifications.

The second, and related, way in which our refinements are "unspecified" stems from the "absence of operational procedures...for determining the point where the 'mainstream' resides for the population and for the comparison of subgroups." In short, the "mainstream" is not a point. "Mainstreaming" is a process of convergence, homogenization, and standardization, in the direction of television's version of social reality. It is this criterion which Hirsch ignores in his charge that our theory is confirmed by "whatever surprises or findings emerge from the data" and that cultivation explains "any percentage movements in any direction." The "mainstream" can only be identified in terms of baseline comparisons of counterpart subgroups. The "point" at which one set of (appropriately matched) counterpart subgroups may converge need not be the same "point" where another set of counterparts converge. The "operational procedure" is simply the empirical identification of those groups who tend to deviate from the "mainstream" view for a particular issue or topic.

"Post Hoc." This is not too different from the preceding "grounds for rejection" of our refinements. Again, we explicitly introduced these

constructs as having derived from observing consistent, meaningful, conditional associations across numerous data bases and areas of analysis. Certainly, we are aware of the potential dangers of post hoc analysis. We can only insist that all theoretical developments are "post hoc," in the sense that they develop from the perceived structure of regularities in phenomena. We believe this represents reason for continued elaboration, testing, and refinement -- not rejection.

"Irrefutable" (i.e., non-refutable). To a purist, the critical determination of whether our refinements are "scientific" lies in their ability to be falsified. We accept the need to specify evidence that would constitute falsification. Furthermore, we laid out such a model in our previous comment. Yet, Hirsch "refutes" our demonstration of conditions for falsifiability.

The only way he can dismiss our explanation of the criteria by which these concepts could be falsified, however, is by twisting what we actually said. Of the six possible subgroup patterns which we claimed were inconsistent with "mainstreaming" or "resonance," Hirsch asserts that three would be interpreted as evidence supporting them.<sup>14</sup> It is bizarre to take examples presented by us as explicit instances of contrary findings, and claim that we would interpret them in the opposite manner.

This means that Hirsch's argument (that cultivation theory has an unusually high probability of being confirmed by chance) is specious, because his conclusion that 73% of the possible patterns provide support for the theory is based on blatant distortion of what we actually said. In any case, we doubt his assumption that all of these patterns are equally likely outcomes (since the chance number of significant specifications would be partly determined by the number of subgroups examined), which further confuses

his conclusion. And to make an additional restriction on the acceptability of supporting evidence (thus increasing the chances of falsification), we repeat that the observed specifications must reflect a theoretically meaningful aspect of televised social reality.

Finally, we must reiterate that Hirsch is clearly wrong, and his mistake stems from his basic misconstruction of our work, when he claims that we would interpret "no relationship" as evidence that "mainstreaming" and "resonance" cancelled each other out. Once again, his rejoinder ignores contrary explanations and evidence presented in our previous comment.

The entire protracted process of our "debate" with Hirsch in this journal has been an extraordinarily and unfortunately distasteful experience. Whatever scientific value it might have had was compromised by its secretive beginnings, uncollegial tone, editorial prejudice (including the deletion of strategic parts of our comments without our knowledge and approval), and we have had a sense of trying to disperse a smokescreen rather than of advancing a scientific argument. Whatever the original impetus might have been, we can only conclude that Hirsch's attack on cultivation theory and each of his reasons for rejecting the concepts of "mainstreaming" and "resonance" are unwarranted and incorrect.

- 1 We are bemused that Hirsch faults us for not making these other datasets available, when during the year in which he conducted his "reanalysis," he never asked us for any information regarding our ongoing work, much less for access to other data bases.
- 2 We admit to a clerical error in our presentation, in Violence Profile No. 9 (Gerbner, et al., 1978), of the "approval of violence" question asked of adolescents. The article erroneously states that the phrase, "for a good reason," was included in one sample (New Jersey) but not in the other (New York). A check on our original questionnaires, however, reveals that "for a good reason" was included in the questions for both samples. The only difference is that the New Jersey version reads "if you are mad at the person for a good reason" and the New York version reads "if you are mad at them for a good reason."
- 3 The GSS/NORC item about fear of walking alone at night has numerous problems that we detailed in our previous comment (see especially footnote 8). Since he does not address these, we need not repeat them here.
- 4 Two-thirds (66.6%) of those who could not "imagine a situation" said "yes" to at least two follow-ups.
- 5 This is also suggested by Hawkins and Pingree (forthcoming).
- 6 Despite the disappearance of our annual Technical Reports from Hirsch's library in footnote 2 and their sudden reappearance in footnote 13, these reports always contain complete sample documentation. Relatedly, we cannot fathom his allegation that we "currently" dichotomize television viewing. Except for the small samples reported in Violence Profile No. 10, we do not.
- 7 We surveyed 649 sixth-ninth graders in New Jersey and 113 ten-to-thirteen year-olds in New York. Dominick and Greenberg (1972) surveyed 838 fourth-sixth graders in Michigan; McLeod, et al. (1972) surveyed 229 seventh graders and 244 tenth graders in Maryland, and 225 junior and senior high school students in Wisconsin; and McIntyre and Teevan (1972) surveyed 1242 senior and 1057 junior high students in Maryland.
- 8 McIntyre and Teevan (1972) used separate scales measuring approval of violence inflicted by adults, teenagers, and the police. The adult measure was the same as the GSS/NORC item's follow-ups; i.e., they only asked about approval in the specific situations, and did not include the problematic "any situations in which you can imagine" general version. Thus, from the data they report (again, which suggest a positive association between television and approval), we cannot unambiguously determine whether adults and children indeed show different associations with the same measure.
- 9 In our previous comment, we noted a significant specification in this relationship in 1978 among college-educated respondents. As it turns out, this is not replicated in the 1975 or 1980 data, supporting our conclusion that this association is essentially spurious. Examining within-group partial correlations reveals that exactly four out of 36 are significant. (The 36 correlations are obtained by multiplying the years of the GSS which include this item (three) by 12 groups, defined by sex (male/female), race (white/non-white), education (no college/some college), age (under 30/30-54/55 and up), and income (low/medium/high). The partials include all other demographic controls, including residual variation in the control group

variable, where it is continuous.) Moreover, three out of the four significant within-group partials are in the 1978 data, accounting perhaps for his observation that this relationship holds up under multiple controls. The three that are significant (in 1978) are for males, high income respondents, and college-educated respondents -- in all cases, groups who are more likely to be able to "imagine a situation." Thus, although this pattern does not appear in either the 1975 or 1980 data, one could argue that when heavier viewing does go with less "approval of violence," it is only within "privileged groups."

10 We had discussed these replications in our original comment. The discussion, however, was cut from our response without our knowledge or consent.

11 Note that these recent studies reflect independent confirmation only of our theoretical refinements. Our general findings about cultivation have been replicated by many other investigators; see Beuf (1974), Bryant, et al. (1981), Elliot and Slater (1980), Freuh and McGhee (1975), Haney and Manzolati (in press), Neville (1980), Tan (1979), Volgy and Schwartz (1980), Zill and Peterson (1980), and particularly the comprehensive review of this research by Hawkins and Pingree (forthcoming).

12 Analyses are in progress in two cultivation areas (conceptions of marriage and attitudes towards blacks) in which these assumptions are being implemented in an advance prediction framework. Preliminary results strongly support the validity of this framework.

13 Briefly, Hornik, et al.'s (1980) concept of "distance" implies that television will be most influential when the environment is supportive of its messages, or when immediate information is low, and depending upon the "need to act" upon the issue. In some ways, this is not unlike "resonance."

14 We would agree with Hirsch that graph f illustrates an across-the-board cultivation effect; it does not, however, illustrate either "mainstreaming" or "resonance." Graph i, which was intended to be a perfect "X", does not illustrate "mainstreaming" because it does not represent the convergence of outlooks that marks the presence of "mainstreaming." Finally, graph j cannot be interpreted as an illustration of "resonance" because there are two very different effects of viewing. We would postulate the occurrence of "resonance" when one of the subgroups is very positively affected by viewing and the other remains static, or when one group is much more likely and the other group a little more likely to give the "television answer" (see graphs d and e). Clearly, graph j illustrates some television effect; however, since one group is much more inclined and the other group much less inclined to give the television answer, we would not explain the results as "resonance." In brief, both "resonance" and "mainstreaming" reflect the overall patterns exhibited by both (not just one) subgroups.

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