A CURIOUS JOURNEY INTO THE SCARY WORLD OF PAUL HIRSCH

GEORGE GERBNER
LARRY GROSS
MICHAEL MORGAN
NANCY SIGNORIELLI
The Annenberg School of Communications
University of Pennsylvania

Paul Hirsch's two-part exposition (the first of which appeared in the October 1980 issue of this journal) confronts us, and the reader, with an improbable scenario. In order to take it seriously, which we intend to do, one must entertain the likelihood of a brilliant scholarly surprise attack making mincemeat out of a plodding band of academic poachers. The masterful "reanalysis" of selected data not only demolishes cumulative results of a decade of fairly massive cooperative research and theory building, along with substantial independent confirmation; it also demonstrates that the research is both worthless and stubbornly wrong-headed.

Unlikely as that dramatic coup for pure science might be we intend to demonstrate that Hirsch's analysis is flawed, incomplete, and tendentious. We believe that the data, looked at cumulatively over numerous samples (including

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EDITOR'S NOTE: *This comment and the accompanying rejoinder refer to Part I, "The 'Scary World' of the Nonviewer and Other Anomalies," by Paul M. Hirsch, Communication Research, Volume 7, Number 4, October 1980, and Part II, "On Not Learning from One's Own Mistakes," immediately preceding in this issue.*

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the National Opinion Research Center's General Social Survey), provide considerable evidence that television makes a consistent independent contribution to viewers' assumptions, outlooks, and beliefs about social reality. Furthermore, we shall show that our two recent refinements reflect advances which were implicity in virtually all of our theoretical writing, rather than radical "reformulations" of our theory.

Certain problems pervade both pieces of his "critique," such as overstatement, exaggeration, and inaccuracy, but each contains its own primary flaws. The outstanding gaps in Part I are the neglect of subgroup specifications and the unsubstantiated claim that the overall associations are "nonlinear." In Part II he misrepresents our recent refinements and makes the claim that they are logically contradictory, ambiguous, untestable, and thereby incapable of being disproved. We shall divide this companion piece into two sections, each focusing generally (but not exclusively) on Hirsch's respective installments. We believe Hirsch's work should be viewed as two independent pieces, and we offer our two sections with this in mind.

PART I

Science is little more than a way of studying the world that allows others to retrace your steps. Data never speak for themselves; it is up to the consumer of research to determine whether they support an investigator's claims. Hirsch purports to have done just that and finds our conclusions unjustified.

But Hirsch has come to his conclusions based upon analysis of one dataset, the General Social Survey conducted by the National Opinion Research Center (GGS/NORC), incorporating some questions (e.g., the series on suicide) which have no connection with known data about the television world, and others which exhibit some of the weakest associations we have ever found. Furthermore, he greatly inflates the extent to which we have relied upon GSS/NORC data, claiming that it is a "major and critical source" of "much of the empirical support" for the cultivation hypothesis, and that GSS/NORC, and the 1976 election survey from the University of Michigan (Survey Research Center) Center for Political Studies, represent the only national adult samples we have analyzed. (On the contrary, our published reports have used four others: two from Opinion Research Corporation, one from Starch, and one from Harris.) We have reported a great deal of data, including analyses which provided only peripheral or even tenuous support for our theses; but we have used many questions and many samples because all data are flawed in some way, and knowledge accumulates gradually, if not linearly, over many studies. In fact, it is the cumulative consistency of our findings that makes them most compelling.

A minor but revealing confusion begins with Hirsch's early assertion (p. 407): "Conceptually, this article begins at that point where Gerbner et al. seek to impose their categories for purposes of content analysis onto the interpretive mind of the viewer." In a footnote (p. 451), Hirsch even implies that we claim that viewers are aware of the impact of specific messages upon them. Of course, we impose neither categories nor awareness upon the "interpretive mind of the viewer," whatever that might be. We simply identify clear-cut and pervasive patterns in the world of television, such as age and sex roles, occupations, certain types of prevalent actions, and the like, and ask viewers questions that can reveal what they assume to be the facts of the real world with regard to these patterns. The questions do not mention television, and the respondents' awareness of the source of their information is irrelevant for our purposes. The relationship between amount of viewing and the tendency to respond to these questions according to the facts presented in the world of television, with other factors held constant, is what reveals television's cultivation of viewer conceptions of reality.

Amid a barrage of other accusations, Hirsch levels four primary charges against us in Part I:

(1) that our definition of "light," "medium," and "heavy" viewers varies across different samples of respondents;
(2) that when "nonviewers" and "extreme viewers" (over eight hours a day) are separated from the light and heavy viewing categories, the resulting relationships between amount of viewing and attitudes are nonlinear, with nonviewers more imbued with the "TV perspective" than light viewers, and extreme viewers less afraid/anomic than heavy viewers;
(3) that we have selectively reported findings to support our theory, overlooking other "relevant" items;
(4) that the application of multiple controls eliminates the evidence for any overall, independent contribution of television viewing to conceptions of social reality.

Although we will deal with the first three of these below, the fourth point is the most critical, and essentially identical to Hughes's (1980) reanalysis. Basically, both authors reexamined some GSS/NORC data we presented in Violence Profile No. 9 (1978) and concluded that, at least in these data, simple relationships between amount of viewing and some attitudes are wiped out when a number of control variables are held constant simultaneously. We also observed this—and more (see Gerbner et al., 1980a, 1980b).

A conclusion of "no overall relationship" is of limited value because there may be (and often are) significant, meaningful, and nonspurious associations within specific subgroups. We believe that these variations in susceptibility are critical to understanding television and are neither random nor uninterpretable. They are systematic phenomena which can usually be explained by one of two processes we call "mainstreaming" and "resonance."

"Mainstreaming" implies a convergence of outlooks among the heavy viewers in "otherwise" disparate and heterogeneous groups. Differences deriving from other factors tend to be reduced or even eliminated among heavy viewers in specific subgroups. These differential patterns may cancel each other out and thus not appear when looking only at overall relationships.

"Resonance" occurs when a given feature of the television world is most congruent with the real-life circumstances of the viewer. These are instances where specific issues have particular salience to people's everyday reality (or even perceived reality) and the combination "resonates" and amplifies cultivation.

Most of the major critiques of our work have focused on the question of controls, in one form or another.[1] Among Canadians, Doob and Macdonald (1979) controlled for neighborhood crime level and concluded that respondents' environments made any relationship between viewing and fear of crime utterly spurious. They neglected to acknowledge, however, that the relationship in question held up quite strongly for city residents—particularly those in high-crime areas.

We found parallel results in our own data (Gerbner et al., 1980a). The relationship between television viewing and fear of crime is strongest among low-income urban dwellers (who arguably are more likely to live in higher-crime areas). In addition, this association stands up under numerous controls, singly or *simultaneously.* This phenomenon is what we call "resonance"; i.e., special cases of particular salience may amplify television's impact.[2]

Hughes (1980), using some of the same GSS/NORC data as Hirsch, also added a few more controls, notably church attendance (also in Violence Profile No. 8), club membership, and hours working per week. Both Hughes and Hirsch implemented all controls simultaneously, and both convincingly demonstrate that this procedure in some cases results in either curvilinear or negative overall relationships; in most cases in the GSS/NORC dataset, the aggregate associations are reduced to trivial proportions.

But an overall aggregate relationship is simply the product of subrelationships which may tug and pull at each other in different directions and with varying intensity. Hughes's, Hirsch's, and our own reanalyses show quite clearly that for many questions—again, particularly those in 1977 GSS/NORC data—amount of viewing has no single, universal, across-the-board impact, in the same direction for all groups of respondents. To Hirsch in Part I, this seems to be the final word on the subject.

But he ignores a number of subgroup variations that almost jump out of his own tables. Also, in Table 5 he presents a problematic version of our "cultivation differential" and notes:

> A positive sign supports the cultivation hypothesis, for agreement with the "television answer" would be associated with more viewing. *A negative sign suggests there is no relationship between them* [italics in original].

Such an assertion is not only illogical, confusing, and scientifically indefensible, but it also blinds Hirsch to the more subtle aspect of the cultivation process, which we call "mainstreaming." More than anything else, and above and beyond a plethora of methodological quibbles we have yet to address, the empirical evidence leading to the concept of "mainstreaming" effectively obliterates Hirsch's "reanalysis." As we shall see below, his critique of "mainstreaming" in Part II fails to cast any doubt on the *validity of the concept,* thereby reaffirming our dismissal of Part I.

The foundations of "mainstreaming" were implicit in our early theoretical and conceptual considerations of the role of television in our society. We stressed television's central role in the mainstream of the culture, its celebration of conventional morality, and its potential for promoting homogeneity by crossing class, age, ethnic, and other boundaries. "The repetitive pattern of television's mass-produced messages and images forms the mainstream of the common symbolic environment that cultivates the most widely shared conceptions of reality" (Gerbner et al., 1978: 178).

In the early stages of our research, the number of "positive" cultivation differentials in specific demographic groups led us to stress what seemed to be happening for "most groups." While Hirsch was controlling for everything at once and finding no *overall* associations, we were paying closer attention to the "exceptions." Signorielli (1979) found that nonwhites are more "sexist" as a group, but that nonwhites show a significant *negative* association between amount of viewing and expressing sex-role stereotypes. Morgan and Gross (1980) found that adolescent heavy viewers score lower on achievement tests—unless they have low IQ's; low-IQ students show a significant positive association between amount of viewing and reading comprehension scores.

In these cases, light viewers of counterpart subgroups manifest wide baseline differences, but the heavy viewers' outlooks or scores reflect a convergence. Heavy viewing thus goes with a reduction of differences attributable to other variables. This kind of homogenization is obscured in measures of overall associations.

This same principle was also found in data we had previously analyzed, including the GSS/NORC data considered by Hirsch. Education was found to be a major control illuminating "mainstreaming." Less-educated people are far more likely to give "television answers" to "mean world" questions of interpersonal mistrust, alienation, and anomie; they also tend to show no relationships between expressing these views and amount of viewing. But among better-educated people—who, as light viewers, are relatively more trusting and less anomic—cultivation associations are enhanced. Most importantly, these relationships *withstand all other controls, singly or simultaneously* (Gerbner et al., 1980a). In some cases, we even found significant *negative* associations, even after controls, among extremely mistrustful groups.

Far from showing "no relationship," that "negative sign" is the key to the puzzle. While this recognition confirms our basic hypothesis—that television cultivates common conceptions of social reality—it refines, extends, and amplifies our conclusions. It also renders the remainder of Hirsch's criticisms in Part I irrelevant. Nevertheless, a few of his other points, as noted above, bear mention.

Part of his criticism is that our operational definition of light and heavy viewers varies across samples. Hirsch's charges of "shifting bases" serve only to distract attention from the larger issues. We have never implied nor argued that the terms "light" and "heavy" viewer are anything but *relative*, determined by the distribution of responses in any given sample. We approximate an even three-way split, tempered by judgment *and always clearly defined.* Any attempt to specify "absolute" levels of heavy viewing or absolute proportions of the sample is doomed to failure if these standards are applied to samples of different ages.

For example, in some of our adolescent samples, an even three-way split would require designating up to three hours a day as "light" viewing, so adjustments are made. In any case, we see self-reported viewing primarily as a useful ranking device and do not focus on specific hours of exposure. The groupings are helpful for illustrative purposes, and our increasing use of continuous data bypasses the problem completely. Unlike Hirsch, we do not take these self-reports at face value as accurate measures. We simply expect that those who report more than four hours a day do indeed consistently watch more than those who report less than two hours a day.

Hirsch has focused a major thrust of his critique on respondents in two extreme viewing categories. This analysis of "nonviewers" and "extreme viewers" in addition to the light, medium, and heavy categories and the supposed justification for it are heavy handed and unconvincing. It is a little like trying to study religion by comparing atheists and fanatic fundamentalists.

The two extreme groups together represent less than *ten percent of the GSS/NORC sample.* Moreover, since many of his reanalyses show monotonic associations among light, medium, and heavy viewers, but "nonviewers" scoring higher than "light" and "extreme" viewers scoring lower than "heavy," he has merely shown that some overall relationships *are* monotonic for over 90% of the population. Patterns of responses for these marginal groups are clearly of some interest, but they are irrelevant to cultivation theory because these groups probably differ from other viewers on uncontrolled third variables. At most, he has shown that their inclusion in our analyses means that our measures of cultivation are *underestimates* (see p. 439 of Part I).

In the case of the nonviewers, their complex and contradictory profile is even more problematic than their size. Jackson-Beeck (1977) found, and Hirsch concurs, that they constitute a bizarre and inconsistent segment. They are better educated than viewers and tend to work in higher-level careers; and yet they have significantly *lower* incomes (Jackson-Beeck, 1977) and had *higher* family incomes when they were 16 (Tankard and Harris, 1980). While they are more likely to have been raised in a "traditional," nuclear family, they tend to be unmarried and childless. They are more likely than are viewers to claim no religious preference (Jackson-Beeck, 1977); they also have a stronger view of themselves as religious but attend religious services less often (Tankard and Harris, 1980).

Hirsch insists that they are more anomic (which may not be surprising, given the above), while Tankard and Harris (1980) report that they are "happier with things in general." Measurement error may account for some of the perplexity, and factors (such as social desirability) which lead some respondents to report no viewing may color many other answers as well.

Extreme viewers are also problematic. Jackson-Beeck and Sobal (1980) examined some social and behavioral correlates of relatively extreme viewers. Their analysis is not fully comparable to Hirsch's since they defined "heavy" as over six hours a day, while Hirsch's extreme viewers report watching over eight hours a day. In any case, they pooled the 1975, 1977, and 1978 GSS/NORC samples, and found that heavy viewers (by their definition) constitute 5% of the three samples. These authors note that those who watch over six hours a day are likely to be women, young, nonwhite, homemakers, less educated, and less active socially; those who work tend to be in blue-collar occupations and have lower incomes.

Beyond these problems, his supposed demonstration of curvilinearity, based on "unexpected" findings from the small and bizarre group of "nonviewers," is utterly unsubstantiated. Let us ignore, for the moment, that some of the items he analyzes (such as approval of suicide) have no discernible basis in our TV message analyses or in any of our discussions of cultivation.

The fact is, *Hirsch's claims of nonlinearity are simply false.* In Table 1, we present the results of *tests* for linearity and nonlinearity, based on Hirsch's dependent variables *and Hirsch's viewing categories.* These are the same 18 items he discusses; we present 22 comparisons, however, because data for the 1977 and 1978 suicide questions are presented separately. Of these 22 comparisons, 17 (77%) show significant linearity beyond the .05 level. *Only one item is significantly nonlinear at the .05 level.*[3]

We know that "nonviewers" often seem to be more likely than light viewers to given "television answers." Also, it is not unreasonable to question whether they should be lumped with "light" viewers. But, given their trivial numbers, they cannot constitute grounds for claiming that relationships with viewing are nonlinear; they merely affirm that the simple relationships are indeed linear for over 90% of the population.

Hirsch then moves on to assess overall associations through Multiple Classification Analysis. (It would make more sense to begin with overall patterns and then turn to

TABLE 1
Significance of Linear and Nonlinear Trends, Based on the Simple Associations Between Hirsch's Items and Viewing Categories*

Alienation (1978)	Significance of Linearity		Significance of Non-linearity	
People running country don't care		.001		.33
Rich get richer, poor get poorer		.09		.29
What you think doesn't count		.004		.23
You're left out of things		.05		.07
Powerful people take advantage of you		.004		.74
People in Washington are out of touch		.07		.48
Meanworld (1978)				
People are just looking out for themselves		.15		.55
People would take advantage of you, given chance		.0001		.37
Can't be too careful in dealing with people		.0002		.87
Approval of Suicide	(1977)	(1978)	(1977)	(1978)
If incurable disease	.44	.0008	.98	.94
If bankrupt	.0002	.0000	.38	.26
If dishonored family	.0006	.0000	.91	.74
If tired of living	.02	.03	.38	.39
Lot of average man getting worse		.0003		.05
Not fair to bring a child into world		.0002		.30
Officials not interested in average man		.0000		.08

(table continued next page)

*Nonviewers; light viewers (1-2 hrs/day); medium viewers (3 hrs/day); heavy viewers (4 to 7 hrs/day); extreme viewers (8 hrs/day and up).

TABLE 1 (Continued)

Anomia (1977)	Significance of Linearity	Significance of Non-linearity
Ability to imagine a situation in which a man punching an adult male stranger would be approved by respondent* (1978)	.0004	.53
Fear of walking alone within a mile of home at night (1977)	.07	.66

*Hirsch calls this item "actual violence."

specifications.) Importantly, he does not tell us whether the "control" variables are entered as covariates or as competing independent factors.[4]

His neglect of subgroups in Part I all but invalidates his conclusions about our so-called "unreported discrepancies," as shown for a variety of analyses *based on a variety of samples* (Gerbner et al., 1980a). It is particularly evident in his analysis of the relationship between amount of viewing and what he calls "attitudes toward actual violence."

To begin, he claims that the following two items are "comparable":

—How often is it all right to hit someone if you are mad at them for a good reason? Is it almost always all right or almost never all right?

—Are there any situations you can imagine in which you would approve of a man punching an adult male stranger?

The first question was asked of adolescents; the second comes from the 1978 GSS/NORC survey. The GSS/NORC item was "never discussed or referred to" by us for two simple reasons: (1) 1978 NORC data were not available in time for our 1978 report, and our 1979 report dealt solely with adolescents; and (2) we were suspicious of its reliability. Specifically, the scale of items measuring situations in which violence might be approved is neither internally homogeneous nor unidimensional; Cronbach's alpha is only .32.

Moreover, these two questions are neither substantively nor empirically "comparable," as Hawkins and Pingree (forthcoming) note when discussing Hughes's (1980) parallel use of this question:

> Hughes' NORC questions asked people to *imagine a situation*, where Gerbner, *et al.*, asked children *how often is it all right?* Perhaps the NORC light viewers have better imaginations than heavy viewers. Hughes himself makes a similar argument about [other differences] [Hawkins and Pingree, forthcoming; italics in original].

Underscoring these contentions is Loftin and Lizotte's (1974) finding, based on GSS/NORC data, that *high-SES groups are more likely to respond affirmatively to this question.* This counterintuitive relationship also holds in the 1978 General Social Survey: those with higher occupational prestige ($r = .16$, $p = .000$), more education ($r = .20$, $p = .000$), and higher incomes ($r = .16$, $p = .000$) are more likely to be able to "imagine a situation in which they would approve of a man punching an adult male stranger." Yet, in our adolescent sample, the relationship between the supposedly comparable variable and an SES index is indeed negative ($r = -.12$, $p = .01$).

Thus, given the low reliability of the NORC question and its surprising relationship with background variables, we chose not to report or analyze its association with amount of viewing on the grounds that we cannot tell what indeed it is measuring. At the same time, even this questionable item provides evidence of mainstreaming, as seen on Table 2, which breaks down responses to this question according to Hirsch's viewing categories separately for college and non-college-educated respondents. It is worth noting the relationship is not significantly nonlinear for either group.

TABLE 2
Relationship Between Approving of a Man Punching an Adult Male Stranger and Hirsch's Viewing Categories, by Education (NORC 1978)

EDUCATION:	Non-Viewers	Light	Medium	Heavy	Extreme	Significance of: Linearity	Non-Linearity
No College	62.8	61.1	65.3	59.9	53.1	.43	.50
(N)	(43)	(411)	(193)	(264)	(49)		
Some College	82.9	79.3	75.6	56.7	66.7	.0002	.35
(N)	(41)	(285)	(86)	(67)	(6)		
Difference between education groups:	20.1	18.2	10.3	3.2	*		

*Too few cases in extreme viewing/high education group.

Among less-educated respondents, there is essentially no relationship. But among more educated respondents, whose light viewers (and even nonviewers) are quite likely to be able to "imagine a situation," the relationship with viewing is negative and significantly linear. Heavy viewing may thus "moderate" outlooks of "otherwise" extreme groups so that they converge into a more homogeneous "mainstream." Ignoring the college-educated "extreme" viewers (because there are only six of them), we find that the *difference* between more- and less-educated respondents *monotonically decreases at each subsequent viewing level.* The two groups of "nonviewers" are 20 points apart, while the "light" groups are 18, the "medium" groups 10, and the "heavy" groups only 3.

Further, as shown on Table 3, the relationship within the college-educated group withstands controls for sex, race, age, income, and residual variation in education itself, *either singly or all at once.* Thus, even this question, although it is fundamentally unclear what it in fact measures, provides another example where "mainstreaming" is totally masked in an overall trivial association.

If Hawkins and Pingree are correct (that light viewers are better able to "imagine a situation"), then we can conclude

TABLE 3
Simple and Partial Correlations Between Amount of Viewing and Approving of a Man Punching an Adult Male Stranger, Within Low and High Education Groups (NORC 1978)

	No College	Some College
Simple r	-.02	-.17*
Controlling for:		
Sex	-.02	-.18*
Age	-.03	-.17*
Education	-.02	-.16*
Income	.00	-.17*
Race	-.02	-.15*
All Controls	.00	-.14*
Final d.f.	887	459

*$p < .001$

that higher-educated people are also better able—*unless they are heavy viewers.* Heavy viewers in the college-educated group join those without college, resulting in a more homogeneous outlook.

In sum, Hirsch's Part I fails to demonstrate that our conclusions are unjustified. Many of the 18 items do not constitute fair or meaningful tests of the cultivation hypothesis because they are either irrelevant (with no basis in TV message analysis, as in the case of the suicide questions) or because they are of problematic reliability and validity (as with "approval of violence"). The charge that we are "shifting bases" by defining light, medium, and heavy viewers according to each sample's distribution is transparently simplistic.

Examining "nonviewers" and "extreme viewers" adds little to understanding the consequences of mass communication because both are tiny and bizarre groups.

Furthermore, Hirsch's claims of nonlinearity are statistically unsound; regardless of the inappropriateness of many items, the vast majority show significant linear trends with almost no significant deviations from linearity, across *his* five viewing groups.

Moreover, we are particularly affronted by Hirsch's insinuation that we have intentionally misreported data. He argues that, because we have used samples of different sizes and from different locations (which seems a reasonable way to help accumulate findings), and because we have used sample-relative distributions to categorize respondents' viewing patterns and have employed numerous statistical techniques, then "the question arises whether important issues covered by one or more of the samples are reported at all, and, if so, reported accurately."[5]

Above all, Hirsch's failure to consider differential patterns within subgroups and his emphasis on global associations blinds him to findings which may be more critical than any overall "effects." Our explorations of such specifications, which we analyze within the framework of "mainstreaming" and "resonance," show systematic and consistent patterns within subgroups. These concepts are considered—and rejected—by Hirsch in Part II; but as we shall show in the next section, his alleged disconfirmation is based on fundamental misconceptions.

PART II

> It is sometimes assumed, either explicitly or implicitly, that there is a single correct approach to survey analysis and that approaches which deviate from this path are in error. . . . Pure hypothesis testing is a valuable research model and should be employed where appropriate, but research can be severely cramped if it is employed as the *sole* method of analysis . . . in actual practice, much survey analysis involves the hot pursuit of an idea down paths and byways which have little to do with one's original hypothesis. . . . A reluctance to follow the lead of the findings

> may stultify and abort a good deal of promising research. . . . Although the professional literature tends to present its results within the hypothesis-testing framework, the published report may by no means correspond to the actual research procedures. . . . The history of science is replete with . . . serendipitous discoveries. . . . It may further be noted that in actual research practice the contrast between hypothesis-testing and post-factum interpretation is not so great as it may appear [Rosenberg, 1968: 197-238].

The flaws in Part I of Hirsch's "reanalysis" are compounded and overshadowed by the more serious gaps, confusions, and misrepresentations which permeate his second installment. As in Part I, his reliance on one sample, further contaminated by questionable items, provides no basis whatsoever for his dismissal of our accumulated findings. In particular, in this section we will show that:

(1) Hirsch distorts and convolutes cultivation theory and presents his erroneous straw-man extrapolations a if they were necessary, direct implications of our theory—only to refute them;
(2) contrary to these distortions and misrepresentations, "mainstreaming" and "resonance" are neither all encompassing nor unfalsifiable; and
(3) far from being the drastic "reformulations" he alleges, "mainstreaming" and "resonance" are explications of concepts deeply embedded in all our previous work.

We are disappointed in Hirsch's "critique" of our recent refinements. We had anticipated some challenging and novel insights into potential flaws in the conceptualization and analysis of these new ideas, and had expected careful scrutiny which might help develop theory and point toward directions for more research.

Instead, Hirsch demonstrates an astonishing ability to selectively attack limitations and ambiguities *which we explicitly acknowledge and discuss* in our work, and to present them as if he has discovered some "hidden" flaw which we are trying to obscure.

In Part II Hirsch makes the following claims:

(1) our initial hypothesis specified universal, across-the-board effects of television viewing on people's conception of social reality;
(2) growing aware of subgroup differences, we contended that real-life subgroups whose fictional counterparts are overly victimized will show the strongest cultivation patterns;
(3) finding this not to be the case, or ignoring the idea altogether, we scrambled around trying to find *post hoc* explanations for random or damaging subgroup patterns;
(4) these post hoc explanations are logically contradictory, ambiguous, and untestable, and thereby nonrefutable.

The first two claims are imaginary. They confuse clearly presented *speculations* with explicit *conclusions.* We were unable to find (and Hirsch failed to quote) any statements in our publications which assert absolute, global impact. From the earliest published cultivation analyses, the theory and the method focused on possible subgroup differences:

> All responses are related to television exposure, other media habits, and demographic characteristics. We then compare the response of light and heavy viewers controlling for sex, age, education, and other characteristics. The margin of heavy viewers over light viewers giving the "television answers" *within and across groups* is the "cultivation differential." . . . The analysis is intended to illuminate the complementary as well as the divergent roles of these sources of facts, images, beliefs, and values in the cultivation of assumptions about reality [Gerbner and Gross, 1976: 182; italics added].

Although this clearly provides for across-group comparisons, we do not, as Hirsch claims, "simply abandon without explanation" in Violence Profile No. 11 the examination of within-group cultivation differentials (a look at the tables in that report reveals that this claim is blatantly false). Moreover, Hirsch is incorrect when he states that our latest work "instead substitutes an entirely new method of measurement and statistical procedure," the "across-group" comparisons.

This is as confused and unfounded as his accusations that we have continuously "reformulated" our basic arguments and analyzed, using different techniques, "the same data from year to year." Our latest publication (Gerbner et al., 1980a) is the first time data have been reanalyzed and refinements offered to support what we believe to be an important theoretical development.[6]

In regard to point 2, while it is evident that we were aware of conditional relationships, we had not yet tested any specific hypotheses about what shapes they might take:

> The pattern of relative victimization is remarkably stable from year to year. It demonstrates an invidious (but socially functional) sense of risk and power. We do not yet know whether it also cultivates a corresponding hierarchy of fear and aggression [Gerbner and Gross, 1976: 191].

It is clear, as Hirsch notes, that we did later speculate that viewers "may be especially receptive to seeing how characters" like themselves fare in the dramatic world. But we made it equally clear in that same article that we did not offer this statement as an empirical finding:

> Television makes somewhat different contributions to the perspectives of different social groups. These differences cannot be expected to replicate the structure of power shown on television because many other factors enter into the overall determination of real-life relative powers [Gerbner et al., 1978: 206].

Yet, Hirsch asserts that our "first reformulation"—that cultivation will be most evident within the groups most victimized on television—"follows directly from the text of Violence Profiles 7 through 10." He reconstructs our theory to build in the assumption that this proposition implies that viewers will "adopt as their own attitudes and perceptions the same interpretations of television content" as we derive from message system analysis. These claims are nowhere to be found in our theory. They are *his*, not ours. It is Hirsch who "cuts loose" our message analysis from our cultivation analysis in imputing a level of conscious, isomorphic "interpretation." In clarifying a similar misconception of CBS a few years ago, we noted:

> We must repeat that the validity of a TV content indicator does not depend on viewers' conscious understanding of its meaning [Gerbner et al., 1977b: 286].

Hirsch also imputes into our theory a level of "identification" with television characters which we have never asserted. If anything, the available research (McArthur and Eisen, 1976; Miller and Reeves, 1976; Reeves and Miller, 1977) suggests that, at least for children, "identification" has far more to do with the availability and range of models presented than with one-to-one demographic correspondence between characters and viewers. Hirsch's version of our theory is symptomatic of a consistent effort to oversimplify it into a mechanical concept.

In any case, Hirsch's reformulation—that cultivation should depend upon how demographically similar characters fare in the TV world—is, although probably oversimplistic, far from uninteresting. One contribution of Hirsch's work is that it provides the first actual test of that proposition. His results suggest rejecting this hypothesis; still, we question the validity of the test, because of the small number of groups examined and the comparison of inappropriately matched groups.[7]

A more convincing test of this hypothesis (which we have begun) must be based upon a large number of groups. We report "risk-ratios" (reflecting relative likelihood of committing or suffering violence, and of killing or being killed) for five major variables which have measurable real-world demographic parallels: sex, age, race, occupation, and marital status. Each of these has two categories in our data base, except for age, which has three. This produces a total of 263 different combinations of characteristics, or 263 potentially definable groups. We are now examining the relationship between each character group's victimization likelihood and each real-life group's correlation between amount of viewing and perceptions of danger and are looking forward to determining the viability of this hypothesis.

In addition, Hirsch bases part of this analysis on the following NORC question (italics added):

> —Is there *any* area right around here—that is, *within a mile*—where you would be afraid to walk alone at night? (yes, no)

and concludes that there is no evidence that television cultivates "fear." In a 1979 national probability survey conducted as part of our research by the Opinion Research Corporation, we included a question which, while similar, seems to us more focused on people's real apprehensions:

> —How safe do you feel walking around in your own neighborhood *alone, at night*—very safe, somewhat safe, or not safe at all?

We found strikingly different results (see Gerbner et al., 1980a). The weak association with the NORC question may be in part due to insufficiently sensitive response categories as well as to the off-center focus of the question: Most of us could very likely think of *some* area where we would be afraid to walk alone at night; that does not mean most of us are necessarily fearful when we walk in our own neighborhoods.[8]

Still, it is worth noting that the cultivation of "fear" per se may be a "secondary" hypothesis. Our basic notion is that television should cultivate images of what "the world" is like. Since our message system analyses show over half of all leading characters involved in some kind of violence, year in and year out, we proposed the idea that television might cultivate the belief that a relatively large number of people are involved in violence. In nine out of ten samples,[9] we have found that greater television viewing (with or without multiple controls) goes with heightened estimates of the number of people involved in violence in the real world. Again, this finding represents the cultivation of a *conception of social reality*, an image of the world as a more or less violent place, and does not *necessarily* have any direct relationship to consciously experienced "fear."

By a natural extension, however, we wondered whether or not it might apply to personal projections of risk and danger. We found that, indeed, it *did* for children. For adults, using the GSS/NORC question analyzed by Hirsch (as well as by Hughes), we found *and reported* that it had a "slight tendency" to show a "weak" association with television viewing (Gerbner et al., 1978). It is hardly surprising or profound that a weak simple association disappears under simultaneous controls.

In any case, the ORC survey contains both questions—perceptions of the number of people involved in violence and level of safety in one's own neighborhood—and we have found that the two are indeed somewhat distinct concepts. The correlation between them is a relatively low .15; two-thirds (63.7%) of those who feel "very safe" in their own neighborhoods still overestimate the number of people involved in violence, despite their sense of relative security. (Conversely, 38.6% of those who inflate the proportion of people involved in violence nevertheless feel safe in their own neighborhoods.)

Even more interesting is the way each of these variables conditions the association between viewing and the other—results that reveal "mainstreaming." Figure 1 shows that the relationship between amount of viewing and exaggerating the number of people involved in violence decreases monotonically as fear of walking in one's own neighborhood increases. Those who feel "very safe" in their own neighborhoods are relatively unlikely to overestimate the proportion of people involved in violence—unless they are heavy television viewers. Similarly, light viewers who do not overestimate the proportion of people involved in violence are likely to feel "very safe" in their own neighborhoods. In both cases, the differences deriving partly from other dispositions are reduced—the proportion of the heavy viewers in these groups who give the TV answer is closer, thus reflecting a "mainstream" commonality of outlooks.

Turning to points 3 and 4, it is clear that the most important issue in Hirsch's Part II is the role of "mainstreaming" and "resonance" in cultivation theory. It is particularly difficult to respond to Hirsch's "critique" of these concepts because it is not apparent that he understands them. In his haste to manufacture "contradictions" in our position, he fails to consider rather obvious grounds for falsification and constructs an unfair and incoherent explication of their meaning.

He begins by presenting his reformulation of our "original version," and argues that it is in conflict with "mainstreaming" and "resonance." As we state in Violence Profile No. 11, cultivation is often a virtually across-the-board phenomenon. It is quite clear from our article that "mainstreaming" and "resonance" *deal with the exceptions.* The refinement which aggravates Hirsch so is simply the proposition that many (if not most) of the specifications which emerge when overall relationships disappear, as well as other systematic variations in susceptibility to cultivation, can be explained by one of these two concepts.[10]

His disregard of our observation that a majority of cultivation questions do show consistent and robust effects for most groups is based on his use of a number of problematic items from virtually one data base, which, for whatever reasons, show incongruous results. Other data of comparable quality, representativeness, and scope show dramatically different patterns. Table 4 shows within-group

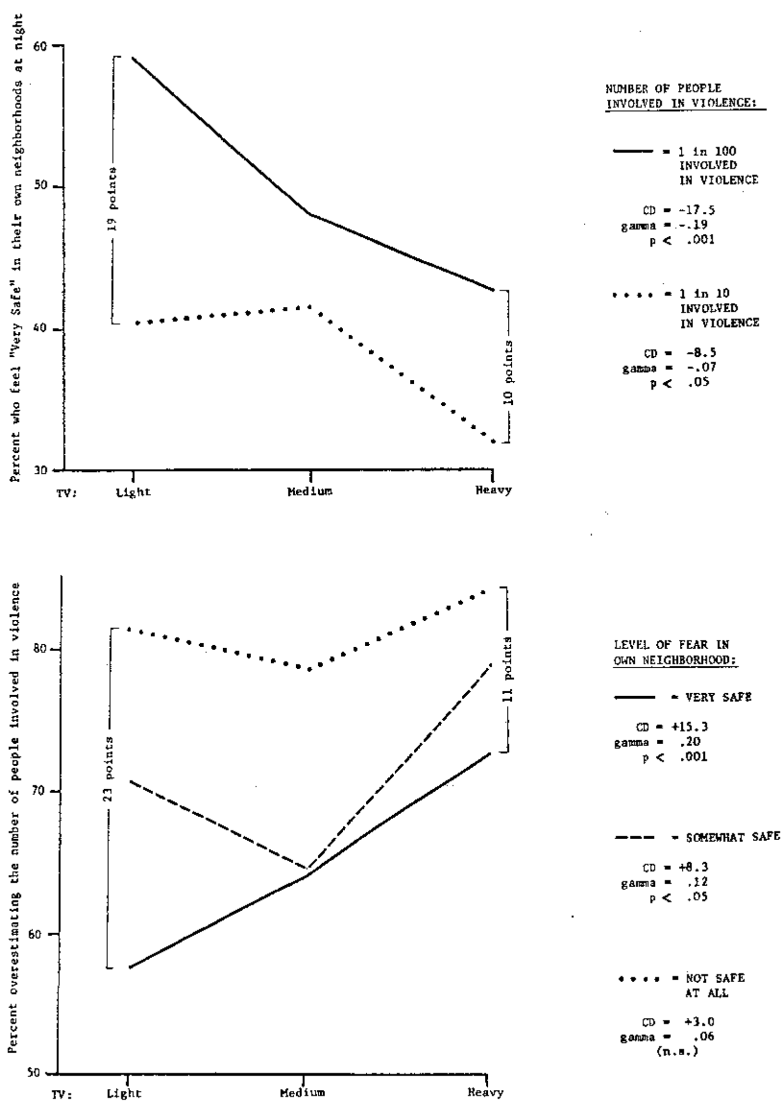

Figure 1: "Mainstreaming" in Conditional Associations Between Amount of Television Viewing, Neighborhood Fear, and Estimation of the Number of people Involved in Violence (ORC data)

TABLE 4
Within-Group Partial Correlations Between Amount of Television Viewing and an Index of Perceptions of Violence and Danger (ORC data)

	Simple r	Sex	Age	Education	Residence	Income	Race	Newspaper Reading	ALL CONTROLS	(df)
OVERALL	.17***	.16***	.17***	.14***	.18***	.15***	.16***	.17***	.11***	(4980)
AGE										
18-29	.22***	.22***	.22***	.19***	.24***	.22***	.20***	.22***	.19***	(1589)
30-54	.14***	.14***	.14***	.11***	.14***	.11***	.13***	.15***	.07***	(2049)
55+	.13***	.11***	.13***	.13***	.13***	.09***	.15***	.13***	.10***	(1320)
EDUCATION										
No College	.17***	.16***	.17***	.17***	.16***	.15***	.16***	.17***	.14***	(3467)
Some College	.12***	.11***	.12***	.10***	.12***	.10***	.11***	.12***	.08***	(1505)
NEWSPAPER READING										
Everyday	.14***	.12***	.13***	.11***	.15***	.11***	.13***	--	.09***	(3230)
Sometimes	.25***	.24***	.24***	.21***	.25***	.23***	.24***	--	.17***	(1302)
RACE										
White	.17***	.15***	.16***	.14***	.17***	.15***	--	.17***	.12***	(4405)
Non-white	.14***	.13***	.17***	.13***	.11***	.08*	--	.14***	.07 (p=.06)	(569)
RESIDENCE										
City over 250,000	.21***	.21***	.18***	.06*	--	.16***	.18***	.20***	.00	(898)
City under 250,000	.22***	.23***	.23***	.17***	--	.23***	.21***	.22***	.21***	(561)
Suburb	.18***	.14***	.18***	.15***	--	.16***	.18***	.18***	.10***	(1915)
Non-metro.	.13***	.12***	.13***	.13***	--	.11***	.13***	.14***	.11***	(1583)
INCOME										
Under $10,000	.20***	.19***	.19***	.18***	.19***	.20***	.19***	.20***	.17***	(1777)
$10-25,000	.10***	.09***	.10***	.08***	.11***	.10***	.10***	.10***	.08***	(2240)
Over $25,000	.15***	.12***	.15***	.12***	.15***	.15***	.16***	.15***	.08**	(946)
SEX										
Male	.16***	--	.16***	.12***	.16***	.11***	.14***	.15***	.09***	(2350)
Female	.17***	--	.16***	.14***	.17***	.15***	.16**	.17**	.13***	(2623)

*$p < .05$; **$p < .01$; ***$p < .001$

partial correlations between amount of viewing and scores on an index of perceptions of violence and danger drawn from questions in our ORC survey. There are seven control variables; each row presents the correlations for each subgroup, controlling for all other variables (and residual variation in the variable itself, when it is continuous), singly and simultaneously. Clearly, the associations between amount of television viewing and this index are persistent and potent.

Anticipating Hirsch's rejoinder that statistical significance is "an artifact of sample size," we would remind him that the larger the sample, the less likely the obtained coefficients are due to chance. As we have often argued, the "size" of an effect may be less important than the direction of its steady contribution.

As further confirmation of our belief that "positive" cultivation effects hold for "most groups," we present summary data from the same ORC survey in Table 5. This table summarizes the effects of single and simultaneous controls on the five variables which make up the index of perceptions of violence and danger.[11] The middle row is particularly provocative given Hirsch's claims that "practically any two" controls, when applied together, wipe out cultivation. This row represents "two controls"—in turn, each category from Table 4 is held constant along with one other variable which is partialled out—*and 483 out of 580 "double-controlled" correlations (83.3%) remain positive and significant.* In this light, our contention that "most groups" show evidence of cultivation hardly deserves Hirsch's sarcasm. But even this only tells part of the story.

Hirsch's superficial and slanted recounting of "mainstreaming" and "resonance" reflects either incomprehension or misrepresentation. The accusation that they are "all-encompassing" and nonfalsifiable reveals scanty contemplation. It does not require much effort to generate numerous conditional associations which would not support either one. He paints them as contradictory opposites (and also contradictory to our "original formulation"), and as "all-purpose" explanations, when in fact they are complementary processes which are proposed as applicable to "many" subgroup differences.

His overstated concern about specifying the conditions under which either (or neither) will occur overlooks and belies one fundamental fact—*that nonspurious and meaningful specifications do indeed exist in the very data he concludes show no associations with amount of television viewing.* By portraying them as all-encompassing (which they are not), he sidesteps the realization that certain identifiable subgroups show systematic, nonspurious, and significant cultivation patterns even

TABLE 5
Summary of Simple and Partial Within-Group Correlations Between Amount of Viewing and Perceptions of Violence and Danger (ORC data)

	Number of Correlations which are:			
	Positive and Significant	Positive and Non-Significant	Negative and Non-Significant	Negative and Significant
Within-Group Simple Correlations (N=90 r's)	77 (85.6%)	9 (10.0%)	4 (4.4%)	0 (0.0%)
Within-Group First-Order Partial Correlations (N=580 r's)	483 (83.3%)	67 (11.5%)	27 (4.7%)	3 (0.5%)
Within-Group Partial Correlations with all controls * (N=90 r's)	55 (61.1%)	24 (26.7%)	9 (10.0%)	2 (2.2%)

*"All controls" includes residual variance in the controlling variable, where continuous; e.g., residual variance in income is held constant within any given income category, in addition to all other controls.

where overall relationships disappear. In Part II, he all but abandons data which might support or refute his conclusions. *Where are the data* to show that these conditional associations are indeed spurious or nonlinear? He argues that they are with great passion but absolutely no evidence.

Clearly, both "mainstreaming" and "resonance" are falsifiable. Figure 2 presents a variety of possible conditional associations; in these figures the amount of television viewing is the x-axis, and some assumption, belief, or conception about social reality is the y-axis. Graphs a, b, and c show examples of "mainstreaming," in

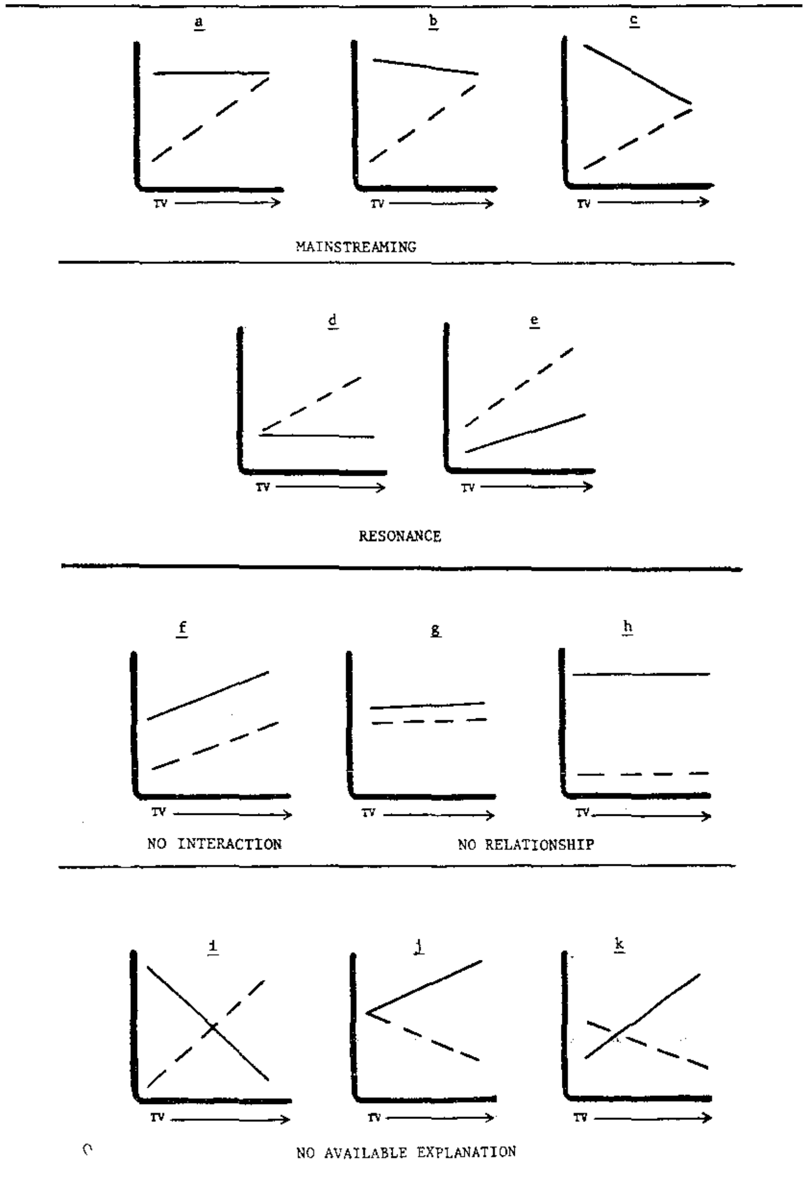

Figure 2: Hypothetical Within-Group Specifications

which the outlooks of heavy viewers are more homogeneous. Graphs d and e show "resonance," where, for some meaningful reasons, a given message is highly salient to one subgroup. The relationship may hold for both but be amplified for one, as in graph e.

The remaining six graphs show neither "mainstreaming" *nor* "resonance." In f, the relationship holds for both groups, despite baseline differences. In g and h, there is no relationship at all. There are clear relationships in i, j, and k, but they would not fit any available explanation. If patterns like these occur, they may indeed reflect "real" effects of television, but they would by no means be accounted for by the phenomenon of "mainstreaming" or of "resonance."

The point is that conditional patterns within subgroups may take on a wide range of forms. Given variables which have little or no overall relationship with viewing, if the bulk of the subgroup patterns fit into the shapes hypothesized in graphs g,h,i,j, or k—or many possible others not shown—it would provide unambiguous falsification of "mainstreaming" and "resonance." Hirsch's comment that we would explain the absence of within-group relationships by saying that "mainstreaming" and "resonance" are happening simultaneously and canceling each other out, while ridiculous, raises an important point.

If we were to find an unexpected within-group relationship of zero, we would not claim that "mainstreaming" and "resonance" were canceling each other out like Dr. Doolittle's "Push-Me-Pull-You." Hirsch's simplistic accusation overlooks the fact that further examination of differences within that group—controlling for additional variables—might reveal a theoretically intelligible pattern underlying the apparent lack of relationship with television viewing. A single variable would not simultaneously generate both "mainstreaming" and "resonance," but the implementation of additional controls might uncover their presence in distinct subsets of the group, just as it does in an overall association. This kind of elaboration is a fundamental task of scientific analysis—to delve ever more deeply into phenomena, to examine layers of association, with an ever-sharper focus.

The fact is that the criteria by which the constructs of "mainstreaming" and "resonance" could be falsified are painfully evident. We did not present them in a "pure hypothesis-testing framework" because they are indeed post factum interpretations. That is why they need further examination, in a broad range of cultivation issues. Our latest findings—as well as those of the California State Board of Education (1980) and Lull et al. (forthcoming)—merely suggest that stopping at the point of finding no overall associations may prevent the discovery of systematic processes which are both more subtle and more profound—and which Hirsch would bury.

These remarks should not be taken as a reflection of Olympian smugness. We welcome critiques and find them, for the most part, helpful; they indicate a healthy scientific skepticism and often lead us to more rigor, new directions, and valuable insights.

But, we must wonder, what compels the gleeful and sarcastic hostility in Hirsch's two pieces? We are saddened by his ad hominem intimations. Surely, it is possible to reappraise our work and reach different conclusions without virulence; see Hughes (1980) for one example. Hirsch's vituperations are embarrassing, unbecoming, and serve no scientific purpose; we regret if he has provoked harsh treatment from us.

Ultimately, "mainstreaming" and "resonance" may lead to more questions. As we continue to expand the focus of our work—into the cultivation of images about sex roles, age roles, health, education, marriage and the family, occupations, science, courts and law, religion, and more—and attempt to investigate "mainstreaming" and "resonance" in this larger context, we look forward to a more even-handed and collegial scientific scrutiny and dialogue.

NOTES

1. An exception is Wober (1978), who failed to replicate our findings in Britain, but he also failed to replicate our design or our measures (Gerbner et al., 1979a; Neville, 1980).

2. Several additional instances of this concept are presented in Gerbner et al. (1980a).

3. In 19 of these comparisons (86%), the nonlinear trend does not even reach the .10 level of significance; only in two cases is the significance of linearity *not* below .10.

4. We tried it both ways for one of his items ("approval of violence") and found that which procedure is followed makes a difference. The covariate method produces a lower R^2, and stronger coefficients for television. The simultaneous independent method reveals a significant interaction between viewing and education ($p < .05$), which we discuss below.

5. It takes little effort or ability to do a hatchet-job on *any* social science research. Hirsch himself provides ambiguous or no information about which NORC datasets are used in certain analyses, beyond that they are either 1975, 1977, or 1978. For his description of the univariate distribution of television exposure, he uses all three years. Similarly, he combines all three samples when comparing his five viewing groups to background factors. But in only one analysis of the relationship between viewing and attitudes does he again use data from 1975. Further, the sample years are not specified on the tables. Some items are only in the 1977 GSS/NORC (like anomia), some are only in 1978 (like alienation), and some are in both (like suicide). He never explains which sample years are used, sometimes employing just one when two years are available.

6. In both Parts I and II, Hirsch argues that Violence Profile No. 10 (1979b) suggests that "each 'latest' statistical procedure is to supersede all previous reported results," and we find this puzzling. The only possible explanation stems from our statement in the introduction to the Technical Report that the data from previous years is summarized in each year's Technical Report, which also "presents trends for all years studied." While this statement could specify more clearly that it applies only to message analysis data, which are indeed "superseded" each year because they are included cumulatively in each subsequent report, the parallel claim of new statistical techniques appearing annually is nonsense. The only evidence we could find for this claim is in our 1978 Technical Report, in which we attempted to summarize all our previous cultivation analyses. Specifically, we presented data on individual items from ten samples (48 tables), a few of which had been reported in index form in 1977; this effort to archive results from comparable questions across samples hardly represented a "reanalysis."

7. He equates "nonwhite" characters with "black" respondents, as well as male characters who are "American, white, middle class, and in the prime of life" with respondents who are simply "white men." This further clouds the value of his test.

8. It is not likely that the difference derives simply from "house effects." Smith (1978) notes that "don't knows" and "no answers" are a common indication of such "house effects," but the proportions not answering the questions are comparable—0.7% in GSS/NORC and 0.9% in ORC. More likely, the response

categories for the GSS/NORC question are too crude to detect associations. This not only helps account for why television viewing is only weakly related to the NORC question, but may also explain why *other* variables are more strongly related to degree of neighborhood fear in the ORC data. In parallel simultaneous multiple regressions of neighborhood fear onto sex, age, income, race, and education, substantially stronger relationships were found between demographics and fear in the ORC data than in the GSS/NORC data for all predictors except sex. All betas in the ORC regression were significant beyond $p < .001$; in NORC, while most were significant, education was not. Even within the ORC data, all regression coefficients are weaker when the fear variable is dichotomized. Thus, the relatively negligible predictive power of the demographics in NORC does not establish, as Hirsch claims, a basis for rejecting the theory of cultivation; rather, it suggests that the GSS/NORC question itself is weak.

9. This relationship holds in two national probability adult samples (ORC—1974 and 1979), one national quota sample (Starch—1974), four samples of adolescents, one of college students, and one of Philadelphia adults. The one sample showing no association (also Philadelphia adults) was asked in open-ended, rather than forced-choice, questions.

10. We also note that other factors may enhance or diminish cultivation, such as absence of direct experience, parental involvement in viewing, and peer-group integration (Gerbner et al., 1980a).

11. A description of the components and their reliability can be found in Violence Profile No. 11.

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George Gerbner is Professor of Communications and Dean of The Annenberg School of Communications at the University of Pennsylvania. He *is editor of the* Journal of Communication *and of* Mass Media Policies in Changing Cultures *(Wiley Interscience: 1978). His research is on the social aspects of mass communications.*

Larry Gross is Associate Professor of Communications at The Annenberg School of Communications at the University of Pennsylvania. He is coeditor of Studies in Visual Communication. *His research is in the cultural determinants of symbolic behavior.*

Michael Morgan is Research Specialist at The Annenberg School of Communications at the University of Pennsylvania. His research is on television's influence on role socialization and academic achievement.

Nancy Signorielli is Research Coordinator at The Annenberg School of Communications at the University of Pennsylvania. Her research is on television images and their effects upon people's conceptions.