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FINAL REPLY TO HIRSCH

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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" and 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

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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."¹ 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 dataset. 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 dataset 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 "re-analysis," 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?"², 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.³

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 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 be-

cause 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, and so forth) 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 the 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.⁴ 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.⁵ The adolescent question asks *how often is it all right* (a value judgment 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 question 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 counterintuitive 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 "counterintuitive" 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 is 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 (1972: 265) 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."

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 our 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.⁶ 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 schoolchildren) 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.⁷

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, and so on. This takes an 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.⁸ 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.⁹

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 datasets, 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 "counterintuitive" 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, and so forth—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 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: 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 (1981: 79) rejects them as "speculative, nonpredictive, unspecified, post hoc, and irrefutable." 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 causal 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.¹⁰ In addition to the work of Dominick and Greenberg (1972), Lull et al. (forthcoming) 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 education (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 (forthcoming) 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.¹¹

"Nonpredictive." Whether "mainstreaming" and "resonance" are "nonpredictive" is yet to be seen. When the concepts were introduced, we (1980a: 16) 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.

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* expectations, 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, Hirsch (1981: 83) 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"* (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 "nonpredictive" 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 override 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."¹²

"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,¹³ 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., nonrefutable). 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.*¹⁴ 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.

Whatever the original impetus for the critique 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.

NOTES

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 years-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 as 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/nonwhite), 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), Elliott and Slater (1980), Freuh and McGhee (1975), Haney and Manzolati (forthcoming), Neville (1980), Tan (1979), Vc'gy 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.

REFERENCES

- BEUF, A. (1974) "Doctor, lawyer, household drudge." *J. of Communication* 24: 142-145.
- BRYANT, J., R. A. CARVETH, and D. BROWN (1981) "Television viewing and anxiety: an experimental examination." *J. of Communication* 31: 106-119.
- California State Department of Education (1980) *Student Achievement in California Schools: 1979-80 Annual Report*. Sacramento: California Assessment Program.
- DOMINICK, J. R. and B. S. GREENBERG (1972) "Attitudes towards violence: the interaction of television exposure, family attitudes, and social class," pp. 314-335 in G. A. Comstock and E. A. Rubinstein (eds.) *Television and Social Behavior, Vol. III*. Washington: Government Printing Office.
- ELLIOTT, W. R. and D. SLATER (1980) "Exposure, experience, and perceived TV reality for adolescents." *Journalism Q.* 57: 409-414, 431.
- FREUH, T. and P. E. MCGHEE (1975) "Traditional sex-role development and amount of time spent watching television." *Child Development* 11: 109.
- GERBNER, G. (1980) "Sex on TV and What Viewers Learn From It." Paper presented to the National Association of Television Program Executives Annual Conference, San Francisco.
- L. GROSS, M. JACKSON-BEECK, S. JEFFRIES-FOX, and N. SIGNORIELLI (1978) "Cultural indicators: Violence Profile No. 9." *J. of Communication* 28: 176-206.
- GERBNER, G., L. GROSS, M. MORGAN, and N. SIGNORIELLI (1980a) "The 'mainstreaming' of America: Violence Profile No. 11." *J. of Communication* 30: 10-27.
- (1980b) "Television's Contribution to Public Understanding of Science: A Pilot Project." The Annenberg School of Communications, University of Pennsylvania.

- GERBNER, G., M. MORGAN, and N. SIGNORIELLI (forthcoming) "Programming health portrayals: what viewers see, say, and do," in National Institute of Mental Health, *Television and Behavior: Ten Years of Scientific Progress and Implications for the 80's*.
- HANEY, C. and J. MANZOLATI (forthcoming) "Television criminology: network illusions of criminal justice realities," in E. Aronson (ed.) *Readings about the Social Animal*. San Francisco: Freeman.
- HAWKINS, R. P. and S. PINGREE (forthcoming) "TV 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*.
- HIRSCH, P. M. (1981) "Distinguishing good speculation from bad theory: rejoinder to Gerbner et al." *Communication Research* 8, 1: 73-95.
- HORNIK, R. C., J. GOULD, and M. GONZALEZ (1980) "Susceptibility to media effects." Presented at the International Communication Association Annual Conference, Acapulco, Mexico.
- LOFTIN, C. and A. LIZOTTE (1974) "Violence and social structure: structural support for violence among privileged groups." Presented to the American Sociological Association, Montreal.
- LULL, J., A. MULAC, and S. L. ROSEN (forthcoming) "Feminism as a predictor of mass media use." *Sex Roles*.
- McINTYRE, J. J. and J. J. TEEVAN, Jr. (1972) "Television violence and deviant behavior," pp. 383-435 in G. A. Comstock and E. A. Rubinstein (eds.) *Television and Social Behavior*, Vol. III. Washington: Government Printing Office.
- McLEOD, J. M., C. K. ATKIN, and S. H. CHAFFEE (1972) "Adolescents, parents, and television use: self-report and other-report measures from the Wisconsin sample," pp. 239-313 in G. A. Comstock and E. A. Rubinstein (eds.) *Television and Social Behavior*, Vol. III. Washington: Government Printing Office.
- MORGAN, M. and L. GROSS (1980) "Television viewing, IQ, and academic achievement." *J. of Broadcasting* 24: 117-133.
- NEVILLE, T. (1980) "Television viewing and the expression of interpersonal mistrust." Ph.D. dissertation, Princeton University.
- SIGNORIELLI, N. (1979) "Television's contribution to sex-role socialization." Presented at the Telecommunications Policy Research Conference, Skytop, Pennsylvania.
- TAN, A. (1979) "TV beauty ads and role expectations of adolescent female viewers." *Journalism Q.* 56: 283-288.
- VOLGY, T. and J. SCHWARTZ (1980) "TV entertainment programming and sociopolitical attitudes." *Journalism Q.* 57: 150-155.
- WERNER, A. (forthcoming) "The mainstreaming function of television: the case of attitudes to the USA among adolescents in Norway."
- ZILL, N. and J. PETERSON (1980) "Television and children's intellectual development: results from a national sample of youth." Presented at the Annual Conference of the American Association for Public Opinion Research, Cincinnati.