

September 13, 1974

Dr. William J. McGuire
Department of Psychology
Yale University
333 Cedar Street
New Haven, CT. 06510

Dear Bill:

I hope that Bob Shayon has reached you by now and that you will be able to give a colloquium on November 4, and perhaps even stay over for a faculty seminar the next day.

That would be a good time and occasion to talk. If that is not convenient, we hope to see you some other time.

I am enclosing the Ideological Perspectives piece (a fairly cumbersome method I would not recommend anymore), and the grant renewal proposal (now about a year old) of our main current project. The grant was approved and we are now in its second year. But of course we have had seven years of prior research behind this.

I am also enclosing a current issue of the Journal of Communication which we now edit and publish. I think you will agree that it has become a lively journal, and we hope that it will fill a void in the field. I would like to invite you to contribute to it.

We are indeed thriving and are looking forward to seeing you here.

Sincerely yours,

George Gerbner

GG:kas
encls.

TO: George

FROM: Larry

DATE: September 18, 1983

RE: Comments on McGuire Handbook chapter

1. The argument that empirically demonstrated media 'effects' are small is not unreasonable. But it should be noted that there is a difference between 'small' and 'negligible' which the chapter text does not always acknowledge. The approach used in the chapter may influence the interpretation of this fact: the chapter deals with attitude change and, as we have often noted, the primary consequence of media

exposure may be the prevention of change (the maintenance of

established attitudes) which most research does not consider or estimate (and which most methods have difficulty assessing). As Bill knows, messages can also serve to increase resistance to persuasion.

The earlier edition of this chapter (2nd edition) reached identical conclusions -- the difference here being more recent references, some additional 'salvage' hypotheses, and the inflation of ad expenditures from \$20 to \$50 billion. There doesn't appear to be any change in the somewhat narrow focus on 'change,' but rather the addition of new studies to the previous conclusions.

There is no mention of the vast amount of unpublished, proprietary market research, which may or may not show such extravagant waste of sponsor monies. For those who have to spend money on persuasive messages, the costs of not advertising or running media campaigns may

vastly exceed what they do spend, since their competition is advertising, and small (negligible?) increases in market shares or votes may bring huge profits, or victory. (I think I just read that a single Nielsen point in the national news ratings is now worth \$30 million a year, although well within the margin of statistical error.)

2. The section on page 61, dealing with the "impact of tv program violence on viewer aggressiveness," is unfortunate from our perspective right from the title, because it reinforces the assumption that the primary 'effect' of tv violence is viewer aggression -- an assumption we question theoretically, and empirically. As a result, our contrary

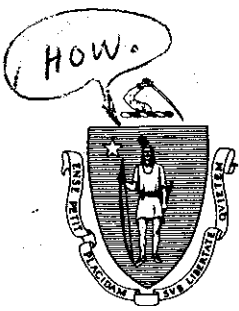
view that tv violence cultivates fear doesn't appear until page 63, in a different context (and with other problems, of which more later).

3. The statement (page 61) that "The violence counts have methodological flaws..." is quite objectionable, unsubstantiated and misleading. He could say: 'controversial,' 'debatable,' or somesuch, if it is necessary to get into these issues -- but, if the issues are raised it would be appropriate to acknowledge our responses to the criticisms (I think the Signorielli, Gross & Morgan chapter in the 1982 NIMH volume does a good job of responding to the issues McGuire mentions, and thus deserves notice even if its seamless arguments have failed to enlighten the heathen).

4. The discussion of cultivation theory and attendant controversies on page 3 is also quite unsatisfactory. Given the abundant use of references, it would be nice if more of the ones which independently support our findings were included (Hawkins and Pingree, Neville, etc.). The mention of "other relevant items" not showing the expected relationships could be improved with a reading of Hawkins and Pingree's NIMH paper, concluding that those "other relevant items" are not so relevant, and that the rationale for including them "is so lacking" that the results should be ignored. Another case in point is that set of items about whether the respondent thinks its ever ok to punch someone (on the NORC General Social Survey), where Hirsch accuses us /f suppressing items which "dont wori" despite our explanation of the internal contradictions within the set of items which led us to discard them; and the subsequent (uncited) article by James Davis of NORC sayinge these items were useless because of these very internal contradictions.

5. Along the same lines, in the same paragraph, the reference to Doob and MacDonald is also erroneous: it is not the case that the redationship held "across but not within" neighborhood groups. Indeed it did hold in high-crime areas, especially cities (another impmrtant specification). He does hint at the possibility of specifications (p.65), but dmesnt mention ours -- although the introduction of such specifications is the key to our mainstreaming/resonance analyses. In fact, the whole issue of political 'effects' which we deal with in "Charting the mainstream" is relevant to the discussion of specifications (looking at 'susceptible populatio.s' is only one part of this issue), and to other points addressed (political outcomes, agenda setting, etc.).

In general I have the impression that Bill is seeing us as more preoccupied with vioence lence and basic demographics, and is ignoring the other dimensions of our research.



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September 20, 1983

Gerbner & Gross et al., Inc.
The Annenberg School of Communications
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3620 Walnut St. C5
Philadelphia, PA 19104

Dear George and Larry:

Larry had warned me of, and sent, Bill McGuire's "ultra-short" (!) version which I had but skimmed; George's just-arrived scribbled plea along with McGuire's correspondence and the pages in question have prodded me into a more focused reading.

To wit:

His primary argument (quite clear in his letter, less so in the draft) that empirically demonstrated media "effects" are small is not unreasonable. There is, however, a considerable difference between the notion of "small" effects and the notion of "negligible" effects. He slips back and forth between these two in subtle and not-wholly-justifiable ways.

This may, in part, be a function of what he's dealing with -- attitude and behavior change. His earlier opus in the previous edition of the Handbook reached identical conclusions -- the only differences here being more recent references, some additional "salvage" hypotheses, and the inflation of ad expenditures from \$20 billion to \$50 billion. My point is that, within the rather narrow confines of "change" criteria, he seems to have been influenced by his earlier conclusions, merely plugging in more recent studies.

Relatedly, he does not mention the reams of unpublished, proprietary market research, which may or may not show such extravagant wastes. For those who deal with persuasive communications, the costs of not advertising or running media campaigns may vastly exceed what they do spend, since small (negligible?) increases in market shares or votes may bring huge profits, or victory.

Anyway, the problems in his treatment of our work are, I'm sure, rather obvious, but I'll mention them briefly.

His statement that the "violence counts have methodological flaws" (p. 61) is unnecessary, unsubstantiated, and misleading. He could say "controversial," "debatable," or something similar if he needs to qualify (in which case, references to our rebuttals should be included), but "flaws"??

Similarly, at the least he should cite our responses to Hirsch and Hughes (on p. 63), including the "mainstreaming" paper, cited elsewhere, which show that while multiple controls do indeed eliminate some overall associations, many specifications and conditional patterns hold (under multiple controls) within groups, in theoretically meaningful ways.

Even worse is his mention of "other relevant items" not showing the expected relationships; cf. Hawk and Suzanne's NIMH paper's conclusion that those "other relevant items" are not so relevant, and that the "rationale" for including them is "so lacking" that the results should be dismissed.

Worst of all is the citation of Hughes and Hirsch as showing that the "most realistic and refined measures tend to show the least effects" (p. 64), which is strange and nonsensical.

Re Doob and Macdonald, it is purely and simply false (and he should know, since I recall we sent him some of that stuff) that the relationship held "across but not within" neighborhood groups. Indeed it did hold in high-crime areas, especially cities (another important specification). He does hint at the possibility of specifications obscuring overall effects (p. 65), but doesn't mention ours!

Some miscellaneous notes: On p. 61 he says that "an even higher level of violence is depicted in popular novels and motion pictures," which is, conspicuously, one of the few sentences without a reference. His conclusion on violence that "Perhaps the public have the right to get what they want..." might be okay if they were ever given a choice. Cute, but inappropriate and misleading. Also, I was frustrated by the way he tended to continually undercut his 14 "excuses" (his word) for the size of the obtained effects.

There's nothing I've said which isn't on the obvious side, but I hope it's of some help.

On "TIO" -- Horowitz's suggestions are fine with me, and I'll work on them as soon as I get a chance to breathe. Also, I've enclosed a copy of the first draft (as-sent) of my Television Quarterly review of the NBC study. On our book plans, I am obviously well behind our schedule. I can only plead first-semester-new-courses distraction and hysteria, and hope that I'll make some progress on it if a terminal arrives.

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Other than all this, all of us here miss all of you there, and hope all is well with the new academic year. And don't forget, the Morgan-Rothschild Motel is always open for business.

Best,

Michael Morgan
Michael Morgan,
Assistant Professor

MM/bwl

October 25, 1983

William J. McGuire
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New Haven, CT 06520

Dear Bill:

I am sorry it has taken me so long to respond to your letter of September 8. Even now, I must do it in a rush and late at night, as well as very sketchily, but I hope I can still make some points we believe to be important.

Larry already gave you a copy of his notes which note many of the specific points we believe need correction. Let me deal with the more general aspects.

Your review of research is couched in the terminology of attitude and persuasion research, focusing on the measurement of change. That misses the major innovative aspect of cultivation analysis, which focuses on the stabilizing aspects of media. Therefore, the strategy of design and methodology must be very different, and the magnitudes of differences that one can expect to find are also different.

The cultivation model assumes that television is the mainstream of our culture. Children are born into it and absorb its lessons, but not uniformly. In any case, there is no "before" against which to measure change. When everyone is swimming (or drifting) in the same river, differences in direction and velocity depend on the location of the swimmer (or drifter), and a few social and individual characteristics, but they are expected to be relatively small.

We see television viewing as an activity integrated into stable lifestyles. What conceptions of reality (and I don't like to refer to attitudes because my own feeling is that the major associations are with "construction of reality" types of concepts) are cultivated depends on demographic and lifestyle characteristics. We measure the independent contribution of television to these conceptions by comparing heavy and light viewers in different groups, holding other characteristics constant. But because both heavy and light viewers are members of the same culture, and both are increasingly members of the "television generation," the range of differences we typically find among them are comparable to those between other demographic characteristics. In other words, we see television functioning as age,

sex, income, etc. On some issues it makes a difference whether you are a man or a woman. On others it does not; on some it makes a difference whether you are young or old, rich or poor, on others it does not. The average differences tend to be small, although some are quite sizable.

So the argument comes down to the significance of small differences. We believe they can be very significant if the field in which they occur is very large or pervasive. For example, a one percent difference in a large market (as with Nielsen ratings or selling soap) amounts to many millions of dollars over a season, which is why advertisers pay television and continue to advertise. Small but pervasive differences can also be lethal and of epidemic proportions. When deaths from influenza rise from the average rate of 24 per hundred thousand to 26 per hundred thousand, an additional 5 thousand people are dead, nearly five times the total number of AIDS victims. A few degrees' drop in the average temperature will usher in an ice age, and a comparable rise will melt the ice caps; perhaps two feet rise in the ocean level may inundate two-thirds of human habitation. A few votes can decide a close election. So it's not always the size of the difference, but its direction, persistence, and pervasiveness that counts.

I am enclosing reprints of the articles in which we have attempted to explain and demonstrate our concepts and findings. Although you have cited some of these, it does not appear that they were read carefully enough. We would certainly appreciate whatever additional attention you can give to what we believe are benchmark studies resting on research conceptions and strategies very different from most others.

With best regards,

Sincerely yours,

George Gerbner
Professor of Communications
and Dean

GG:ab

Enclosures