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Dear Michael:

I was delighted to see the interview by Ramesh Closepet and Lai-si Tsui in the January 1992 issue of *Media Development* but appalled to read the introduction. By itself, the interview could be so useful that it may seem ungrateful to complain about any aspect of it. Nevertheless, I feel compelled to do so, perhaps even because it was such a friendly and flattering piece and from such a congenial place.

The introduction notes that our research methodology "has been severely criticized." It cites Doob and MacDonald (1970), Hirsch (1980) and Hughes (1980) to that effect. "...Therefore," it notes, "Gerbner's findings have often been dismissed as spurious... Yet, while Gerbner's research methodology may be open to question, one should not dismiss the validity of his theory, etc." Of course, the validity of a theory rests on the research on which it is based, which, in turn, depends on the methodology that, according to the interoduction, may not only be open to question (as everything should be) but may as well be dismissed.

Now I do not think I am thin-skinned in feeling hurt and undermined by this unfelicitous and basically ignorant introduction. There are several reasons for these feelings.

First, in the lengthy interview, I was never asked to comment on the methodological critiques. If I had been asked, I could have easily put them in a more complete and fair perspective. As it is, the extremely selective and biased introductory note without the opportunity to comment amounts to ambush journalism.

Secondly, the critics are cited but not the responses. Even the references make no mention of any of the published replies that followed the critiques and dealt with each of them in a fair and conclusive way, rebutting each of their points. For example, we pointed out that Hirsch did not avail himself of an up-to-date report of our results (already in press when his critique was published). The results showed that different subgroups (e.g. low and high income) were significantly cultivated but in opposite directions, thereby cancelling each other out when lumped together (which is what Hirsch and some others did). That convergence of subgroups in the "mainstream" was the

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important finding that lead to the theory of "mainstreaming" or homogenization, as noted in the interview. But the evidence for it is not cited. The only references are to the old flawed critiques.

Third, the critiques references date back to 1979 and 1980. Those critical articles referred to the first partial publication of our results. But not even that publication is cited. In fact, none of our "severely criticised" work is cited. Therefore, the reader could not locate the work itself, only the criticism!

Fourth, and most importantly, I was never asked to discuss or even to provide a bibliography of our work of the past 12 years. Most of the work on which the theories discussed in the interview are based was done during that time by independent investigators, as well as us, in several countries. Dozens of studies have been published, confirming and elaborating the initial findings. It is now one of the best established lines of research on long-term media effects (cultivation). To make reference only to the old, faulty, and fully discredited critiques is what I expect of opponents in the media. They often use an old hatchet job to make the research "controversial" and more easily dismissed. It hurts me to find it in *Media Development*.

A book published by Sage in 1990 on *Cultivation Analysis; New Directions in Media Effects Research* summarized recent work. It would have been easy to refer the reader to that (or any) of the many studies confirming and further developing the knowledge base on which the theories discussed in the interview are based. I was never asked to provide that information. I am enclosing a bibliography and the most recent study that brings the research up to date on the international front.

All this is not to denigrate or seem ungrateful for the many flattering notes in the interview. It is just to express a sense of frustration over the baseless and misleading comments in the introduction. They ignore one of the largest bodies of evidence in the history of communication research. If one's major work is ignored and one's method and reasoning is termed "spurious," how can the theories be valid? I am afraid the impression given muddies the waters and does more harm than good, at least among those who look for reason and evidence and not only congenial speculation.

Now that I have gotten this off my chest, let me close with expressions of friendship, peace, and best regards.

Sincerely yours,