

Mass Communications
Research Center

503 Bascom communication hall
Madison 53706

University of Wisconsin
School of Journalism and
Mass Communication

September 5, 1973

Dean George Gerbner
Annenberg School of Communication
University of Pennsylvania
Philadelphia, Pennsylvania

Dear George:


We're working on a project that will relate perceived reality to the general relationship between television and social behavior in children. After a few pilot tests, plus some of the experimental and survey data from the NIMH studies a few years ago, I'm convinced that learned perceptions of reality provide a key intervening variable in the overall process.

I notice in your Scientific American article that you and Larry Gross have been working on the relationship between your television content analyses and the cultivation of "popular notions about people and life." That sounds as though it is somewhat related to the concepts I'm pursuing. I would appreciate it if you could send me copies of your measures, procedures, preliminary results, etc. Please note anything that you consider incomplete, premature, or otherwise confidential.

Since yours is the one major continuing study in the field, it would make sense for us to design our work so that it coincides with your key measures where that is possible. I'm quite familiar with your content analysis procedures, of course; it is the "effects" measures that I'm wondering about now.

By the way, I assume you've seen the paper Peter Clarke and I did on the Ph.D. job market in journalism education. We don't seem to be in any danger of creating a surplus of unemployable doctorates. I suspect this is true of the entire communication field, although our data don't extend that far.

Sincerely,



Steven H. Chaffee,
Willard G. Bleyer Professor

SHC:mc

AIR MAIL September 13, 1973

Dr. Steven H. Chaffee
Willard G. Bleyer Professor
Mass Communications Research Center
University of Wisconsin
5018 Vilas Communication Hall
Madison, Wisconsin 53706

Dear Steve:

In response to your letter of September 5 I am sending you, enclosed, a copy of our grant renewal proposal, which also includes a progress report and findings as of last May. We expect to have a fuller report about the end of this year.

I agree that there seem to be important similarities in our respective approaches. I would be glad to exchange further information, ideas, visits, etc., anything you think might make our efforts more productive.

Yes, I read the Chafee-Clarke paper and it rang true as far as our own experiences in communications are concerned.

With best regards.

Sincerely yours,

George Gerbner,
Professor of Communications
and Dean

GG:kas
encl.

GG: Larry Gross

20, 1973
AIR MAIL

Dear Steve:

Thank you very much for your full and helpful commentary on the Buchli-Pearce article. As you will note, I have attached a copy of your letter (minus the first paragraph and the signature) to my comments.

Would you be willing to look at the revision?

Since you have such a good grasp of Journal aims, I hope that you will submit your work to us too. I am also looking for outstanding reviewers for significant books. A list of books available for review is enclosed for your perusal.

I look forward to hearing from you soon.

Sincerely yours,

George Garbner

GG:kas
encl.

mass communications
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5018 Vilas communication hall
madison 53706

*Sent
White?*

university of wisconsin
school of journalism and
mass communication

November 14, 1973

Dean George Gerbner
Annenberg School of Communication
University of Pennsylvania
Philadelphia, Pennsylvania 19120

Dear George:

Enclosed is the vita of Bob Pepper, a very promising faculty prospect in the areas of broadcast skills and mass communications research. Having worked with him on several projects, I can recommend him to you personally in case you have an opening for next year.

As you can see from the vita, Bob is a student in Communication Arts, rather than in our Journalism program. His research interests are mainly in the areas of cable and the broadcast industry more broadly. He is trying to put together a reasonably comprehensive accounting of the relationships among the economic basis, the work structure of the industry, and the decisions that are made about news and programming. This is a pretty promising area, to judge from some of the recent work being done by Herb Gans, Larry Lichty, and others. Bob has worked quite a bit with Lichty, and is strongly motivated toward combining professional teaching with his scholarly work. He handles data well, and is not afraid to pour himself into a big project and see it through to completion.

Such people are rare, and I would like to see him located at a university where these things are taken seriously. The contacts he can make via Speech departments don't lead to many promising academic environments, which is why I am sending this to you and a few others in research-centered programs.

Sincerely,



Steven H. Chaffee,
Willard G. Bleyer Professor

SHC:mc
enc.

November 28, 1973
AIR MAIL

File

Dr. Steven H. Chaffee
Willard G. Bleyer Professor
Mass Communications Research Center
University of Wisconsin
5018 Vilas Communication Hall
Madison, Wisconsin 53706

Dear Steve:

Thank you very much for writing me about Bob Fepper. His skills and interests seem promising and, even though we have some senior people in the same areas, I would like to pursue the matter.

Enclosed is a copy of our Bulletin. Please ask him to state his teaching and research interests in terms of our program. What courses would he feel qualified to teach? What types of research would he want to conduct and to supervise?

Also, I would appreciate receiving copies of his major publications.

May I ask you for another favor. Also enclosed is the copy of an article on "Listening Behavior in Coorientational States" by Virginia Buchli and W. Barnett Pearce. This was submitted for publication in the Journal of Communication. May I have your evaluation as to its significance, competence, and suitability for the Journal? I would appreciate receiving your evaluation as soon as you can conveniently send it to me.

With best regards.

Sincerely yours,

George Garbner

GG:kas
encl.

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university of wisconsin

school of journalism and
mass communication

December 12, 1973

Dr. George Gerbner
Annenberg School of Communications
University of Pennsylvania
Philadelphia, Pennsylvania 19174

Dear George:

In my opinion, the article "listening Behavior in Coorientational States" by Buchli and Pearce is suitable -- in general -- for publication in the Journal of Communication. It is a competently done study on an aspect of communication behavior that has not been examined before, to my knowledge.

If it is accepted, however, some revisions would be in order. Those mostly involve expansion of the article, in the interest of explaining it to those who are not already familiar with the research that preceded it, and even simply to give a full report of the study to those who do know the previous work and care about this addition to it.

The introduction is awfully terse, and suggests a one-sidedness to the literature on selectivity. They should at least note Atkin's POQ article, in which he re-examines data from the Elmira survey and a Chaffee-McLeod study, and adds a field experiment of his own -- and concludes that Sears and Freedman were too precipitous in their judgment that selective exposure is a dead hypothesis. Atkin ends up on the same side as Buchli and Pearce, but for quite different reasons. And his is not the only other voice in the debate, by any means; just an example.

The Stamm-Pearce model is clearly the basis for this study. But it is not very informative to the reader to jump directly from the alphabet soup of PAC and PDD to a hypothesis (at the bottom of page 3 of the manuscript). No logical basis or hypothesized process linkage is spelled out. Surely the authors have thought about the lines of thinking that might intercede between an expectation-confirmation process and an attentional (in this case, listening) behavior. They should share their thinking with us, especially since only one of their two hypotheses is confirmed.

The operational definition of "listening behavior" is in fact one of simply recall of material the subjects had been exposed to. Recall is an ancient and honored dependent variable in psychological research. It is a function of many factors, only one of which is attention (listening). Since their independent variables are based on measures, rather than manipulations, they cannot make a clearcut case for listening as the sole and sufficient condition for recall. (This is not to say that they do not have a plausible case.) Therefore, it would seem prudent to call their dependent variable what it is -- "recall" or "knowledge gain" or "information" or somesuch -- rather than labeling it "listening" when it is at best a distant indicator of that behavior inter alia.

December 12, 1973

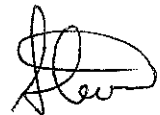
The results are grossly under-reported. A table of the means for the four conditions would seem a minimal amount of data, particularly since two of the four were hypothesized to differ from the other two in opposite directions. I would at least like to know if the mean for PAD was in the direction hypothesized, regardless of significance. Numbers please.

The significance tests for sex (which prove non-significant) constitute an alien intrusion in the "results" section. Aside from the single reference to a non-specified difference found by Weaver, the topic of sex differences gets no treatment in the paper. (There is a rather vast literature on the topic in general.) Finding no hypotheses, and little interest, here, one is scarcely surprised (or interested) in the finding that F 1.

There are some little problems too. Data are plural (p.5). Scheffe' should be referenced (p.5). And I suspect I am not the only reader who will be put off by the idiom of superlative negatives such as "most poorly understood" (p.1) and "listened significantly more poorly" (p.5).

My overall reaction is that the authors have run a single experiment and rather hurriedly cranked out a single article, for purposes of publication more than education. There is something to be learned here, and it fits in a larger intellectual context and empirical literature. That is why I recommend publishing it. But they seem to be treating it more as a research note, on the order of the Journal of Experimental Psychology. Our field is not one in which dozens of avid scientists are working in narrow traditions in which they keep up with all the current studies. We have a small number of active empiricists and a much larger audience of appreciative consumers of research. For the Journal of Communication it seems appropriate that we address the latter even more than the former. Accordingly, I would invite the authors to flesh out their paper with a good deal more context and at least a little more detail on the data.

Sincerely,



Steven H. Chaffee,
Willard G. Bleyer Professor

SHC:mc

June 28, 1974
AIR MAIL

Dr. Steven H. Chaffge
Willard G. Bleyer Professor
University of Wisconsin
Mass Communications Research Center
5018 Vilas Communication Hall
Madison, WI. 53706

Dear Steve:

The Buchli and Pearce article which you were kind enough to review last December is in the summer issue of the Journal, coming out in July. I am sending you, enclosed, another piece this time by Nightingale and Pearce on "Coorientation and Interpersonal Communication Between Political Partisans: A Field Study."

I would be grateful if you would look at it and give me your opinion as to its suitability for the Journal.

With best regards,

Sincerely yours,

George Gerbner

GG:kas
encl.

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university of wisconsin
school of journalism and
mass communication

July 19, 1974

Professor George Gerbner
School of Communication
The Annenberg School of Communication
University of Pennsylvania
3620 Walnut Street C5
Philadelphia, Pennsylvania 19174

Dear George:

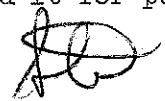
Regarding the Nightingale and Pearce article, "Coorientation and Interpersonal Communication between Political Partisans: A Field Study," I do not consider it worth publishing in the Journal of Communication.

It is an interesting idea, and there are some useful insights in the paper. But there is very little empirical value to the study itself. The most basic problem is the very small sample size, coupled with the rather indeterminate nature of the population from which the samples are drawn. The tie between the theoretical discussion of conflict and the data is less than clear. Partly this is due to indiscriminate mixing of different levels of analysis of the concept of "conflict", drawing on literature about inter-group, inter-personal, and intra-personal conflict without noting these conceptual distinctions.

Purportedly, the study is testing some Stamm-Pearce hypotheses in a field setting, with the aim of extending the generalizability of findings from the earlier studies in which various coorientational states were manipulated. Here, coorientational sequences are simply measured. The findings (with the exception of a single unhypothesized interaction of theoretical conditions with political party) turn out to be null. It is difficult to tell whether this is due to (a) a too-small sample, (b) poor fit between the coorientational hypotheses and the operational definitions, or (c) a lack of generalizability in the hypotheses themselves. Given the rather high probability that the null results are due to (a) and/or (b), it would be hard to justify acceptance of (c) as a conclusion.

A failure to replicate, which is what the authors conclude they have found, calls for much more confidence in the empirical procedures than I can work up in this case. One can hardly accuse the authors of bias in this regard, of course. Indeed, their interest would naturally be in successful replication of the findings from earlier, more controlled, studies. But this attempt at replication is too weak (starting with the inherent weakness of a small sample and uncontrolled field measures) to conclude that the hypotheses themselves have been seriously tested and found wanting. It is quite possible that the hypotheses will not replicate when tested in field situations, but this study is insufficient to reach that conclusion. The "failure" is of the study itself, not of the theoretical propositions; it is simply inadequate to direct us to any conclusions about the hypotheses, which is why I do not recommend it for publication.

Sincerely,



Steven H. Chaffee
Willard G. Bleyer Professor

SHC:mc

August 12, 1974

file

Dr. Steven H. Chaffee
Willard G. Bleyer Professor
School of Journalism and
Mass Communication
University of Wisconsin
Madison, WI. 53706

Dear Steve:

Thank you very much for your thoughtful critique of
July 19 of the Nightingale and Pearce article. I
have found your letter upon my return from a brief
vacation, hence the delayed reply.

I appreciate your continuing help and interest in
the Journal.

Sincerely yours,

George Gerbner

GG:kas