

**DECLINE AND FALL
AT THE WHITE HOUSE
A Rejoinder to Professor Davis**

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Davis has raised several objections to our research strategy which, he feels, has led us to largely null findings. While we wish to respond to the criticisms of the strategy in some detail, we find it important to argue first that his view of the findings simply misses too much.¹

It seems wrong to us to conclude that communication variables had no effect on voters' responses to the 1974 elections. Reading about the Senate hearings in newspapers and magazines and discussing them with friends produced several important consequences. To be sure, these consequences are quite often different for the two age groups studied. But we see

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no reason to be either disturbed or embarrassed by these conclusions as Davis seems to think we should be. Watergate may not have had the massive effects some thought it would. But our data suggest that Watergate did have an effect, and the media played a prominent role in determining some of it.

Our findings are null, by and large, where television and radio exposure is concerned. While we have acknowledged the possibility of various explanations for this finding, other than that of no relationship, and recognize the error of accepting the null position, we feel the critic's explanations merit serious challenge. It is his position, as we understand it, that our null findings may result from our strategy, which he judges to be too conservative and lacking in creativity.

We do not see, quite simply, our research as the direct lineal descendant of the early Columbia studies. Our program of research, we feel, has tried to reexamine (e.g., Blumler and McLeod, 1974; McLeod, Becker, and Byrnes, 1974; McLeod and Becker, 1974; McLeod and Brown, 1975) many of the conclusions attributed to the Columbia scholars. We see ourselves as proponents of neither the limited effects position nor the massive effects model. Rather, our research has attempted to seek a middle ground where the characteristics of audience members and the media content interact to produce meaningful consequences. In the case of our agenda-setting research, for example, we have argued that our findings suggest neither massive cognitive effects of the agenda-setting nature nor total lack of effects (McLeod, Becker, and Byrnes, 1974). Rather, the evidence indicates that the newspaper's agenda seemed to have consequences for particular types of people under some specified circumstances. We are disturbed by the fact that the complexities of those findings have tended to be ignored both by adherents of the universality of agenda-setting and by those who prefer to interpret them as null findings.

Davis argues that our strategy for examining effects is too conservative, in part, because we have sought to control out various prior influences. He is particularly concerned with the influences of social structure on media use. By controlling for

social structure, he argues, we might miss important media consequences. We do not accept his contention that we have controlled for such social-structural forces here; we also feel uncomfortable with his position that researchers should not do so.² If media effects are of any consequence, we feel, they must be strong enough to withstand *relevant* controls. In fact, in at least one case where we applied rather rigorous controls for social structure, the media withstood them (Blumler and McLeod, 1974). The critic may be too protective of the media variables.

Davis seems to have an underlying concern that communication researchers must justify their activities in terms of strong media effects. Such a concern, of course, could lead us to analysis stances which, while generally guaranteeing effects, would not withstand serious theoretical and methodological challenges. While we accept the critic's call for more creativity in looking for effects, we have opted to do so by increasing the sample frame of relevant independent and dependent variables. Our study here, for example has included gratification measures in an effort to expand the horizon of media variables and such variables as turnout and campaign activity to increase the range of possible effects. We reject the notion, however, that such creativity should lead us away from the methodological rigor we now have in the field. We opt to continue to borrow methodological tools from our parent disciplines while challenging their conceptual narrowness.

Davis has suggested a strategy, which he is exploring. From what he has told us here, it appears exciting on several counts. We are in complete agreement with his contention that the media may operate under certain contingencies which researchers need to explore fully. Our partition of the sample according to age was such an effort, and one which we feel was rewarding. The young showed a clearly different pattern of effects than the older voters. Our efforts along this line have been hampered by small sample sizes, but we recognize the merits of the approach. We do not find it conflicting with what we have done here.

We think Davis has ignored an important element of our research. Our perspective in this particular problem has been shaped to a considerable degree by the popular notion that Watergate was having massive and perhaps unprecedented effects on the political system. The media were assumed to play a crucial role in this transformation. We began with the desire to test this popular position, and our strategy and findings must be considered in this light. We think our data show no strong case for *massive* effects. But we believe our results do indicate that the media did play some role in determining reactions to the scandals. We would like to think that careful readers will agree that our conclusions have added both to an understanding of what happened during the Watergate period and to the body of theory in our field.

NOTES

1. While our comments here reflect our continuing disagreement with Davis, we would like to acknowledge the helpful suggestions he made regarding an earlier version of this paper. We think this manuscript is a stronger one because of those comments.

2. We controlled for only three types of variables here: prior levels of the dependent variable in order to study change rather than level, party affiliation so as to control for the greater Watergate communication avidity of Nixon-haters, and usual patterns of communication so that we could evaluate the specific increment of Watergate attention. Only the first of these proved to be important in accounting for large proportions of variance. None of these controls are enduring social-structural properties such as social class. The topic of the appropriateness of controls under various situations is too complex to discuss completely here. However, we do not argue that social-structural controls are relevant in all cases, but in many cases they would be. Education would be an obvious control for spuriousness, for example, if we found a zero-order relationship between media exposure and the kinds of knowledge that might have been acquired previously in the classroom. Similarly, we might control for education or other measures of stratification if we found a connection between communication behavior and criteria depending upon self-report measures prone to produce socially acceptable responses among the more sensitive, "well-educated," middle-class respondents (e.g., racial attitude measures).

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