

# STUDIES OF THE EFFECTS OF EVIDENCE IN PERSUASIVE COMMUNICATION

James C. McCroskey

Speech Communication Research Laboratory  
Department of Speech  
SCRL 4-67  
December, 1967



Michigan State University  
East Lansing, Michigan 48823

STUDIES OF THE EFFECTS OF EVIDENCE  
IN PERSUASIVE COMMUNICATION

James C. McCroskey

December, 1967

SCRL 4-67

Speech Communication Research Laboratory  
Department of Speech  
Michigan State University  
East Lansing, Michigan

## FORWARD

The report of research which follows is one of a series sponsored by the Speech Communication Research Laboratory of Michigan State University. This Laboratory was created within the Department of Speech and Theatre for the purposes of conducting research and stimulating and facilitating the distribution of research evolved by the faculty and the students of the department.

The definition of research used within the Laboratory places an emphasis on creative effort as well as rigor and information. To put this point more succinctly, we believe that good research is the product of a creative mind using the methods of a discipline as tools to aid the creative process.

The Department of Speech and Theatre and several other units within Michigan State University make a financial contribution to the Speech Communication Research Laboratory. Such support is, of course, encouraging. In a real sense it is inspiring. We believe that our response to this support is best represented by the quality of the research which follows.

William B. Lashbrook, Director  
Speech Communication Research Laboratory  
Michigan State University

## PREFACE

Traditional theories of rhetoric and argumentation hold that the proper use of evidence is a vital aspect of effective persuasive communication. Contemporary textbooks, almost without exception, extol the virtues of proper evidence usage and provide the reader guidance in using evidence effectively.

A series of experimental studies reported between 1953 and 1963 cast serious doubt on the practical importance of evidence as a factor in persuasion. The findings of these studies suggested that evidence may have little effect in most cases. These findings have led some writers to discount evidence as an important factor in persuasion.

As a person schooled in the traditional theories of rhetoric and argumentation I found the results of these studies disturbing. On the surface they seemed to disprove much that I had accepted as "truth". Since some of these studies had found an effect on attitude change attributable to evidence, I retained some faith in the traditional theories. However, I recognized that the results of the other studies had to be explained. I suspected that some factor or factors not considered by these researchers had led to their conflicting results.

My purpose in conducting the series of studies discussed in this report, therefore, was threefold: To discover the factor or factors which could have produced the conflicting results of earlier studies; to determine as specifically as possible within the limitations of the designs of my studies what role evidence does play in persuasive communication; and to reconcile, if necessary, the results of the experimental studies and the traditional theory of the importance of evidence in persuasion.

No experimental researcher can operate completely independently. He must have the advice and cooperation of others. I am most indebted to Robert E. Dunham whose advice and criticism during the early stages of this project prevented me from making many errors in the design of the research and helped me to discover those I didn't avoid. My appreciation is extended to Carroll C. Arnold and Richard B. Gregg for their assistance in reconciling the results of my early studies with traditional theory. I also wish to acknowledge the work of William E. Arnold, Jerry W. Koehler, and John R. Wenburg who collaborated with me in some of the studies reported in this paper. Their cooperation is specifically noted in the reports of the appropriate studies.

Finally, a word of appreciation to the several thousand undergraduate students at Pennsylvania State University and Michigan State University who served as experimental subjects for this series of studies. Their cooperation, though not fully voluntary, is nonetheless deeply appreciated.



## TABLE OF CONTENTS

FORWARD	<u>Page</u> ii
PREFACE	iii
LIST OF TABLES	vii
SECTIONS	
I. INTRODUCTION	1
Review of Previous Studies	1
The Experimental Hypotheses	3
Definitions	5
The Studies to be Reported	5
II. PILOT STUDIES	6
Study I	6
Study II	8
Study III	8
Conclusions	10
III. STUDIES OF ETHOS AND EVIDENCE	11
Study IV	11
Study V	14
Study VI	16
Conclusions	17
IV. STUDIES OF THE CREDIBILITY OF TYPES OF EVIDENCE	20
Study VII	21
Study VIII	24
Study IX	27
Study X	31
Conclusions	33
V. STUDIES OF EVIDENCE AND DELIVERY	35
Study XI	35
Study XII	38
Conclusions	47
VI. A STUDY OF EVIDENCE, ETHOS, AND PRIOR KNOWLEDGE OF AUDIENCE	49

Study XIII	49
Conclusions	54
VII. THE ROLE OF EVIDENCE IN PERSUASIVE COMMUNICATION	55
A Model of a Persuasive Unit	55
The Types of Data	58
The Theory Versus the Research	59
The Need for Future Research	61
REFERENCES	63

#### LIST OF TABLES

	<u>Page</u>
Table 1: Results of Study I	7
Table 2: Results of Study III	9
Table 3: Results of Study IV	13
Table 4: Results of Study V	15
Table 5: Results of Study VI	18
Table 6: Results of Study VII	23
Table 7: Results of Study VIII	26
Table 8: Results of Study IX	30
Table 9: Results of Study X	33
Table 10: Results of Study XI	37
Table 11: Main Effect Results of Study XII Not Affected by Interactions	43
Table 12: Results of Study XII--Individual Cell Means	44
Table 13: Results of Study XIII--Primary Dependent Variables	52
Table 14: Results of Study XIII--Speech Evaluation Dependent Variables	53

character scales. All Ss in both groups also completed a six-item speech evaluation questionnaire for each speech and a personal data form concerning age, term in school, and cumulative grade point average. A control group (N = 69) completed the attitude scales four weeks before the experiment and again 24 hours before the experiment during regular class sessions.

Results. Posttest measures of authoritativeness, character, and attitude on both topics were subjected to analysis of covariance. The covariate in each case was pretest attitude on the appropriate topic. The analyses of covariance and subsequent *t*-tests indicated that the inclusion of evidence significantly increased perceived authoritativeness and attitude change on both topics but had no significant effect on perceived character on either topic. A clear trend on both topics, however, favored the evidence versions. Table 1 reports the mean posttest authoritativeness and character scores for each topic and the mean attitude shift for each topic.

Table 1  
Results of Study I

Dependent Variable	Evidence	No Evidence	$\bar{D}$
Perceived (posttest) Authoritativeness*			
Capital Punishment	53.95	57.90	3.95***
Federal Control of Education	57.53	63.57	6.04***
Perceived (posttest) Character**			
Capital Punishment	49.92	53.42	3.50
Federal Control of Education	51.82	54.57	2.75
Attitude Shift			
Capital Punishment	4.38	1.48	2.70***
Federal Control of Education	11.10	8.10	3.00***

\*The lower the score, the higher the perceived authoritativeness. The hypothetical neutral point on the scale is 66.0.

\*\*The lower the score, the higher the perceived character. The hypothetical neutral point on the scale is 60.0.

\*\*\*Statistically significant at at least the .05 level.

## Study II

Procedures. The procedures for this study were the same as for the previous study with the following exceptions: 1) only the capital punishment speeches were included; 2) the Ss were students participating in the Summer High School Speech Institute at The Pennsylvania State University; 3) there was no pretest attitude measurement; 4) all Ss completed both the ethos and the attitude posttest measures; and 5) two experimenters were present, the director of the institute and one of the teachers in the institute (the writer).

Results. Analysis of variance of the posttest measures of authoritativeness, character, and attitude indicated that the inclusion of evidence had no significant effect on any of the dependent variables.

It is important to note that the mean scores on authoritativeness and character in this study indicated that the Ss perceived the experimental speaker to be of significantly higher ethos than he was perceived to be by the Ss in the first study. In addition the Ss mean posttest attitudes in this study were significantly more opposed to capital punishment than the subjects in the first study. These observed differences between the two studies led to the speculation that the ethos of the experimenters in this study affected the perceived ethos of the unknown, unidentified, tape-recorded speaker (McCroskey & Dunham, 1966).

## Study III

Study III was designed to investigate the possibility that an experimenter's ethos can affect the perceived ethos of an unknown, unidentified, tape-recorded speaker's ethos (Holtzman, 1966). Our interest in the study here stems from its implications for the design of the evidence studies to be reported in later chapters.

Procedure. The two tape-recorded versions of the federal control of education speech (see Study I) were each presented to two audiences composed of 25 students enrolled in the basic course in speech at Pennsylvania State University. Each version of the speech was presented in an "unfamiliar sponsor" condition and an "instructor sponsor" condition. A pretest attitude measure of the semantic differential type (McCroskey, 1966b) was administered two weeks prior to the experiment. In the "unfamiliar sponsor" conditions the experiment was conducted at evening sessions, in classrooms other than the Ss regular speech classrooms, by casually dressed, unidentified graduate students.

The "instructor sponsor" conditions were administered in the regular speech classroom, during a normal class, by the Ss' regular instructor. Subsequent to exposure to the experimental speech the Ss completed posttest measures of attitude and ethos of the semantic differential type (McCroskey, 1966a; McCroskey, 1966b).

Results. Analysis of covariance of the posttest measures, employing pretest attitude as the covariate, and subsequent t-tests indicated that inclusion of evidence increased perceived authoritativeness and character of the experimental speaker and attitude change in the "unfamiliar sponsor" condition but had no effect in the "instructor sponsor" condition. (The Holtzman results are reproduced in Table 2.) These results led Holtzman (1966) to conclude that an experimenter's ethos can affect the perceived ethos of an unknown, unidentified, tape-recorded speaker.

Table 2  
Results of Study III

Dependent Variable	Evidence	No Evidence	$\bar{D}$
Perceived (posttest) Authoritativeness			
Unfamiliar Sponsor	17.8	19.8	2.0**
Instructor Sponsor	16.9	17.0	.1
Perceived (posttest) Character*			
Unfamiliar Sponsor	19.3	22.5	3.2***
Instructor Sponsor	20.1	20.3	.2
Covariance Adjusted Posttest Attitude*			
Unfamiliar Sponsor	16.9	20.1	3.2***
Instructor Sponsor	16.3	16.2	-.1

\*The lower the score, the more favorable the ethos or attitude. The hypothetical neutral point on the scale is 24.0

\*\*Statistically significant at the .10 level.

\*\*\*Statistically significant at at least the .05 level.



### Conclusions

It seems clear from the results of these studies that ethos induced unintentionally by known and respected experimenters interacted with the evidence treatments to produce inconsistent results. Two of our theoretical hypotheses set forth in Section I, however, are supported by these results. If we presume that the experimental speakers in Study I and in the "unfamiliar sponsor" conditions of Study III were initially perceived by the experimental Ss to be moderate to low-ethos sources while the experimental speakers in Study II and in the "instructor sponsor" conditions of Study III were initially perceived by the experimental Ss to be comparatively high-ethos sources, our first hypothesis is supported--the inclusion of evidence increases the amount of immediate attitude change produced by a message attributed to a low-ethos source but has no effect on immediate attitude change when the message is attributed to a high-ethos source. Since, however, these ethos levels were unintentionally induced, the test of our hypothesis lacks the rigor we should demand before we conclude that the hypothesis is correct. The studies reported in the following section sought to achieve more rigorous tests.

Our third theoretical hypothesis also received some support from the results of these three studies. (The second theoretical hypothesis dealt with long-term effects of evidence.) Evidence was found to improve perceived ethos in Study I and in the "unfamiliar sponsor" condition of Study III. The fact that evidence did not have any observed effect on ethos in Study II or in the "instructor sponsor" condition of Study III, however, suggests that our hypothesis that including evidence in a message increases perceived ethos of a communicator may not be correct when the communicator is initially perceived as a moderately high-ethos source. Again, however, since the initial ethos levels of the speakers in these studies can only be inferred, we need a more rigorous test of this hypothesis before we decide to accept, reject, or modify it. The studies reported in the following section provide a better basis for this decision.



### III

#### STUDIES OF ETHOS AND EVIDENCE

The studies discussed in the preceding section provided information relevant to two of our three theoretical hypotheses. The three studies to be discussed in this section, as a group, provide information relevant to all three hypotheses. Initial ethos and evidence usage was manipulated in each study. In all three studies immediate attitude change was measured. Terminal ethos was measured in studies 4 and 6. Delayed attitude change was measured in studies 4 and 5.

Studies 4 and 5 are reported in detail elsewhere (McCroskey, 1966a). Although in each of these studies three levels of ethos were induced, only the high and low-ethos conditions will be discussed here. The effect of evidence in the middle-ethos condition in these studies was generally consistent with that in the low-ethos condition.

#### Study IV

Procedure. The "evidence" and "no evidence" versions of the two speeches developed for Study I were each presented to two audiences composed of sixty-eight males and thirty-nine females randomly assigned from forty-two sections of students enrolled in the basic speech course at The Pennsylvania State University. Each version was presented to one audience following an introduction designed to induce high initial ethos and to another audience following a low-ethos introduction. The ethos introductions had been pretested and found effective in inducing the desired perceived ethos levels. Each audience heard an "evidence" speech on one topic and a "no evidence" speech on the other. Pretests of attitude on the two topics were administered during regular class periods two weeks prior to the experiment.

The experiment was conducted at evening sessions in a large lecture hall. Ss were told that they were to hear two speeches recorded for broadcast on a proposed new network radio program called "The Citizen Speaks." The reason they were selected to hear the speeches was purported to be because the proposed program was to be directed to young adults. The Ss were told the scales they were asked to complete were designed to indicate the reaction young adults would have to this type of program. The introductions of the speakers were preceded by comments indicating that these would not be the introductions broadcast by the network, but "we feel that you are entitled to know the background of the speaker."

Immediately after each speech the Ss completed semantic differential measures for attitude on the topic and the two dimensions of ethos. (Ss also completed Likert-type measures for these variables, but only the semantic differential results will be presented here. Likert results were comparable, but the later studies employed only the semantic differentials. Thus, comparisons between studies can be made best by looking at the semantic differential results.) Four weeks after the experiment the Ss completed Likert attitude scales on the two experimental topics. Three weeks later (seven weeks after the experiment) the Ss again completed the semantic differential attitude measures.

Results. Each of the dependent variables was subjected to analysis of covariance with the pretest attitude score on the appropriate topic serving as the covariate. The analyses of covariance and subsequent t-tests indicated that the effects of evidence were not consistent across topics.

The results obtained on the capital punishment topic indicated that inclusion of evidence had no significant effect on immediate attitude change, perceived authoritativeness, perceived character, or attitude after seven weeks. The only dependent variable that appeared that it may have been affected by inclusion of evidence (across ethos levels) was attitude four weeks after the experiment. A difference in favor of evidence was significant at the .01 level.

Results on the federal control of education topic provided support for all three of our theoretical hypotheses. Inclusion of evidence increased immediate attitude change, perceived authoritativeness, and perceived character in the low-ethos condition. There was no significant effect favoring evidence on these variables in the high ethos condition. On the delayed measures of attitude, inclusion of evidence (across ethos levels) produced significantly more favorable attitudes toward federal control of education. (Table 3 reports the covariance adjusted mean perceived authoritativeness and character scores and immediate and delayed posttest attitude scores on both topics.)

The major differences in the effects of evidence on these two topics were unexpected and difficult to explain in the context of the research design employed. Such differences could be produced by the nature of the topics employed, by an audience variable relating to evidence usage not taken into account in the theory leading to our hypotheses, or by one or more factors in the experimental design. In order to reduce the possible explanations for these conflicting results, Study V was conducted. Two factors were suspected as possibly confounding elements in the present study--high emotionality in the capital punishment speeches and excessive measurement of dependent variables. Both of these elements were eliminated in Study V.

Table 3  
Results of Study IV

Dependent Variable	Evidence	No Evidence	$\bar{D}$
Perceived (posttest) Authoritativeness			
Capital Punishment			
High Ethos	16.10	15.66	-.44
Low Ethos	24.42	23.88	-.54
Federal Control of Education			
High Ethos	16.98	17.15	.17
Low Ethos	21.15	25.46	4.31**
Perceived (posttest) Character*			
Capital Punishment			
High Ethos	10.77	12.09	1.32
Low Ethos	18.37	18.33	-.04
Federal Control of Education			
High Ethos	11.76	12.66	.90
Low Ethos	16.93	25.74	8.81**
Covariance Adjusted Immediate Posttest Attitude*			
Capital Punishment			
High Ethos	18.73	19.18	.45
Low Ethos	20.97	21.54	.57
Federal Control of Education			
High Ethos	18.06	17.53	-.53
Low Ethos	16.75	20.07	3.32**
Delayed Posttest Attitude (7 week) (Across Ethos Levels)			
Capital Punishment	22.21	22.38	.17
Federal Control of Education	19.36	20.62	1.26**

\*The lower the score, the more favorable the ethos or attitude.

The hypothetical neutral point on the scale is 24.0.

\*\*Statistically significant at at least the .05 level.

### Study V

Procedure. The procedure employed in this study was identical to that followed in Study IV with the following exceptions. There were forty male and forty female Ss in each cell. Only the semantic differential attitude measures were administered. The two versions of the capital punishment speech were revised to reduce the degree of emotionality of the messages. (For copies of the original and revised speeches see McCroskey, 1966a.) Four weeks after the experiment all subjects again completed the semantic differential attitude measures for the two topics during regular class periods.

Results. The immediate and delayed posttest attitude scores on each topic were subjected to analysis of covariance. The pretest attitude score on the appropriate topic served as the covariate. The analyses of covariance and subsequent *t*-tests indicated that the immediate posttest attitude results were not consistent across topics, but the delayed posttest attitude results were consistent across the two topics.

The results obtained on the capital punishment topic indicated that inclusion of evidence had no significant effect on immediate attitude change. Results on the federal control of education topic, however, indicated that inclusion of evidence increased immediate attitude change in the low-ethos condition. There was no significant effect favoring evidence in the high ethos condition. The results of the delayed posttest attitude measure on both topics indicated that inclusion of evidence (across ethos levels) increased attitude change retention over time. (Table 4 presents the results of Study V.)

The results of this study almost perfectly replicate those obtained in Study IV. In both studies inclusion of evidence had no effect on immediate attitude change on either ethos level on the capital punishment topic or on the high-ethos level on the federal control of education topic. In both studies inclusion of evidence increased immediate attitude change on the low-ethos level on the federal control of education topic. In both studies inclusion of evidence improved attitude change retention over time (across ethos levels) on the federal control of education topic. A similar result was obtained on the capital punishment topic in the present study and a trend in this direction was observed in Study IV. In short, the changes in procedure and stimulus materials in this study did not produce results which help us to explain the conflicting results between the two topics, except to indicate that the two suspect procedures did not contaminate our results.



Table 4

Results of Study V

Dependent Variable	Evidence	No Evidence	$\bar{D}$
Covariance Adjusted Immediate Posttest Attitude*			
Capital Punishment			
High Ethos	14.46	15.97	1.51
Low Ethos	17.40	17.83	.43
Federal Control of Education			
High Ethos	15.39	16.26	.87
Low Ethos	18.01	20.04	2.03**
Covariance Adjusted Delayed Posttest Attitude* (Across ethos levels)			
Capital Punishment	18.35	19.59	1.24**
Federal Control of Education	18.96	20.51	1.55**

\*The lower the score, the more favorable the attitude. The hypothetical neutral point on the scale is 24.0.

\*\*Statistically significant at at least the .05 level.

### Study VI<sup>1</sup>

This study was designed to provide more data relevant to our theoretical hypothesis concerning the effect of inclusion of evidence on perceived ethos and to test whether the relationship between evidence and ethos in producing attitude change on the federal control of education topic could be generalized beyond the particular ethos inductions employed in studies IV and V.

Procedure. The two versions of the federal control of education speeches were presented under high and low-ethos conditions to audiences composed of students enrolled in the basic speech course at The Pennsylvania State University. The speaker was introduced in the high-ethos condition as Dr. William Anderson, professor of political science, holder of a Ph.D degree from Northwestern University, and former member of the American Communist Party. It was said that he recently renounced his membership and condemned the party. In the low-ethos condition the speaker received a similar introduction except that he was identified as a present member of the American Communist Party. Treatments were administered at the beginning of regular class sessions during the first week of the course. The experimenter and the classroom teacher were both present in all cases.

All Ss were led to believe that they were being exposed to the experimental speech so that the instructor could allude to it during the day's lecture. The experimenter was referred to as "a fellow I asked to bring over a tape recorder and a tape from our departmental library." A dialogue between the experimenter and the instructor was designed to give the Ss the impression that the tape had been selected at random from a group of "new tapes" the department had just received from the Mutual Broadcasting Company. Supposedly neither the experimenter nor the instructor had previously heard the tape. The introduction of the speaker was presented in response to the instructor's query concerning whose speech the class was to hear. The experimenter responded by shuffling through papers in a notebook and then reading the appropriate introduction. Post-experiment discussions with the classes indicated that no S perceived that he was participating in an experiment and that all Ss believed the speaker on the tape was the person introduced.

Each experimental condition was administered to two classes. Data from the classes were combined for purpose of analysis, but the classes received the experimental treatments separately. After random exclusion of Ss to equalize "n" there were 36 Ss in each cell.

---

1. This study was conducted by the writer in cooperation with Mr. Jerry W. Koehler of The Pennsylvania State University.



The semantic differential attitude measure on the federal control of education topic was administered to the Ss along with several other measures two days before the experiment. Immediately after exposure to the experimental treatment the Ss completed the attitude measure again as well as the semantic differential ethos measures.

Results. Each of the dependent variables was subjected to analysis of covariance with the pretest attitude score serving as the covariate. The analyses of covariance and subsequent *t*-tests indicated that the inclusion of evidence had no significant effect on immediate attitude change, perceived authoritativeness, or perceived character in the high-ethos condition. In the low-ethos condition the inclusion of evidence was found to significantly increase immediate attitude change, perceived authoritativeness, and perceived character. (Table 5 presents the results of Study VI.)

### Conclusions

The results of these three studies, like the results of the three studies discussed in the previous section, do not permit us to draw firm conclusions with regard to our first two theoretical hypotheses. The "consistently inconsistent" results between topics indicate that the theory underlying our first two hypotheses is inadequate to explain the functioning of evidence in persuasive communication. It is obvious from the results of these six studies that evidence can have an effect on immediate attitude change and ethos in some cases, but that it will not always have a significant impact. It does seem, however, that evidence has a positive effect on attitude change retention. Just why such an effect can occur when no effect is observed on immediate attitude change is not clear.

The major question is why the results on the two topics were so markedly different. Intensive interviews with some of the Ss who participated in Studies IV and V (who were subsequently excluded from the study because of the possible effects of the interviews on delayed posttest attitude) shed some light on this question. The almost universal reaction from these Ss was that the evidence cited in the capital punishment speech was "old hat." Almost every S interviewed indicated that he was already familiar with the evidence included or with similar evidence. On the federal control of education topic, on the other hand, the most frequent comment was one of interest and surprise at what was described by several Ss as the "shocking facts" presented.

If we presume that evidence must be "new" to have an effect, our results become quite explainable. Such a presumption is highly consistent with some information theories. It is also consistent with dissonance theory. Old evidence has already entered the cognitive domain of the S. If it

Table 5  
Results of Study VI

Dependent Variable	Evidence	No Evidence	$\bar{D}$
Perceived (posttest) Authoritativeness*			
High Ethos	20.15	19.04	-1.11
Low Ethos	20.64	25.20	4.65**
Perceived (posttest) Character*			
High Ethos	15.05	14.11	- .94
Low Ethos	14.53	19.09	4.56**
Covariance Adjusted Immediate Posttest Attitude*			
High Ethos	17.66	16.90	- .76
Low Ethos	18.49	22.39	3.70**

\*The lower the score, the more favorable the attitude or ethos. The hypothetical neutral point on the scale is 24.0.

\*\*Statistically significant at at least the .05 level.

created dissonance, that dissonance would have already been resolved and defense mechanisms constructed to avoid the reoccurrence of dissonance as a result of that evidence. Thus the presentation of that evidence to the S would have no effect. A later study was designed to specifically test this theory.

During these interviews it also became apparent that some of the evidence judged to be "excellent" by the writer and those with whom he consulted was not perceived in the same light by some Ss. There seems to be two ways to overcome this problem in studies examining the effects of evidence in persuasive communication. One way is to determine from the Ss in advance who they think are qualified, trustworthy sources and to employ evidence from these people in experimental speeches. This procedure was employed by Bettinghaus (1953). While this is excellent experimental design, it does not provide results that are of much value to practicing communicators. We are not ordinarily able to employ such procedures in the "real world." The other way of overcoming this problem is to develop empirically based generalizations as to what people in general actually perceive to be good evidence. This procedure is represented by the four studies reported in the following section.

Another question that arose as a result of the interviews with the Ss was the effect that delivery might have on the impact of evidence in persuasive communication. The delivery of the federal control of education speeches was universally praised by the Ss. The delivery of the capital punishment speeches, however, received a mixed response. Even though the individual who recorded these speeches was a highly skilled and experienced speaker, several of the interviewed Ss indicated that they thought the delivery was "monotonous." We may speculate, therefore, that good delivery permits evidence to have an impact while poor delivery inhibits its impact. The effect of delivery on the impact of evidence is examined by the two studies reported in Section V.

In sum, at this point in the series of studies of the effect of evidence usage in persuasion more questions were raised than answered. Most of these questions were resolved, subject to replication, in later studies and on the basis of the complete series of studies a relatively comprehensive theory of the role of evidence in persuasive communication was developed. This theory is set forth in the final section of this report.



IV

STUDIES OF THE CREDIBILITY OF TYPES OF EVIDENCE

In the preceding studies the attempt was made to employ "evidence" versions of the experimental speeches which represented excellent use of evidence. The normal textbook tests of evidence were applied to each item of evidence considered for inclusion. Only those which in the opinion of the writer and other professional speech educators met high standards of evidence quality were included. The presumption was that if evidence of high quality could not produce effects on attitude change and/or ethos, evidence of lesser quality could not. This high quality evidence, however, did not produce immediate effects on the capital punishment topic. Thus, the question arises as to whether the evidence included really was of high quality.

A review of the literature on use of evidence in persuasive speaking indicated that authority-based testimony could be placed into three categories--biased, unbiased, and reluctant. None of the evidence included in the experimental speeches was in the first category--biased. Most of the evidence was unbiased with a few pieces which could be categorized as reluctant.

Reluctant testimony is considered excellent evidence by most writers and some recommend it above all other types of testimony. For example, Bettinghaus (1966, p. 48) says

Most people are inclined to place considerable weight on evidence coming from a reluctant witness, although it may be no more true than evidence that is not reluctant. But the credibility of the witness seems to most of us to be better established when he is testifying against what seem to be his best interests.

This theoretical superiority of reluctant testimony calls into question the assumption that the evidence in the experimental speeches in the preceding studies was of the highest quality. Most of the evidence was unbiased rather than reluctant. The following studies were designed to test the theory that reluctant testimony is superior to other types of authority based testimony.

Theoretically, the most important determinant of the effectiveness of a piece of evidence is the credibility of the source of that evidence. To be useful as evidence in argument evidence should come from a source that is highly credible in terms of his authoritativeness on the topic and his

trustworthiness in the given case. If he is reluctant to give the testimony, so much the better.

There is one problem, however, which is not apparent from a cursory examination of this theory. If the audience shares the quoted source's bias, we should not expect the fact that the source is biased on the point in question to affect adversely the impact of his testimony. In addition, if the source is quoted as expressing a view contrary to what would be expected to be his best interests (reluctant testimony) and his audience shares those interests (holds the same bias as the quoted source), we should not expect an improved effect for his testimony. The effects of bias or reluctance, therefore, should be expected to vary with the bias of the audience. With a randomly selected audience, of course, we would expect a considerable range of bias on a given question. Thus the lack of bias or presence of reluctance on the part of a quoted source should increase the impact of a given piece of testimony as evidence with some audience members even though these factors would have no effect on other audiences members.

There has been no experimental research reported in the literature pertinent to the question of what type of authoritative testimony has the greatest impact as evidence in persuasion. No studies have compared the effectiveness of biased versus reluctant testimony or of reluctant versus unbiased testimony with any audience. However, considerable research has been conducted concerning the force of credibility or ethos in persuasion. The results of these studies indicate that, in general, the higher the credibility of a source, the greater the persuasive effect of the source's message (Andersen & Clevenger, 1963).

Since quoting an authority as evidence is a direct attempt to use the authority's ethos to increase persuasion, we may conclude on the basis of the credibility research that the authority-based evidence which is best is that which is considered by the audience as emanating from the most credible source. Thus, the question posed for the studies reported in this section was, "what source of authority-based evidence is considered the most credible; the biased source; the reluctant source; or the unbiased source? "

### Study VII<sup>2</sup>

Procedure. Two opinion statements of approximately fifty words in length were constructed, one statement pro-labor in orientation, the other anti-labor. Each stated a definite position as to whether labor unions make a significant contribution to inflation. In order to assure that the opinion statements were correctly labeled as pro- or anti-labor, students in two

---

<sup>2</sup>Studies VII and VIII were conducted by the writer in cooperation with Dr. William E. Arnold of the University of Connecticut. They are reported in more detail elsewhere (Arnold & McCroskey, 1967).

sections of Speech 200 at the Pennsylvania State University were asked to read the statements and label them as either "pro-labor" or "anti-labor." All but two of the thirty-eight Ss indicated that the statements were interpreted as intended.

Introductions of three sources were developed. One introduction was for a labor leader, a second was for a management leader, and a third was for a professor of economics. Each introduction was then attached to each of the opinion statements. This provided six different conditions for the experiment.

The experimental Ss were each exposed to one of the message-introduction combinations and asked to complete Likert-type measures of source credibility (McCroskey, 1966b). Separate measures of perceived authoritativeness and perceived character were completed by each subject.

The null hypotheses were that there would be no differences in perceived authoritativeness or character among the biased, reluctant, and unbiased sources. To test these hypotheses the credibility scores of the labor leader--pro-labor treatment and the management leader--anti-labor treatment were combined to represent the "biased source" condition; the labor leader--anti-labor treatment and the management leader--pro-labor treatment were combined to represent the "reluctant source" condition. This procedure provided for reduction of the possibility that audience bias on the labor question would influence the results of the study. While random audience bias was assumed, it was felt wise to reduce the potential effects of such bias as much as possible within the limitations of the design of the study. The professor of economics--pro-labor condition and the professor of economics--anti-labor condition were combined to represent the "unbiased source" condition.

Six sections of students enrolled in the basic speech course at Pennsylvania State University were selected to serve as Ss in the study. Each section received a different introduction-message treatment. The course instructor read both the source introduction and the opinion statement to the Ss. After hearing the experimental stimuli the Ss completed the source credibility measures.

Statistical tests of the hypotheses were provided by subjecting the credibility scores to analysis of variance and computing t-tests of the differences between mean scores of the three conditions.

Results. Table 6 reports the obtained mean authoritativeness and character scores for the three experimental conditions, the difference in mean scores between conditions, and the results of the t-tests of those differences. The hypotheses of no difference in perceived credibility between



Table 6  
Results of Study VII

Credibility Dimension	Source Condition		
	Biased Source (1)	Reluctant Source (2)	Source (3)
(A) Authoritativeness*	59.65	50.48	48.52
(B) Character**	61.56	54.93	43.56

Hypothesis Tests

H:  $A_1 = A_2$ ;  $\bar{D} = 9.17$ ,  $t = 3.77^{***}$

H:  $B_1 = B_2$ ;  $\bar{D} = 6.63$ ;  $t = 2.08^{***}$

H:  $A_2 = A_3$ ;  $\bar{D} = 1.96$ ;  $t = .77$

H:  $B_2 = B_3$ ;  $\bar{D} = 11.37$ ;  $t = 3.68^{***}$

H:  $A_1 = A_3$ ;  $\bar{D} = 11.13$ ;  $t = 3.52^{***}$

H:  $B_1 = B_3$ ;  $\bar{D} = 18.00$ ;  $t = 8.42^{***}$

\*The hypothetical neutral point on this measure is 66.0. Scores can range from 22 to 110. The lower the score the higher the perceived authoritativeness.

\*\*The hypothetical neutral point on this measure is 60.0. Scores can range from 20 to 100. The lower the score the higher the perceived character.

\*\*\*Statistically significant at at least the .05 level.

of Speech 200 students to the sources of these materials." The Ss were asked to read the material about the source and his statement and then complete two six-scale semantic differentials. These semantic differentials were measures of perceived source authoritativeness and character (McCroskey, 1966b).

The hypotheses and statistical analyses for this study were the same as for Study VII.

Results. Table 7 reports the obtained mean authoritativeness and character scores for the three experimental conditions, the differences in mean scores between conditions, and the results of the *t*-tests of those differences. The hypotheses of no difference in perceived character between the biased and reluctant source conditions and between the reluctant and unbiased source conditions could not be rejected. There was no statistically significant difference between the conditions. The difference between the unbiased and biased source conditions approached significance. This difference can be considered significant if we use the results of Study VII as justification for a directional prediction. The hypotheses of no difference in perceived authoritativeness between the various source conditions, however, were rejected. The results indicated that the unbiased source (professor) was perceived to be more authoritative than the reluctant source, which in turn was perceived to be more authoritative than the biased source.

The results of these two studies provide support for the theory expressed in our textbooks that reluctant and unbiased testimony should be more helpful to the advocate than biased testimony. In both studies the reluctant and unbiased sources were perceived to be more authoritative than the biased source. Character scores were also higher for the reluctant and unbiased sources than for the biased source, although the difference in favor of the reluctant source in the second study was not statistically significant. Therefore, on the basis of these results and the previous research on the effect of source credibility on persuasion, we may conclude that reluctant and unbiased testimony are more potent persuasive tools than biased testimony.

The theory that reluctant testimony is superior to unbiased testimony was not supported by the results of these studies. In all cases the differences between the reluctant and unbiased source conditions favored the unbiased source condition. It would, however, seem unwise to conclude from these limited studies that unbiased testimony is superior to reluctant testimony. In these studies the reluctant source condition was a composite of two treatments including sources who were labor leaders and management leaders. The design of these studies, therefore, with college students as experimental subjects, may not have provided a good test of the hypotheses concerning the superiority of reluctant testimony over unbiased testimony.

Table 7

Results of Study VIII

Credibility Dimension*	Source Condition		
	Biased Source (1)	Reluctant Source	Unbiased Source (3)
(A) Authoritativeness	18.06	16.32	12.83
(B) Character	23.06	22.15	21.60

Hypothesis Tests

H:  $A_1 = A_2$ ;  $\bar{D} = 1.74$ ;  $t = 1.81^{***}$

H:  $B_1 = B_2$ ;  $\bar{D} = .91$ ;  $t = 1.23$

H:  $A_2 = A_3$ ;  $\bar{D} = 3.49$ ;  $t = 3.68^{**}$

H:  $B_2 = B_3$ ;  $\bar{D} = .55$ ;  $t = .75$

H:  $A_1 = A_3$ ;  $\bar{D} = 5.23$ ;  $t = 5.83^{**}$

H:  $B_1 = B_3$ ;  $\bar{D} = 1.46$ ;  $t = 1.73^{***}$

\*The hypothetical neutral point on both of these measures is 24.0. Scores can range from 6 to 42. The lower the score the higher the perceived credibility.

\*\*Statistically significant at at least the .05 level.

\*\*\*Statistically significant at the .10 level, two tailed. On the basis of the results of Study VII a directional hypothesis would be justified. A one-tailed  $t$ -test based on this hypothesis would meet the .05 significance criterion.



The unexpected finding that unbiased sources were perceived to be more credible than reluctant sources led to the decision to design and execute Studies IX and X. Study IX was a replication of Study VIII with one additional factor controlled--bias of the subjects on the issue discussed in the evidence specimen. The question of labor's role in producing inflation, the topic employed in Studies VII-IX, was believed to be relatively non-involving for college student subjects. It was believed that the possible negative reaction to reluctant testimony emanating from a source whose bias is shared by the receiver might be smaller than would be the case if a more ego-involving topic were discussed. Therefore, Study X employed the topic of college tuition. The results of these two studies provide additional information concerning the credibility of reluctant testimony.

### Study IX<sup>3</sup>

Procedure. The experimental stimuli used in Study IX were the same as those used in Study VII. Students in seventeen sections of Speech 101 at Michigan State University served as Ss. Copies of the six message-introduction combinations were randomly distributed to the students in each of these seventeen sections. Their attitudes toward the concept "Labor Unions Are a Primary Cause of Inflation" had been measured previously by a five-scale semantic differential. Scales employed were true-false, right-wrong, correct-incorrect, yes-no, and I agree-I disagree.

The Ss read instructions which informed them that "A workbook is currently being developed for use in Speech 101" and that "To select appropriate examples" for the unit on supporting materials "it is of major importance that we learn how students react to different kinds of written material." The Ss were asked to read the material about the source and his statement and then complete two six-scale semantic differentials. These semantic differentials were the same measures of perceived source authoritativeness and character employed in previous studies.

The pretest attitude measure provided a range of scores from 5 to 35 with the hypothetical neutral point at 20. Ss scoring exactly 20 were discarded. Those scoring 5-19 were labelled "anti-labor" and those scoring 21-35 were labelled "pro-labor". Statements attributed to sources which suggested labor is a cause of inflation were labelled "anti-labor" and statements which suggested labor is not a cause of inflation were labelled "pro-labor". This permitted the construction of six cells for analysis of the data. Figure 1 represents the classification scheme used for analysis of the data.

---

<sup>3</sup>Studies IX and X were conducted by the writer in cooperation with Mr. John R. Wenburg of Michigan State University.

Figure 1

Classification Scheme for Analysis of Data from Study XI

Subject Type	Ss-Message-Source Combinations		
	Biased	Reluctant	Unbiased
Ss who share bias of message	Pro-labor Ss- pro-labor message- labor leader and Anti-labor Ss- anti-labor message- business leader (Cell A1)	Pro-labor Ss pro-labor message- business leader and Anti-labor Ss- anti-labor message- labor leader (Cell A2)	Pro-labor Ss- pro-labor message- professor and Anti-labor Ss- anti-labor message- professor (Cell A3)
Ss who do not share bias of message	Pro-labor Ss- anti-labor message- business leader and Anti-labor Ss- pro-labor message- labor leader (Cell B1)	Pro-labor Ss anti-labor message- labor leader and Anti-labor Ss pro-labor message- business leader (Cell B2)	Pro-labor Ss- anti-labor message- professor and anti-labor Ss- pro-labor message- professor (Cell B3)

This procedure permitted examination of the following hypotheses:

1. A source is perceived as more credible when his message is consistent with the bias of the audience.  $H: \bar{X}_A > \bar{X}_B$

2. A source testifying against when appears to be his best interests (reluctant) is perceived as more credible than a source whose interests are not at stake (unbiased) who, in turn, is perceived as more credible than a source testifying in favor of what appears to be his best interests (biased).  $H: \bar{X}_2 > \bar{X}_3 > \bar{X}_1$

3. A source presenting reluctant testimony is perceived as more credible than an unbiased source who, in turn, is perceived as more credible than a biased source when the testimony is consistent with the bias of the audience.  $H: \bar{X}_{A2} > \bar{X}_{A3} > \bar{X}_{A1}$

4. A source presenting unbiased testimony is perceived as more credible than a biased source who, in turn, is perceived as more credible than a reluctant source when the testimony is not consistent with the bias

of the audience.  $H: \overline{XB3} > \overline{XB1} > \overline{XB2}$

Hypothesis one is based upon and consistent with what would be predicted by congruity theory. Hypothesis two is based upon traditional theory relating to the quality of types of evidence. Hypotheses three and four posit an interaction between bias of source and bias of audience in determining the perceived credibility of a piece of evidence. Specifically, these hypotheses suggest that reluctant testimony is the "best evidence" if the reluctant source and the audience do not share the same bias on the topic but that it is the "worst evidence" if the reluctant source and the audience do share the same bias on the topic. In the latter case it is presumed that the reluctant source would be perceived as a "traitor to the cause."

It should be stressed that the topic in Study IX was presumed to be no more than moderately salient to the experimental subjects. The following study (Study X) employed a highly salient topic.

Statistical analysis of the data for Study IX included  $2 \times 3$  factorial analyses of variance and subsequent t-tests.

Results. Table 8 reports the obtained mean authoritativeness and character scores for the six experimental conditions. None of our experimental hypotheses were confirmed. Hypothesis one was rejected because, contrary to our prediction, the Ss who shared the bias of the source's message rated the source as less credible than the Ss who did not share the bias of the source's message. Although these differences did not achieve statistical significance, they are contrary to what is normally found in dissonance and congruity studies. No explanation for this unexpected finding is available from the design of this study.

Hypothesis two, which was based upon traditional theory relating to the quality of types of evidence, was also rejected. Disregarding the bias of the Ss and adjusting for unequal cell size, there was no significant difference between the three source conditions on the character dimension. On the authoritativeness dimension the unbiased source was perceived to be significantly more credible than either the biased or the reluctant source which were not significantly different from each other. Again, however, we must use caution in interpreting the finding that the unbiased source was perceived to be the most credible. As in the two preceding studies, the unbiased source was a professor and as such may have obtained artificially high credibility ratings on authoritativeness from college student Ss.

Hypotheses three and four were rejected because the unbiased source consistently was perceived to be more credible (although not always significantly so) than either the biased or reluctant source and in no case



were the credibility ratings of the biased and reluctant source conditions significantly different from each other.

Therefore, the results of this study failed to confirm hypotheses based on dissonance theory, traditional theory of the value of reluctant testimony, and our theory of the "traitor" response of certain Ss to reluctant testimony.

Table 8  
Results of Study IX

Subject Type	Credibility Dimension*	Biased Source	Reluctant Source	Unbiased Source	F
Share Bias of Message	Authoritativeness	30.1	28.9	34.5	9.89**
	Character	25.2	24.1	26.3	3.34**
		N=56	N=55	N=51	
Do not Share Bias of Message	Authoritativeness	32.1	32.7	35.1	4.01**
	Character	25.6	25.8	26.1	.23
		N=47	N=43	N=43	

\*The hypothetical neutral point on both of these measures is 24.0. Scores can range from 6 to 42. The higher the score the higher the perceived credibility.

\*\*Statistically significant at at least the .05 level.

### Study X

Procedure. The experimental stimuli used in Study X were statements either favoring or opposing a recently adopted Michigan State University Graduated Tuition Plan and introductions indicating the source either paid maximum tuition under the plan, minimum tuition under the plan, or was a non-student employee in the MSU admissions office. Ss were the same as those in Study IX. Copies of the six message-introduction combinations were randomly distributed to the students in each of these seventeen sections of Speech 101 along with the instruments for Study IX. Ss attitudes toward the concept "The Graduated Tuition Plan at MSU is Unfair" had been previously measured by a five-scale semantic differential. The scales employed were the same as those for the topic in Study IX.

Each S participated in Study X immediately after completing the material for Study IX and no further instructions or "cover" were employed. The Ss were asked to read the material about the source and his statement and then complete the two semantic differentials for source credibility.

The pretest attitude measure provided a range of scores from 5 to 35 with the hypothetical neutral point at 20. Ss scoring exactly 20 were discarded. Those scoring 5-19 were labelled "pro-tuition" and those scoring 21-35 were labelled "anti-tuition". Statements attributed to sources which suggested the tuition plan is fair were labelled "pro-tuition" and statements which suggested the tuition plan is unfair were labelled "anti-tuition". This permitted the construction of six cells for analysis of the data. Figure 2 represents the classification scheme used for analysis of the data.

The hypotheses and statistical analyses for Study X were the same as for Study IX.

While the topic employed in Study IX was presumed to be moderately salient at most, the topic employed in Study X was thought to be highly salient. At the time of the study controversy was raging on the campus and around the state concerning the desirability of the new tuition program MSU had adopted. The experiment was conducted the second day of class of the first term after the new tuition plan had gone into effect. Each S had been confronted with the results of the new plan at registration less than one week previously. It was believed, therefore, that any differences in results between Studies IX and X could be attributed at least in part to the difference in salience of the two topics employed.

Results. Table 9 reports the obtained mean authoritativeness and character scores for the six experimental conditions. These results provided confirmation for our first hypothesis. Ss who shared the bias of the source's message rated the source significantly more credible on both dimensions than the Ss who did not share the bias of the source's message.

Figure 2

Classification Scheme for Analysis of Data from Study X

Subject Type	Ss-Message-Source Combinations		
	Biased	Reluctant	Unbiased
Ss who share bias of message	Pro-tuition Ss- pro-tuition mess.- minimum tuition	Pro-tuition pro-tuition mess.- maximum tuition	Pro-tuition Ss- pro-tuition mess.- non-student
	and	and	and
	Anti-tuition Ss- anti-tuition mess.- maximum tuition	Anti-tuition Ss- anti-tuition mess.- minimum tuition	Anti-tuition Ss- anti-tuition mess.- non-student
	(Cell A1)	(Cell B2)	(Cell A3)
Ss who do not share bias of message	Pro-tuition Ss- anti-tuition mess.- maximum tuition	Pro-tuition Ss- anti-tuition mess.- minimum tuition	Pro-tuition Ss- anti-tuition mess.- non-student
	and	and	and
	Anti-tuition Ss- pro-tuition mess.- minimum tuition	Anti-tuition Ss- pro-tuition mess.- maximum tuition	Anti-tuition Ss- pro-tuition mess.- non-student
	(Cell B1)	(Cell B2)	(Cell B3)

The results did not permit us, however, to accept our second hypothesis. Disregarding the bias of the Ss and adjusting for unequal cell size, there was no significant difference between the three source conditions on the authoritativeness dimension. On the character dimension the results were generally consistent with this hypothesis. The reluctant source was perceived to be more credible than the unbiased source (but not significantly so) and both of these were perceived to be significantly more credible than the biased source.

Hypothesis three was not supported by the results of this study. The unbiased source was perceived to be significantly more credible on both dimensions than the biased source by the Ss who shared the bias of the source's message. On the authoritativeness dimension the biased and reluctant source conditions were not significantly different. On the character dimension the reluctant and unbiased source conditions were not significantly different.

Table 9  
Results of Study X

Subject Type	Credibility Dimension	Source Condition			F
		Biased Source	Reluctant Source	Unbiased Source	
Share Bias of Message	Authoritativeness	26.6	27.4	30.0	3.77**
	Character	25.2	27.8	28.5	6.16**
		N=42	N=52	N=51	
Do not Share Bias of Message	Authoritativeness	22.9	22.6	23.2	.09
	Character	23.8	26.6	24.4	7.86**
		N=57	N=59	N=42	

\*The hypothetical neutral point on both of these measures is 24.0. Scores can range from 6 to 42. The higher the score the higher the perceived credibility.

\*\*Statistically significant at at least the .05 level.

The results of this study also force us to reject our fourth hypothesis. On the authoritativeness dimension the Ss who did not share the bias of the source's message rendered scores for the three source conditions that were not significantly different. On the character dimension the reluctant source was perceived to be significantly more credible than the biased and unbiased source. The latter conditions were not significantly different from each other. It is important to note that this is the only case in which the reluctant source has been found to be perceived as more credible than the unbiased source and is the precise condition where this effect was not expected. Employing our "traitor" theory we predicted that the reluctant source would be perceived to be significantly less credible than either the biased or unbiased source when the Ss did not share the bias of the source's message.

### Conclusions

The primary purpose of these four studies was to test the assumption that the evidence employed in the studies reported in the previous sections was of the type perceived by experimental Ss to be of high quality. The results of these studies provided no reason for us to suspect that the evidence included in the speeches in those studies would be perceived



as less than satisfactory. On the contrary, since the results of studies VII-X generally indicate that evidence from unbiased sources is perceived to be more credible than either biased or reluctant and unbiased evidence was the primary type employed in the experimental speeches, we have good reason to believe that this evidence would be perceived as of the high quality intended.

The results of studies VII-X, however, provide a significant challenge to the traditional theory of the value of various types of evidence. While reluctant testimony is traditionally claimed to be the most desirable form, our results suggest that it is no better than unbiased testimony--if it is even as desirable.

Study X makes this challenge most strongly. Whereas in the other three studies the superiority of unbiased evidence could be attributable to the fact that the unbiased source was a professor and the Ss were college students, in study X the unbiased source was merely an employee in the college admissions office with no other qualifications noted. Similarly, the results of the first three studies could be challenged on the basis that the topic employed was not salient to the Ss. However, the results of study X were generally consistent with the earlier studies and the topic employed was highly salient to the Ss. In fact it is doubtful that a more salient topic could ever be employed in an experimental study than this one.

While the traditional theory of the value of various types of evidence is cast into serious question by our results, our suggested alternate theory--the "traitor" theory--received absolutely no support from our results. In study IX the "traitor" condition received significantly higher authoritativeness scores than the "reluctant" condition--32.7 to 28.9. While in study X the difference was in the predicted direction and significant on the authoritativeness dimension, on the character dimension the "traitor" condition received significantly higher scores than the unbiased condition.

It appears from these results that some variable not controlled in the studies is operating when people respond to reluctant testimony. Speculation as to what that variable might be would serve no useful purpose here. It is clear that further research is needed before we can state with precision the relative credibility of the various types of authority-based evidence. Regardless of the outcome of such studies, however, the results of the four studies reported in this section, which consistently indicated that unbiased evidence is superior to biased evidence, permit us to reject the credibility of the evidence employed in the speeches in the experiments reported in Section III as an explanation for our discrepant results in those studies.

## STUDIES OF EVIDENCE AND DELIVERY

In Section III we noted that Ss who participated in Studies IV and V who were interviewed indicated mixed evaluations of the delivery of the capital punishment speeches. These same Ss indicated that the delivery of the federal control of education speeches was of high quality. This led us to speculate that delivery and evidence might interact in persuasive communication.

It is obvious from the results of the studies reported in Sections II and III that evidence can have a positive effect in increasing immediate attitude change and perceived ethos in persuasive communication. It is equally as obvious that evidence does not always have such an effect. If mediocre or poor delivery interferes with the normal functioning of the evidence included in a message, the inconsistent results between the capital punishment and federal control of education topics might be explained.

Studies XI and XII were designed, therefore, to test one primary hypothesis: Inclusion of good use of evidence in a persuasive message increases attitude change and perceived ethos when the message is well delivered but has no effect on either attitude change or ethos when the message is poorly delivered.

### Study XI<sup>1</sup>

Procedure. The "evidence" and "no evidence" versions of the federal control of education speech were each presented live to two audiences composed of twenty-eight students enrolled in the basic speech course at The Pennsylvania State University. Ss were randomly assigned to experimental treatments administered at evening sessions outside the students' normal classrooms. Each version was presented to one audience with moderately good delivery and to the other audience with very poor delivery.

Ss were informed that they were participating in a study concerned with determining how much they knew about "style in speech." The avowed reason for the study was to determine whether there was a need for greater stress on this subject in the course in which they were enrolled. Ss were told that they would be asked to complete "a questionnaire" and write an evaluation of the style employed in the speech. The experimental speaker (the writer) was casually dressed and not introduced. The experimenter gave

---

<sup>1</sup>This study was conducted by the writer in cooperation with Dr. William E. Arnold of the University of Connecticut.

the instructions to the Ss, asked if there were any questions (there never were), and then nodded to the speaker who rose from the front row and walked to the podium.

The "poor delivery" condition included both poor oral and poor visual characteristics. There were numerous vocalized pauses and repetitions, excessively rapid rate, monotonous pitch, lack of variety, no eye contact except a few furtive glances at the audience, and numerous distracting and meaningless gestures. The "good delivery" condition was as nearly opposite the "poor delivery" condition as was possible given the limitations of the speaking ability of the experimental speaker.

The Ss completed a pre-test attitude semantic differential a week prior to the experiment during regular class sessions along with several irrelevant scales. Immediately after hearing the experimental speech the Ss again completed the attitude measure. They also completed semantic differential measures for the authoritativeness, character, and dynamism dimensions of ethos. The authoritativeness and character measures were the same as those used in previous studies. The dynamism measure was based on the factor analytic research conducted by Berlo, Lemert, and Mertz (1966).

Results. Posttest attitude scores were subjected to analysis of covariance with pretest attitude scores serving as the covariate. Terminal ethos scores were subjected to analysis of variance. In addition, t-tests appropriate to our primary hypothesis were computed. Table 10 reports the covariance adjusted mean terminal attitude scores and perceived ethos scores.

The results partially support our primary hypothesis. The attitude results were precisely as predicted. In the "good delivery" condition inclusion of evidence produced significantly greater attitude change. In the "poor delivery" condition inclusion of evidence produced no significant effect on attitude change. Neither delivery nor evidence produced an overall significant effect on attitude change. All treatments produced significant attitude change; but the good delivery-no evidence, poor delivery--evidence, and poor delivery-no evidence treatments were essentially equally successful in producing attitude change. The good delivery-evidence treatment was significantly more successful in producing attitude change than any of the other three treatments.

Unlike the attitude change results, the terminal ethos results do not support our hypothesis. Both evidence and delivery produced significant effects on terminal ethos on the authoritativeness and character dimensions. There was no significant interaction on either dimension. Delivery produced a large and significant effect on the dynamism dimension of ethos with the good delivery condition being perceived more dynamic. Evidence usage produced no effect on dynamism.



Table 10  
Results of Study XI

Dependent Variable	Evidence	No Evidence	$\bar{D}$
Covariance Adjusted Posttest Attitude*			
Good Delivery	32.83	29.12	3.71**
Poor Delivery	29.23	29.85	-.62
Perceived (Posttest) Authoritativeness*			
Good Delivery	32.29	29.39	2.90**
Poor Delivery	25.65	23.14	2.51**
Perceived (Posttest) Character*			
Good Delivery	34.22	31.00	3.22**
Poor Delivery	25.48	23.09	2.39**
Perceived (Posttest) Dynamism*			
Good Delivery	32.68	31.50	1.38
Poor Delivery	19.65	20.10	-.55

\*The higher the score, the more favorable the attitude or ethos. The hypothetical neutral point on the scale is 24.0.

\*\*Statistically significant at at least the .05 level.



These results indicate that delivery was probably not the factor which produced conflicting results in earlier studies. Although the results with regard to attitude change are consistent with such an explanation, the ethos results are not. It is apparent that even though inclusion of evidence in the message had no effect on attitude change in the poor delivery condition that same evidence did increase perceived authoritativeness and character. These results, therefore, are not comparable to those in Study IV on the capital punishment topic which they should be if the delivery explanation were correct. We will recall that inclusion of evidence in the capital punishment speech in that study had no significant effect on either attitude or ethos.

It does not seem appropriate, however, to completely reject the delivery explanation on the basis of this single study. In addition to the fact that any study needs to be replicated before firm generalizations are drawn, this particular study lacked certain elements of design that would be desirable. The experimental speeches were presented live and thus were not exactly alike in supposedly comparable conditions. The initial ethos of the experimental speaker was not measured or manipulated and only assumed to be controlled. The earlier studies employed tape recorded speeches and thus the live treatment conditions in this study may have introduced variables that interfere with comparisons between studies.

Because of these problems another study was conducted. Study XII employed both a more complex and a more rigorous design. The results of Study XII, as we shall note below, permit us to make more confident generalizations about the interrelationships of ethos, evidence, and delivery in persuasive communication.

### Study XII

In previous studies evidence was found to interact with both initial ethos and delivery in producing immediate attitude change. This was the first study, however, which investigated the effects of evidence while manipulating both initial ethos and delivery. On the basis of the previous studies it was hypothesized that evidence would have a significant impact on immediate attitude change only in a moderate-to-low ethos, good delivery condition. An interaction between initial ethos, quality of delivery, and evidence usage was thus predicted for immediate attitude change.

Although the results of previous studies point to an interaction of ethos and evidence in producing immediate attitude change, there is no indication of such an interaction in producing sustained attitude change. Similarly, there is no indication that there should be an interaction between delivery and evidence in producing sustained attitude change. It was hypothesized, therefore, that inclusion of evidence would increase sustained attitude change for both high- and low-ethos sources whether the message was delivered well or poorly.

The primary concern of this study was to test the two hypotheses indicated above. The procedure employed, however, permitted testing several additional hypotheses. These hypotheses were:

1. Inclusion of evidence increases perceived terminal ethos--authoritativeness and character--for both high- and low-ethos sources whether the message is delivered well or poorly.
2. Good delivery improves perceived terminal ethos--authoritativeness, character, and dynamism.
3. Perceived terminal ethos is higher for a source initially introduced as a high-ethos source than for a source initially introduced as a low-ethos source.
4. Inclusion of evidence has no effect on perceived dynamism.
5. Good delivery has no effect on sustained attitude change.
6. Initial ethos has no effect on sustained attitude change.
7. Messages transmitted by audio tape and messages transmitted by video tape are equally effective in producing immediate attitude change.
8. Messages transmitted by audio tape and messages transmitted by video tape are equally effective in producing sustained attitude change.
9. Media of message transmission has no effect on perceived terminal ethos.
10. Delayed posttest attitude scores are not affected by completing immediate posttest measures.

Procedure. The "evidence" and "no evidence" versions of the federal control of education speech employed in previous studies were shortened by eliminating references to proposed federal control. As shortened the speech represented a condemnation of local control of education without the endorsement of an alternative.

The design for the study was a  $2 \times 2 \times 2 \times 2 \times 2$  factorial. This design provided 32 treatment conditions. The five factors in the design were initial ethos (high and low), evidence usage (evidence and no evidence), quality of delivery (good and poor), media of transmission (video tape and audio tape), and amount of posttest measurement (delayed posttest only and both immediate and delayed posttest). A control group ( $N = 26$ ) completed a pretest, immediate posttest, and delayed posttest attitude measure but received no experimental treatment.

Ss involved in the study were students enrolled in thirty-nine sections of Speech 101 (Public Speaking), twelve sections of Speech 108 (Voice and Articulation), and five sections of Speech 116 (Discussion) during the Fall, 1966, Term at Michigan State University. Each of these courses is considered a "basic" course and there are no prerequisites. Students in these courses are mostly Freshmen and Sophomores although upper division students are also included. Students who were enrolled in more than one of the courses were excluded from the experiment.

Complete sections were assigned to experimental treatments at random with two exceptions. Because the experiment was to be administered during regular class periods it was necessary to avoid selecting sections which met in classrooms without a television receiver for the video-tape treatments. Since the writer was also the experimental speaker a section of Speech 116 which he taught was selected as the control group.

The two versions of the speech were delivered live in the Closed Circuit Television studios at Michigan State University. Each version was delivered once with moderately good delivery and once with very poor delivery. The delivery distinctions were similar to those in Study XI. Each of the four presentations was video taped.

Initial ethos inductions were presented by means of the "voice over" technique during a closeup shot of the speaker seated near the podium. After the introduction the speaker rose and walked to the podium. Each introduction was electronically spliced to each speech presentation so as to produce eight separate and complete introduction-speech combinations.

Each introduction began with reference to the alleged fact that the speech to be presented was produced by the National Educational Television Network for its regular program called "Controversy." During this reference the word CONTROVERSY appeared on the screen. As the introduction of the speaker began the closeup of the speaker appeared in view.

Audio tape recordings were prepared by the Closed Circuit Television staff from the video tape recordings. Thus, the audio tape and video tape treatments were exactly the same orally.

All of the treatment conditions were administered during regular class periods by the section instructors. The Ss were told that they were participating in a project concerning "style" in public speaking and that they would be asked to write a paragraph concerning the style employed by the speaker. They were not told they were in an experiment but rather were led to believe the study was a regular class project. They were told, however, that their reactions would have no bearing on their grade. The alleged purpose of the project was to determine whether more emphasis should be placed on style in the basic public speaking course.



The writer had no direct contact with any of the students in the experiment. It was possible for him to serve as the experimental speaker in this study because he had only recently joined the MSU staff and was totally unknown to the students involved in the study. During the several weeks immediately preceding and following the experiment he systematically avoided any contact with undergraduate students except those enrolled in his classes. Only one of these classes was involved in the study, and as was noted above, this class served as the control group.

All of the Ss completed a pretest attitude semantic differential approximately four weeks prior to the experiment and again completed the same instrument as a delayed posttest measure approximately four weeks after the experiment. In each case the measure was administered along with similar measures on several "cover" concepts. Ss in the "delayed posttest only" condition completed no other measure. After being exposed to an experimental treatment these Ss merely wrote a paragraph on the style employed by the speaker. The Ss in the "immediate and delayed posttest" condition completed the attitude measure and measures of perceived authoritativeness, character, and dynamism immediately after exposure to an experimental treatment before writing their paragraph on style. The ethos measures were the same as those employed in Study XI. The attitude measure had been developed at The Pennsylvania State University at the same time the Capital Punishment and Federal Control of Education measures were developed. The six scales employed were the same as those used for the other two concepts. Their original factor loadings were among the top eight of forty evaluative scales employed in pretesting.

Immediate posttest attitude scores were submitted to four-factor analysis of covariance with the pretest attitude scores serving as the covariate. The delayed posttest attitude scores were submitted to five-factor analysis of covariance with the pretest attitude scores serving as the covariate. Ethos scores on each dimension were submitted to four-factor analysis of variance. To test hypotheses not relating to main effects appropriate t-tests were computed. The .05 level was required for significance.

A total of 800 subjects were included in the final analysis of results (excluding the control group). Ss who did not complete any single part of the study were excluded. Cells were then balanced by random exclusion of Ss with one exception. Thirty Ss were retained in each cell which completed all measures. Twenty Ss were retained in each cell which completed only the pretest and delayed posttest of attitude.

Results. Analysis of covariance of the immediate posttest attitude scores indicated one significant main effect and two significant interactions. The media main effect was significant with the audio tape condition producing significantly more attitude change than the video tape condition. A significant media x delivery interaction was also observed. This can be



attributed to the loss in attitude change produced by the poor physical delivery characteristics observable in the poor delivery-video tape treatments.

The other significant interaction effect was the initial ethos x delivery quality x evidence usage effect. The results of t-tests indicated that inclusion of evidence significantly increased attitude change only in the low-ethos--good delivery treatment. This occurred in both the audio tape and the video tape conditions. In addition, it was observed that high initial ethos significantly increased attitude change only in the no evidence--good delivery treatment. This occurred in both the audio tape and the video tape conditions. Finally, the t-tests indicated that good delivery significantly increased attitude change only in the high-ethos treatments and in the low-ethos--with evidence treatments.

Analysis of covariance of the delayed posttest attitude scores indicated two significant main effects, those for evidence and delivery. Inclusion of evidence produced significantly greater sustained attitude change as did good delivery of the message. Attitudes of the control group did not shift significantly on either the immediate or the delayed posttest.

Analysis of variance of the authoritativeness scores indicated four significant main effects. Higher perceived terminal authoritativeness was produced by the high initial ethos introduction, by inclusion of evidence, by good delivery, and by the audio tape media. There were no significant interactions observed, but the delivery x media interaction approached significance ( $p < .10$ ).

Analysis of variance of the character scores indicated four significant main effects. These results were nearly identical to those on the authoritative-ness dimension. Again there were no significant interactions observed, but the delivery x media interaction approached significance ( $p < .10$ ).

Analysis of variance of the dynamism scores indicated three significant main effects and three significant interactions. Higher perceived dynamism was produced by the high initial ethos introduction, by good delivery, and by the audio tape media. Significant interaction effects were observed for initial ethos x media, evidence usage x media, and initial ethos x evidence usage x delivery quality. Since no interaction on perceived dynamism had been either predicted or expected, these results were not further analyzed.

The results of Study XII are presented in Tables 11 and 12. Table 11 includes results on each of the dependent variables for the main effects not confounded by interactions. Table 12 reports the individual cell means for each dependent variable.

Table 11

Main Effect Results of Study XII Not Affected by Interactions

Main Effect	Dependent Variable	Mean Score		$\bar{D}$
		<u>Evidence</u>	<u>No Evidence</u>	
Evidence	Delayed Posttest*	20.19	18.62	1.52***
	Authoritativeness**	17.35	20.26	2.91***
	Character**	23.41	24.81	1.40***
Ethos		<u>High</u>	<u>Low</u>	
	Delayed Posttest*	19.34	19.47	.14
	Authoritativeness**	15.28	22.33	7.05***
	Character**	22.36	25.86	3.50***
Delivery		<u>Good</u>	<u>Poor</u>	
	Delayed Posttest*	20.14	18.67	1.47***
	Authoritativeness**	15.02	22.59	7.57***
	Character**	21.87	26.35	4.48***
Media		<u>Audio</u>	<u>Video</u>	
	Immediate Posttest*	26.55	23.62	2.93***
	Delayed Posttest*	19.52	19.29	.23
	Authoritativeness**	18.17	19.44	1.27***
	Character**	23.31	24.91	1.60***
Measurement		<u>Immediate</u>	<u>Delayed</u>	
		<u>and Delayed</u>	<u>Only</u>	
	Delayed Posttest*	19.39	19.42	.03

\*The higher the score the more favorable the attitude. The hypothetical neutral point on the scale is 24.0.

\*\*The lower the score the more favorable the ethos. The hypothetical neutral point on the scale is 24.0.

\*\*\*Statistically significant at at least the .05 level.

Table 12

Results of Study XII--Individual Cell Means

Dependent Variable			High Ethos				Low Ethos			
			Video Tape		Audio Tape		Video Tape		Audio Tape	
			Good Delivery	Poor Delivery	Good Delivery	Poor Delivery	Good Delivery	Poor Delivery	Good Delivery	Poor Delivery
Immediate Posttest Attitude (N=30)	a	E	27.08	21.91	29.28	25.36	26.18	22.05	29.04	25.41
		N	26.33	21.52	28.62	24.92	22.32	21.54	24.63	25.14
Delayed Posttest Attitude (N=50)	a,c	E	20.63	19.31	21.20	19.15	20.88	19.69	21.22	19.40
		N	19.12	18.02	19.41	17.86	19.01	17.62	19.68	18.34
Ethos: Authoritativeness (N=30)	b	E	10.29	18.43	10.10	17.19	17.90	25.03	15.97	23.89
		N	12.15	21.81	12.41	19.85	22.68	27.25	18.63	27.30
Ethos: Character (N=30)	b	E	19.95	25.38	18.69	22.72	23.89	27.79	22.61	26.25
		N	21.27	27.24	19.42	24.17	24.65	29.07	24.51	28.17
Ethos: Dynamism (N=30)	b	E	15.89	31.27	12.29	24.64	20.17	33.03	13.19	25.17
		N	13.55	26.81	16.79	25.24	18.46	28.00	12.40	32.12

a The higher the score, the more favorable the attitude.

b The lower the score, the higher the perceived ethos.

c Subjects in the two measurement conditions are included.



Conclusions of Study XII. We can best interpret these results by referring to our hypotheses.

Evidence has a significant impact on immediate attitude change only in a moderate- to low-ethos, good delivery condition. On the basis of the results of this study we may not reject this hypothesis. The effects on immediate attitude change observed in this study were precisely predicted by this hypothesis. We should not expect, therefore, that evidence will have an impact on immediate attitude change if either the source is perceived to be of relatively high ethos or the message is not well presented.

Inclusion of evidence increases sustained attitude change for both high- and low-ethos sources whether the message is delivered well or poorly. This hypothesis cannot be rejected. Sustained attitude change was greater for the "evidence" version than the "no evidence" version under all conditions of initial ethos, delivery, and media. Taken in conjunction with the results of the previous studies in which we measured delayed posttest attitude we can be reasonably assured that although inclusion evidence does not always have an effect on immediate attitude change it does regularly produce greater sustained attitude change. It must be stressed, however, that this is not an example of what is often called the "sleeping effect." When a "sleeping effect" is present, there is an increase in attitude change attributable to some variable over time. In no case have we observed such an effect in any of these studies. What we have observed is that inclusion of evidence reduces the decrease in attitude change over time.

Inclusion of evidence increases perceived terminal ethos--authoritativeness and character--for both high- and low-ethos sources whether the message is delivered well or poorly. This hypothesis may not be rejected. All of our results relevant to this hypothesis are in the direction predicted. We may conclude, therefore, that even though evidence does not always have an effect on immediate attitude change it does regularly improve perceived authoritativeness and character.

Good delivery improves perceived terminal ethos--authoritativeness, character, and dynamism. This hypothesis may not be rejected. The results of both this study and Study XI clearly indicate that delivery has a large and significant effect on all three dimensions of ethos.

Perceived terminal ethos is higher for a source initially introduced as a high-ethos source than for a source initially introduced as a low-ethos source. This hypothesis may not be rejected. The primary implication of this result is that the initial ethos inductions "took." We should note also that this hypothesis held true for the dynamism dimension even though the initial ethos inductions made no apparent reference to this dimension. This finding suggests that either dynamism is not psychologically independent of the other dimensions of ethos or that it is evaluative in nature and thus influenced by other evaluative elements.



Inclusion of evidence has no effect on perceived dynamism. This hypothesis may not be rejected. The results of both this study and of Study XI indicated no significant effect of evidence on perceived dynamism.

Good delivery has no effect on sustained attitude change. This hypothesis may be rejected. Although delivery appeared to have no main effect on immediate attitude change in either Study XI or Study XII, the results of Study XII indicated a significant effect attributable to delivery on sustained attitude change. Greater attitude change was retained by Ss exposed to good delivery than by those exposed to poor delivery.

Initial ethos has no effect on sustained attitude change. This hypothesis may not be rejected. The results of this study indicated no significant effect attributable to initial ethos on sustained attitude change. In some cases in the studies reported in Section III, however, a significant difference in favor of high initial ethos was observed. Further research investigating the long term effects of initial ethos on attitude change is needed.

Messages transmitted by audio tape and messages transmitted by video tape are equally effective in producing immediate attitude change. This hypothesis may be rejected. The results obtained from this study indicated that the audio tape treatment produced significantly greater immediate attitude change. The reason for this difference is not clear. Two explanations seem reasonable. The appearance of the speaker may have detracted from his effectiveness or the good delivery condition may have included some undesirable visual characteristics which detracted from the speaker's effectiveness. Since the poor delivery condition included numerous instances of poor visual characteristics, it was expected that an interaction between delivery and media would be observed. None was. This lends some credence to the second explanation, but much further research is needed before we may make any meaningful generalization relating to this hypothesis.

Messages transmitted by audio tape and messages transmitted by video tape are equally effective in producing sustained attitude change. This hypothesis may not be rejected. The results of this study indicated no significant effect on sustained attitude change attributable to media. This raises the possibility that the finding discussed above relating to the effect of media on immediate attitude change could have been merely a chance occurrence.

Media of message transmission has no effect on perceived terminal ethos. This hypothesis may be cautiously rejected. On all three dimensions of ethos a significant effect on perceived ethos was observed for media. Caution is needed in the interpretation of these apparently clear results however. An interaction between media and delivery was expected on perceived

ethos because of the poor oral delivery characteristics introduced into the video tape-poor delivery condition which was not observable in the audio tape-poor delivery condition. This finding appeared and approached significance on the authoritativeness and character dimensions. The fact that the expected interaction was not significant and that a significant main effect for media favoring the audio tape condition appeared suggests the possibility that some poor visual characteristics crept into the good delivery-video tape condition. Further research on the relationship between media and perceived ethos is needed.

Delayed posttest attitude scores are not affected by completing immediate posttest measures. This hypothesis may not be rejected. No effect attributable to amount of posttest measurement was observed. Covariance adjusted mean delayed posttest attitude scores for the two conditions of testing were almost identical. This finding suggests that researchers need not be concerned with masking the fact that Ss are in an experiment in order to obtain meaningful delayed posttest measures of attitude change. This presumes, of course, that the Ss are not informed of the true nature of the experiment prior to completion of delayed posttest measures.

### Conclusions

The primary purpose for conducting Studies XI and XII was to discover whether the inconsistent results across topics obtained in Studies IV and V could be attributed to mediocre delivery of the capital punishment speeches in those studies. By employing speeches which had been previously demonstrated to obtain significant effects for evidence in a low-ethos condition but not in a high-ethos condition and systematically manipulating delivery quality it was possible to determine whether the inconsistent effects previously observed were induced by deviations in delivery quality.

On the basis of the results of Studies XI and XII it would not seem that delivery was the factor producing our earlier discrepant results. The results relating to immediate attitude change in Studies XI and XII were as we expected when we assumed delivery would interact with ethos and evidence in producing attitude change. Evidence made a significant difference on attitude change only in the low-ethos, good delivery conditions. This finding would lead us to accept delivery as the explanation of the inconsistent results of Studies IV and V if it were taken alone. However, when we consider the results on the ethos dependent variables in Studies XI and XII we find that they do not permit acceptance of the delivery explanation. In Study IV evidence had no effect on ethos on the capital punishment topic when the messages were supposedly poorly delivered but did have an effect on the federal control of education topic when the messages were supposedly well delivered. Yet in Studies XI and XII evidence made a positive contribution to perceived ethos whether the message was delivered well or not.



Even though we must reject delivery as the explanation for the inconsistent results in Studies IV and V, we can conclude with reasonable certainty that delivery does interact with ethos and evidence in producing immediate attitude change. On the basis of the results of these studies it appears that delivery is a non-additive factor in the production of attitude change. Rather, it is a permissive factor. Good delivery allows evidence and ethos to function in the production of attitude change while poor delivery inhibits the operation of these other two communication variables.

In Section III we posited three possible explanations for our observed results which indicated evidence to have a significant effect on attitude change and ethos on one topic but not on another. The first explanation, that our unbiased evidence which was included in the capital punishment topic messages may not have been of as high quality in the perception of our experimental Ss as we thought and thus our good evidence usage treatment was really a poor evidence usage treatment, was rejected in Section IV when we learned that both unbiased and reluctant testimony were perceived to be good by similar Ss and that reluctant testimony in some cases was significantly inferior to unbiased testimony. The second explanation, that poor or mediocre delivery of the capital punishment topic messages produced the obtained inconsistent results, has been rejected in this section.

We are left, therefore, with only one other explanation, that evidence which is already familiar to an audience produces no effect on attitude change or ethos while new or unfamiliar evidence increases attitude change and perceived ethos. If we must also reject this third explanation, we will be forced to conclude that some other factor which we have not considered (or chance which is extremely unlikely) produced the inconsistent results across topics. Study XIII was designed to test our third hypothesized explanation. This study is reported in the following section.

VI

A STUDY OF EVIDENCE, ETHOS, AND PRIOR KNOWLEDGE OF AUDIENCE

Studies discussed in the preceding sections of this report clearly indicate that evidence can be a useful tool for the communicator who desires to influence the attitudes of his audience. It is similarly clear that inclusion of evidence does not guarantee that all communicators will be more successful on all topics with all audiences. In the primary studies discussed in Section III it was apparent that the ethos of the source has a major influence on the usefulness of evidence in producing attitude change, for the high ethos source it has no impact but for the low ethos source it can in some cases.

The failure to obtain precise replication across topics in the studies reported in Section III caused us to speculate about factors which may interact in determining whether inclusion of evidence will increase the amount of attitude change a communicator can generate in his audience. In Section IV we found that some types of sources of evidence are perceived as more credible than others but discounted this variable as a producer of the conflicting results of the studies reported in Section III. In Section V we found that delivery definitely has a relationship to the utility of evidence as a producer of attitude change but again discounted this variable as a producer of the conflicting results reported in Section III.

In the original report of the studies discussed in Section III, the writer posited prior knowledge of the audience as the most likely cause of the conflicting results across topics in these studies (McCroskey, 1966a). Study XIII was designed to test whether if the audience was aware of evidence prior to its presentation by a speaker he could increase their attitude change and his perceived ethos by presenting that evidence in his message.

Study XIII

Procedure. The tape recorded versions of the "evidence" and "no evidence" speeches on federal control of education employed in studies IV-VI were selected for use in study XIII. These tapes were selected because the delivery on them had been perceived previously to be good by Ss similar to those employed in this study. The results of the studies reported in Section V stress the importance of good delivery to avoid contamination of results. Similarly, the speeches on these tapes included evidence that, on the basis of the results of the studies reported in Section IV, can confidently be labelled "satisfactory." Finally, these tapes were selected because they had produced unequivocal differences in the impact of evidence on attitude change between high- and low-ethos sources in studies IV-VI.



Students in sixteen sections of Speech 101 at Michigan State University served as experimental Ss. Experimental treatments were presented during the second regular class period of the course. Each of the two versions of the speech was presented to eight sections. In four sections for each version the source was identified as a high-ethos individual and in the other four sections for each version the source was identified as a low-ethos individual. Ethos inductions were the same as those employed in studies IV and V (McCroskey, 1966a) and were presented to the Ss in written form with a pretest ethos measure immediately below the ethos induction. Analysis of the pretest measures indicated the ethos inductions were perceived as intended. This procedure provided four experimental conditions--high ethos-evidence, high ethos-no evidence, low ethos-evidence, and low ethos-no evidence--with four sections of students in each condition. These four conditions were further divided into eight conditions by exposing two of the sections in each condition to the evidence included in the experimental speech prior to exposure to the speech. This was accomplished by transcribing the evidence used in the evidence version of the speech and presenting it to the students with the following cover:

The speech that you will hear is on the crucial topic of the role of the Federal Government in public elementary and secondary education. Because this topic is so important and; at the same time, so controversial, SCRL believes that it is essential that you be aware of information vital to reaching a decision on this topic before you are exposed to the views expressed in the speech to be presented in this project. We have, therefore, investigated this topic thoroughly and below are listed some of the most important facts on this question.

Please read this information carefully and underline the points that you feel are most important.

It was assumed that this procedure would guarantee that the Ss exposed to the evidence would be aware of it when they listened to the experimental speech. It was further assumed that the Ss in this experiment (MSU students) were similar to the Ss used in studies IV and V (Penn State students) and that the students in all of the studies were relatively unfamiliar with the evidence included in the experimental speech. If the latter assumption were not valid, non-confirming results relating to the hypotheses for this study would enable us to discount prior knowledge as the factor producing the conflicting results across topics in studies IV and V and lead us to severely question any theory of the role of evidence in persuasive communication which is based on prior knowledge as a factor. Results confirming our hypotheses, on the other hand, would suggest that these assumptions are correct and that prior knowledge is indeed a relevant factor which could have produced the conflicting results across topics in studies IV and V.

After completing the ethos pretest and reading the transcribed evidence (or not reading it in the "no prior knowledge" condition), the Ss were exposed to the appropriate tape recorded speech. Immediately after hearing the speech the Ss completed semantic differential posttest measures of attitude, perceived source authoritativeness and character, and speech evaluation. Ss had previously completed a pretest attitude measure.

Posttest attitude measures were subjected to  $2 \times 2 \times 2$  factorial analysis of covariance with the pretest attitude measures serving as a covariate. Scores based on the difference between pretest and posttest ethos measures were subjected to  $2 \times 2 \times 2$  factorial analyses of variance. Speech evaluation ratings were subjected to  $2 \times 2 \times 2$  factorial analysis of variance. Appropriate t-tests were computed when necessary to test specific a priori hypotheses.

The design of the experiment is represented by Figure 3.

Figure 3

Design of Study XIII

(A1) Prior Knowledge of Evidence				(A2) No Prior Knowledge of Evidence			
(B1) Evidence		(B2) No Evidence		(B1) Evidence		(B2) No Evidence	
(C1)	(C2)	(C1)	(C2)	(C1)	(C2)	(C1)	(C2)
High	Low	High	Low	High	Low	High	Low
Ethos	Ethos	Ethos	Ethos	Ethos	Ethos	Ethos	Ethos
Sec. 1	Sec. 3	Sec. 5	Sec. 7	Sec. 9	Sec. 11	Sec. 13	Sec. 15
Sec. 2	Sec. 4	Sec. 6	Sec. 8	Sec. 10	Sec. 12	Sec. 14	Sec. 16
A1B1C1	A1B1C2	A1B2C1	A1B2C2	A2B1C1	A2B1C2	A2B2C1	A2B2C2

The hypotheses for Study XIII predicted an interaction between prior knowledge, evidence, and ethos--an ABC interaction. Specifically, on the basis of the results of the previous studies, there was no significant difference attributable to evidence predicted for either high or low ethos sources in the "prior knowledge" conditions or for the high ethos source in the "no prior knowledge" conditions. It was predicted, however, that the "low ethos-evidence" treatment would produce greater attitude change and perceived ethos than the "low ethos-no evidence" treatment in the "no prior knowledge" condition.

These hypotheses, therefore, suggest that, if the audience is aware of evidence before it is presented by a speaker or if the speaker is perceived to be highly credible, inclusion of evidence does not enable a speaker to increase either the amount of audience attitude change or his perceived ethos. But, if the audience is not familiar with the evidence and the source is perceived to be of moderately low credibility, inclusion of evidence enables a speaker to increase both the amount of audience attitude change and his perceived ethos. In the latter case it is presumed that the delivery of the speaker is good and that the evidence employed is satisfactory.

Results. Table 13 reports the covariance adjusted postcommunication attitude scores and the difference scores (posttest minus pretest) for the authoritativeness and character dimensions of ethos for the eight conditions in study XIII. Analysis of variance (covariance on attitude) for these three dependent variables indicated two significant (at least .05) effects in each of the three cases--ethos and ABC interaction. The high ethos condition produced significantly more attitude change than the low ethos condition. Although in all cases the evidence speech produced at least slightly more attitude change than the no evidence speech, the overall effect for evidence was not statistically significant. However, in the low-ethos, no prior knowledge condition the evidence speech produced significantly more attitude change than the no evidence speech. This result conformed precisely to the predicted ABC interaction.

Table 13

Results of Study XIII--Primary Dependent Variables

Dependent Variable	Evidence Condition	Prior Knowledge		No Prior Knowledge	
		High Ethos	Low Ethos	High Ethos	Low Ethos
Covariance Adjusted Postcommunication Attitude*	Evidence	32.4 N=43	30.7 N=43	32.5 N=44	30.1 N=42
	No Evidence	32.2 N=43	30.3 N=42	32.0 N=47	25.4 N=49
Authoritativeness Difference Scores	Evidence	- .9	11.4	- .4	9.2
	No Evidence	-1.6	9.2	-1.1	1.6
Character Difference Scores	Evidence	.5	5.9	1.7	5.6
	No Evidence	.1	5.6	1.4	.3

\*The higher the score the more favorable the attitude. The hypothetical neutral point on the scale is 24.0.



Examination of the authoritativeness difference scores indicated that the reason for the significant ethos effect was that the high ethos sources all lost some of their initial credibility, although this loss was never significant, and the low ethos sources generally gained a substantial measure of credibility over their initial rating. This gain was significant in every condition except the low ethos, no prior knowledge condition. The latter also explains the observed (and predicted) ABC interaction effect.

The significant effect for ethos observed in the analysis of the character difference scores was produced by shifts similar to those on the authoritativeness dimension. Although in all eight conditions the source gained some credibility over their initial ratings, this gain was significant in only three of the low ethos conditions. Again, the gain in the low ethos, no prior knowledge condition was non-significant.

Analyses of variance of the speech evaluation ratings produced significant effects favoring the evidence condition on the total evaluation scores and two of the individual items--good content-poor content and well supported-poorly supported. A significant effect favoring the prior knowledge condition was observed on the well supported-poorly supported item. Table 14 reports the obtained ratings for total evaluation and these two items. No significant differences attributable to any main effect or interaction were observed on the other four speech evaluation scale items. On the seven point scale the mean ratings on the other items were as follows: objective-subjective, 4.0; clear-confused, 6.3; good delivery-poor delivery, 6.2; organized-disorganized, 6.4.

Table 14

Results of Study XIII--Speech Evaluation Dependent Variables

Dependent Variable	Evidence Condition	Prior Knowledge		No Prior Knowledge	
		High Ethos	Low Ethos	High Ethos	Low Ethos
Total Score of Six Evaluation Scales*	Evidence	35.7	36.4	35.9	35.1
	No Evidence	34.5	35.1	33.9	33.3
Good Content-Poor Content Scale	Evidence	6.2	6.5	6.4	6.2
	No Evidence	6.1	6.0	6.1	5.6
Well Supported-Poorly Supported Scale	Evidence	6.3	6.7	6.4	6.0
	No Evidence	6.0	6.2	5.8	5.0

\*The score on each scale can range from 1 to 7. The total score can range from 6 to 42. The higher the score the more favorable the rating.



Although there was no significant ABC interaction effect observed on the total evaluation scores or the scores for the individual items which were effected by evidence and/or prior knowledge, in each case the condition receiving the lowest rating was the low ethos, no prior knowledge, no evidence condition. Thus there was some consistency between the results on these three dependent variables and the results on the three primary dependent variables. The differences in results, however, are important to note.

The inclusion of evidence did not consistently produce increased attitude change or perceived ethos on the part of the experimental Ss but these same Ss consistently rated the speech with evidence higher than the speech with no evidence. This same type of effect was noted in study IV (McCroskey, 1966a) and it was posited that there may be in our culture an "evidence expectancy" operating when people listen to a speech. The fact that study XIII was conducted at the very beginning of the term in which the Ss were studying speechmaking suggests that this effect is not produced by speech courses but rather has its origins elsewhere. Why this tendency is present and what ramifications it has for communicators are not clear.

### Conclusions

Study XIII was designed to test the final suspected element that could have contaminated the results of the studies reported in Section III. It was posited that if Ss were aware of evidence before it was presented by a source, the evidence might not have a significant impact on their attitudes on the topic or their perceptions of the source. The results of this study suggest that this indeed may be the case. Replication with other Ss and other topics is needed before we can be certain, but at this point in time we are more justified in assuming that this effect exists when building a theory of the role of evidence in persuasive communication than in assuming that it does not exist. In the following section a theory of the role of evidence in persuasive communication is developed which assumes the results of the present study would be typical of future studies which would manipulate the variable of prior knowledge of the audience.

VI

THE ROLE OF EVIDENCE IN PERSUASIVE COMMUNICATION

We may conclude from the studies discussed in this report that evidence, operationally defined as factual statements or opinions originating from a source other than the speaker, may not be particularly useful to many speakers on many topics with many audiences. The studies herein reported point to the probability that evidence is useful for moderate to low ethos communicators with good delivery when that evidence is not already familiar to the audience addressed. The studies also point to the probability that inclusion of evidence will have little immediate effect if the source is perceived as highly credible, if the source has poor delivery, or if the audience is already familiar with the evidence that is included in the message or similar evidence.

The above statements are not consistent with the theoretical role of evidence in persuasive communication set forth in most textbooks on public speaking and argumentation. A revision of the traditional theory is, therefore, very much in order. To develop a revised theory of the role of evidence in persuasive communication it is necessary first to establish a common base of understanding. We will begin with defining the nature of the basic persuasive unit--the argument.

A Model of a Persuasive Unit

Persuasion may be described as the process of relating new beliefs to beliefs already held by an audience in such a manner as to gain audience acceptance of these new beliefs. A single persuasive unit, frequently referred to as an argument, consists of two elements accepted by an audience which, when related to each other, produce audience acceptance of a new element. Employing the terminology of Stephen Toulmin (1958) we may label this new element the communicator's claim, the new belief the communicator hopes his audience will accept.

We may label the other two elements in the persuasive unit data and warrant. Data consist of one or more specific beliefs accepted by an audience. We will suggest that there are three types of data and discuss them in some detail below. A warrant is a general belief held by an audience which relates the data to the communicator's claim. Such general beliefs may be concerned with substantive relationships of things in the external world, values held by the audience, or the audience's perception (ethos) of the source of the argument.

All arguments include data, warrant, and claim--either stated or implied. The relationships among the elements is exemplified diagrammatically in our modified Toulmin models in Figures 4-6.

Figure 4: Argument with Warrant Based on Values or Motives of Audience

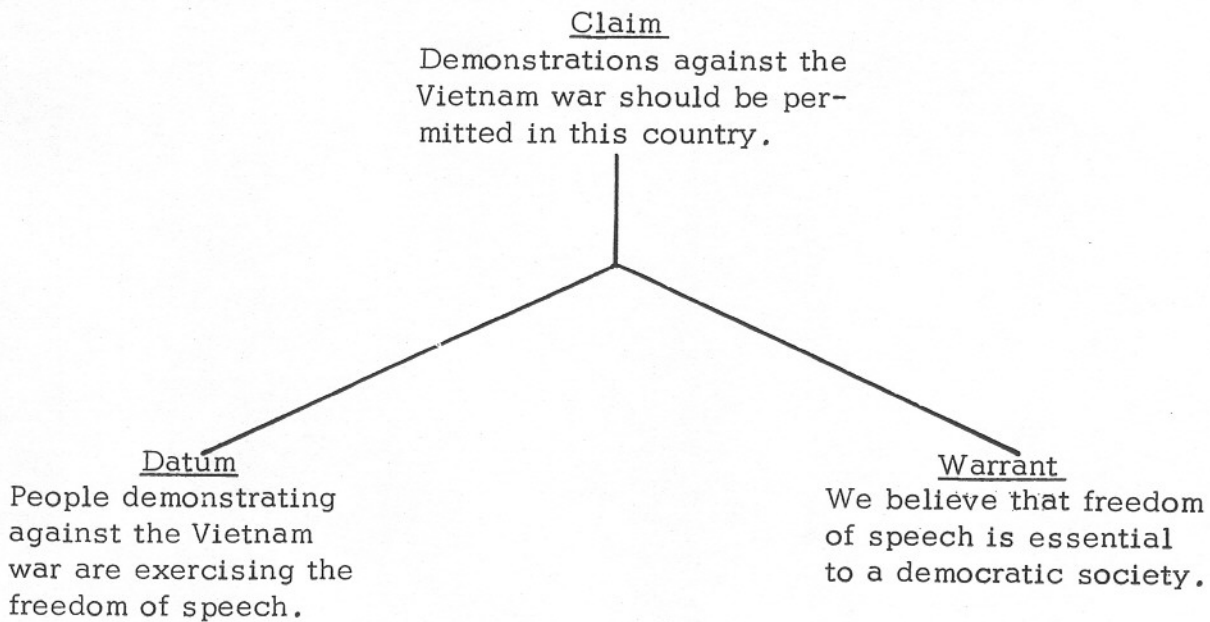


Figure 5: Argument with Warrant Based on Substantive (Logical) Relationships

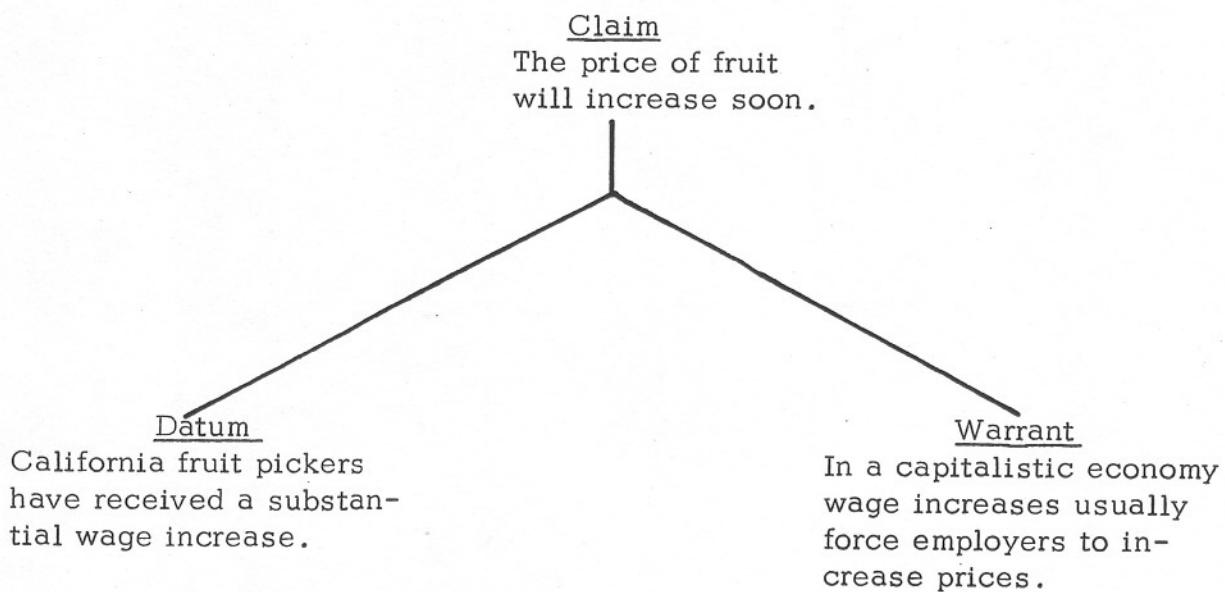
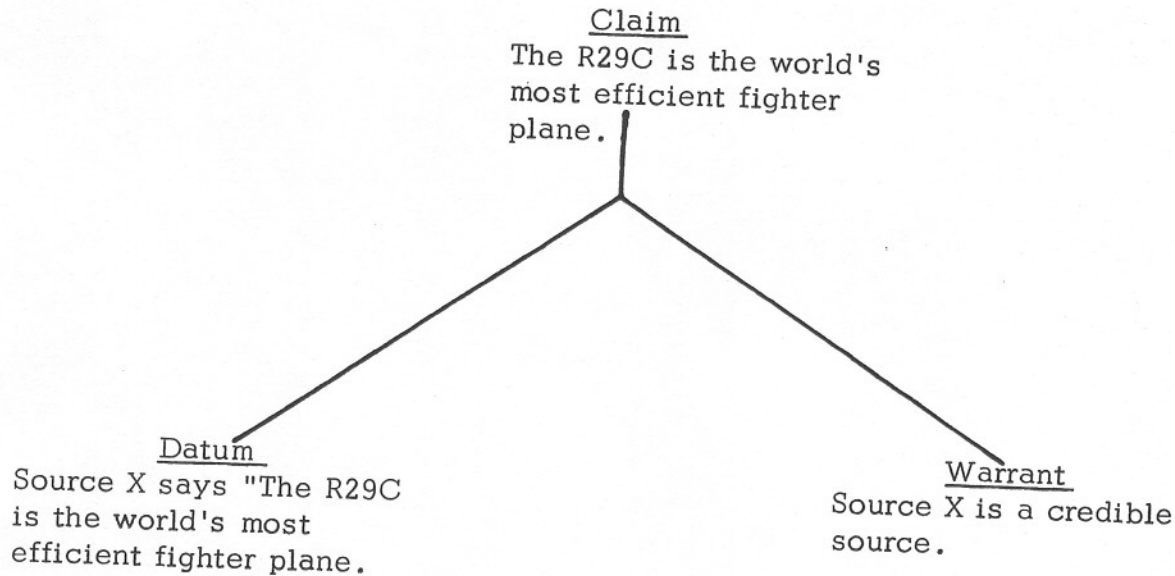


Figure 6: Argument with Warrant Based on Credibility of Source of Data



Data and warrant should be considered coordinate and indispensable parts of the process of gaining acceptance of claims. They are the support upon which the claim rests. If either is not believed or if they appear unrelated to each other, the persuasive process will be disrupted. The acceptance of the claim for which the persuasive unit was created will not occur; and any subsequent claims which are dependent on the completion of this persuasive unit will also be prevented from gaining acceptance.

The types of claims and warrants are set forth and discussed in detail elsewhere (Ehninger and Brockriede, 1963; McCroskey, 1968) and need not be further considered here. For the purposes of generating a theory of the role of evidence in persuasive communication, the nature of data is the crucial concern.

Since data is defined as evidence in most discussions of the nature of argument, evidence becomes one of the three indispensable elements of a persuasive unit as we have described it above. However, the results of experimental investigations have clearly indicated that in many instances evidence is not indispensable. In fact, it would appear from the studies reported here that more frequently than not evidence makes little or no contribution to the immediate success of a persuasive effort. If we consider the nature of data in a broader context, however, we can approach a viable theory of the role of evidence in persuasive communication.



### The Types of Data

There are three distinct types of data. We shall label them simply first-order data, second-order data, and third-order data. The first type is of the higher order, for it is ultimately the only type upon which a meaningful argument may be developed. One example of this type of data is audience opinion. If the audience believes that Negroes are inferior to whites, this opinion may be used by the communicator as data for an argument. He may not use the opinion that Negroes and whites are equal as data unless he first instills that opinion in his audience's minds. Thus, in any given persuasive circumstance a source is restricted in his choice of arguments by the data which he can find or implant in the belief system of his audience.

A second example of first-order data is audience knowledge. Anything that the audience knows can serve as potential data for an argument. If they are aware of wage increases for fruit pickers, this knowledge may be used by the persuader to obtain acceptance of the claim that price increases are likely in the fruit industry in the near future. If, however, the audience is unaware of such wage increases, the source is precluded from this data option until such time as he informs his audience of the wage increases and secures their belief in the fact that they occurred.

There is a narrow line between "knowledge" and "opinion." What is knowledge to one person may be opinion to another. We need not be concerned with this, however, because knowledge and opinion operate in almost exactly the same manner. If the audience "believes" or "knows" something, it can serve as first-order data. If they do not "believe" or "know" something, it cannot be used as first-order data.

Second-order data consists of opinions and information asserted by the communicator. This type of data is dependent on a secondary, usually implied, argument in every case. This secondary argument has as its data the asserted opinion or information. The warrant is based on the credibility of the communicator. An example of this is: "I say X's are usually Y" (data), "I am a credible source" (warrant), therefore "X's are usually Y" (claim). The datum in this secondary argument is first-order because as soon as the audience hears me say it they "believe" that I said it. The determinant of whether the claim is accepted or not is my ethos. If my ethos is high enough for the warrant to be acceptable to the audience, they probably will accept the claim. If my ethos is low, the claim will probably be rejected. This is the case whenever a source makes an assertion in a message. So long as his credibility is high enough there is a warrant that will permit his assertion to become audience opinion or knowledge and thus create first-order data. Thus, assertions of high credibility sources can produce data for further arguments while assertions of sources with low credibility serve no persuasive purpose. When a persuader makes an assertion the audience

immediately (though usually not consciously) completes the secondary argument. If the source's ethos is high enough the assertion is accepted by the audience and becomes either audience knowledge or audience opinion. At this point first-order data is present and the communicator can continue to develop further argument.

Third-order data consist of opinions of others and facts attested to by others. This type of data will be recognized as what we have defined as evidence in this report. As in the case of second-order data discussed above, the introduction of opinions of others or facts attested to by others immediately causes the audience to complete a secondary argument. This argument has as data the belief that the outside source made the statement attributed to it and the warrant is based on the ethos of the outside source.

A second supplementary argument is also produced. It goes something like this: The speaker says that so-and-so said X (data), the speaker is a credible source (warrant), therefore probably so-and-so did say X (claim). Obviously the communicator must have a certain minimal amount of credibility for even this argument to be accepted. But if it is, the data for the other secondary argument is established and new audience opinion or knowledge can be created. This new first-order data can then be used by the communicator to develop further argument.

The establishment of first-order data by means of third-order data (evidence), then, is dependent upon the credibility of the communicator. Is he at least honest enough to tell the truth about what others say? Thus, for the very low ethos source, evidence would serve no persuasive purpose. The audience would reject perfectly valid evidence because of the person presenting it. But if the communicator has the minimal ethos necessary to overcome this obstacle, the credibility of the outside source can become crucial. If the audience is unfamiliar with the outside source, they are less likely to accept the credibility based warrant. Thus, the advice given in public speaking and debate textbooks concerning citing and qualifying sources of evidence appears to be theoretically and practically justified, though not for precisely the reasons usually given in the textbooks.

#### The Theory Versus the Research

The theory set forth above suggests that there are three types of data, one or another of which must be present for an argument to be accepted by an audience. More specifically, the above theory suggests that if first-order data is present there is no need for second-order or third-order data or, if first-order data is not present, if second-order data is present there is no need for third-order data. How consistent is this theory with the results of the studies reported in previous sections?

The results of our research indicate that if the audience has prior knowledge of the evidence included in a speech the evidence has no significant impact. This finding demonstrates the preeminence of first-order data. The results of our research indicate that inclusion of evidence by a highly credible speaker has no impact. This finding is consistent with our theory that second-order data can preempt the impact of third-order data. Thus our two primary findings drawn from the research reported in previous sections are consistent with this theory. Only our findings that delivery interacts with the effect of evidence has not been integrated with this theory.

Delivery may relate to the impact of evidence in two ways. It may draw (or deter) attention to the content of the message or it may aid clarity (or cause confusion) of the message. In either case delivery may be said to operate as a permissive or restrictive element. Good delivery permits the functioning of evidence in circumstances where it has potential value; poor delivery prohibits the functioning of evidence in circumstances where it has potential value.

Our theory of the role of evidence in persuasive communication might lead one to conclude that evidence has a very small if not insignificant place in the overall scheme of things if it were not for one factor which we have not yet considered--the impact of evidence on sustained attitude change. While the results of our several studies point to a very restricted impact of evidence in producing immediate attitude change and enhanced source credibility, the results seem to suggest that evidence will have a desirable effect on sustained attitude change regardless of the ethos of the source, regardless of degree of prior knowledge of the audience, and regardless of the quality of delivery of the message. These results strongly suggest that a communicator should include evidence in his message whenever he seeks sustained attitude change even though it will probably have no immediate effect. The question is why evidence has this impact on sustained attitude change.

A possible explanation lies in the realm of an hypothesized "sleeping effect." This "sleeping effect" is not the one which is often posited in the psychological literature--such an effect was never observed in any of the studies discussed in this report. The usual "sleeping effect" is the circumstance where a stimulus condition (often low credibility) increases its impact over time. In all conditions in all of the studies where sustained attitude change was measured we found a decreased impact on attitude change over time.

The hypothesized "sleeping effect" here, then, is quite different from the one usually discussed. It seems reasonable to posit that including evidence reinforces beliefs already held by an audience even when it can serve no other useful function. The beliefs in their weaker form may be strong enough to permit the communicator to temporarily structure an argument



leading the audience to accept his claims, particularly if he is at least moderately credible in the eyes of his audience. However, as time passes the communicator and his message are forgotten. The links between old and new beliefs (data and claim) lose their strength. Thus, regression toward prior attitudes occurs. By employing evidence, however, the communicator may strengthen the audience's already held beliefs and therefore be able to produce links between these beliefs and the new ones he wants them to accept which are stronger and thus more resistant to regression. This explanation is highly speculative, of course, and needs to be thoroughly tested before we place much stock in it.

Whether we accept the above explanation of the reason why inclusion of evidence increases sustained attitude change or not, we are forced to recognize that indeed evidence does have a favorable impact on sustained attitude change. Evidence also has been shown to have impact on immediate attitude change and perceived source credibility when the communicator is perceived to be moderate-to-low in credibility but delivers his message well and the audience is not previously aware of the evidence he includes in his message. It is at least as important to note that in no case did we observe a decrease in either attitude change or source credibility as a result of including evidence in a message. Our overall conclusion, therefore, is that a wise communicator will include what can be called "third-order data," "evidence," or "documented supporting materials." Such material may not always help, but it certainly will not hurt.

#### The Need for Future Research

While the series of studies of the role of evidence in persuasive communication which embody this report are by far the most extensive to date, it could go without saying that we do not yet have the final answers to many, if any, pertinent questions regarding the functioning of evidence in persuasion. Additional studies in this area are in progress at Michigan State University, but many more need to be done. We will conclude this report, therefore, by posing several questions which might provide a spark in the mind of an interested researcher.

1. What is the effect of evidence on overt behavior change?
2. Can evidence from non-credible sources serve as well as evidence from credible sources under circumstances where first-order and second-order data are not available to the communicator?
3. What type of evidence (opinion, statistics, examples) produces the most favorable impact under circumstances where first-order and second-order data are not available to the communicator?
4. What factors interact with evidence in producing sustained attitude change?



5. What factors other than delivery interfere with the normal functioning of evidence?
6. Do non-students respond to evidence the same way as students?
7. Is evidence more or less important when discussing a highly salient topic than when discussing a relatively non-salient topic?
8. Do successful "real world" speakers employ evidence differently than unsuccessful speakers?

# REFERENCES

- Andersen, K. and Clevenger T., Jr. A summary of experimental research in ethos. Sp. Monogr., 1963, 30, 59-78.
- Anderson, D. C. The effect of various uses of authoritative testimony in persuasive speaking. Unpubl. thesis, Ohio State Univer., 1958.
- Arnold, W. E. and McCroskey, J. C. The credibility of reluctant testimony, Central States Sp. J., 1967, 18, 97-103.
- Baskerville, B. The illusion of proof. Western Sp., 1961, 25, 236-242.
- Berlo, D. K. and Gulley, H. E. Some determinants of the effect of oral communication in producing attitude change and learning. Sp. Monogr., 1957, 24, 10-20.
- Berlo, D. K., Lemert, J. B., and Mertz, R. J. Dimensions for evaluating the acceptability of message sources, Research monogr., Department of Communication, Michigan State Univer., 1966.
- Bettinghaus, E. P., Jr. The relative effect of the use of testimony in a persuasive speech upon the attitudes of listeners. Unpubl. thesis, Bradley Univer., 1953.
- Bettinghaus, E. P. Message preparation: the nature of proof. Indianapolis: Bobbs-Merrill, 1966.
- Brandes, P. D. Evidence and its use by selected United States senators. Unpubl. doctoral dissertation, Univer. of Wisconsin, 1953.
- Brandes, P. D. Evidence in Aristotle's Rhetoric, Sp. Monogr., 1961 28, 21-28.
- Cathcart, R. S. An experimental study of the relative effectiveness of four methods of presenting evidence. Sp. Monogr., 1955, 22, 227-233.
- Cathcart, R. S. An experimental study of the relative effectiveness of selected means of handling evidence in speeches of advocacy. Unpubl. doctoral dissertation, Northwestern Univer., 1953.
- Cooper, L. The rhetoric of Aristotle. New York: Appleton-Century-Crofts, 1932.

- Costely, D. L. An experimental study of the effectiveness of quantitative evidence in speeches of advocacy. Unpubl. thesis, Univer. of Oklahoma, 1958.
- Dresser, W. R. Studies of the effects of evidence: implications for forensics. A. F. A. Register, 1962, 10 (3), 14-19. (a)
- Dresser, W. R. Studies of the effects of satisfactory and unsatisfactory evidence in a speech of advocacy. Unpubl. doctoral dissertation, Northwestern Univer., 1962. (b)
- Dresser, W. R. Effects of "satisfactory" and "unsatisfactory" evidence in a speech of advocacy. Sp. Monogr., 1963, 30, 302-306.
- Dresser, W. R. The impact of evidence on decision making. Paper read at Sp. Assoc. Amer. Conv., New York, December, 1965.
- Edwards, A. L. Statistical methods for the behavioral sciences. New York: Holt, Rinehart & Winston, 1954.
- Edwards, A. L. Techniques of attitude scale construction. New York: Appleton-Centruy-Crofts, 1957.
- Edwards, A. L. Experimental design in psychological research. New York: Holt, Rinehart & Winston, 1960.
- Ehninger, D. and Brockriede, W. Decision by debate. New York: Dodd, Mead, 1963.
- Festinger, L. A theory of cognitive dissonance. Evanston: Row, Peterson, 1957.
- Gardiner, J. C. An experimental study of the use of selected forms of evidence in effecting attitude change. Unpubl. thesis, Univer. of Nebraska, 1966.
- Gilkinson, H., Paulson, S. F., and Sikkink, D. E. Effects of order and authority in an argumentative speech. Quart. J. Sp., 1954, 40, 183-192.
- Goetzinger, C. S., Jr. An analysis of the "validity" of reasoning and evidence in four major foreign policy speeches, 1950-51. Unpubl. thesis, Purdue Univer., 1952.

- Gregg, R. E. Some hypotheses for the study of the psychology of evidence. Paper read at Sp. Assoc. Amer. Conv., Chicago, December, 1964.
- Guilford, J. P. Psychometric methods. New York: McGraw-Hill, 1954.
- Holtzman, P. D. Confirmation of ethos as a confounding element in communication research. Sp. Monogr., 1966, 33, 464-466.
- McCroskey, J. C. Experimental studies of the effects of ethos and evidence in persuasive communication. Unpubl. doctoral dissertation, Pennsylvania State Univer., 1966. (a)
- McCroskey, J. C. Scales for the measurement of ethos. Sp. Monogr. 1966, 33, 65-72. (b)
- McCroskey, J. C. Toward an understanding of the importance of "evidence" in persuasive communication. Penna. Sp. Annual, 1966, 23, 65-71. (c)
- McCroskey, J. C. The effects of evidence in persuasive communication. Western Sp., 1967, 31, 189-199.
- McCroskey, J. C. An introduction to rhetorical communication. Englewood Cliffs, N. J.: Prentice-Hall, 1968.
- McCroskey, J. C. and Dunham, R. E. Ethos: a confounding element in communication research. Sp. Monogr., 1966, 33, 456-463.
- Mills, G. E. Reason in controversy. Boston: Allyn & Bacon, 1964.
- Osgood, C. E., Suci, G. J., and Tannenbaum, P. N. The measurement of meaning, Urbana, Ill.: Univer. of Illinois, 1957.
- Ostermeier, T. H. An experimental study on the type and frequency of reference as used by an unfamiliar source in a message and its effect upon perceived credibility and attitude change. Unpubl. doctoral dissertation, Michigan State University, 1966.
- Paulson, S. F. An experimental study of spoken communication: the effects of prestige of the speaker and acknowledgement of opposing arguments on audience retention and shift of opinion. Unpubl. doctoral dissertation, Univer. of Minnesota, 1952.
- Wagner, G. A. An experimental study of the relative effectiveness of varying amounts of evidence in a persuasive communication. Unpubl. thesis, Mississippi Southern Univer., 1958.



Walster, E., Aronson, E., and Abrahams, D. On increasing the persuasiveness of a low prestige communicator. J. Exp. Soc. Psychol., 1966, 2, 325-342.

Winer, B. J. Statistical principles in experimental design. New York: McGraw-Hill, 1962.