Journal of Personality and Social Psychology
1977, Vol. 35, No. 8, 548-569

In Search of Reliable Persuasion Effects: I. A Computer-Controlled Procedure for Studying Persuasion

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The research literature on persuasion is in a state of some disarray, at least in part due to widespread use of nonstandardized procedures for collecting data and of statistically weak (low power) designs. The present research employs (a) a computerized procedure as an aid to standardization and (b) within-subjects designs to obtain high power. This procedure has some resemblance to mass-media settings in which audiences are exposed to a large number of persuasive appeals in a short period. Data from eight experiments demonstrated the possibility of studying effects of traditionally interesting independent variables with this procedure. Most importantly, reliable evidence of a rapid decay of persuasion within a single experimental session was found for a subset of the communication topics that were used. This result indicated potential applications of the computer-controlled procedure to the study of variables that might be expected to influence the rate of decay of persuasion. However, the occurrence of rapid decay for only a subset of the communication topics necessitates caution in applying the procedure. Advantages and disadvantages of the reliance on within-subjects designs are considered.

Current State of Persuasion Research

Despite three decades of research, the state of knowledge on effects of persuasive communication is such that only the roles of a few major variables in the persuasion process are established with confidence, and the effects of many other variables that are widely assumed to be important in persuasion research remain to be established.[1]

How many variables are there for which persuasion effects are known confidently? Little more than the fingers of one hand is needed to count them: (a) source credibility (greater persuasion with more expert and trustworthy sources); (b) issue involvement (less persuasion for audiences who attach great importance to the issue); (c) time since communication (less persuasion the further an opinion measure is removed in time after an effective persuasive communication); (d) social pressure (more persuasion the greater the proportion of reference group

The research reported in this article was supported in large part by National Science Foundation Grants SOC 74-13436 and BNS 76-11175 (both titled "Research In Persuasive Communication"). The authors are grateful to several people for various types of help: Karl E. Rosenberg assisted generally in programing the NOVA computer; Rebecca A. Buehler wrote FORTRAN programs that controlled plotting of Figures 2 through 12 on Ohio State University's IBM 370/168 computer; Lewis Hinkle assisted with apparatus auxiliary to the NOVA computer; Donald Fischer, Glen W. Milligan, Richard J. Harris, Leonard S. Feldt, and Paul Feder provided statistical advice; and Jean A. Greenwald photographed Figure 1. Additionally, we thank the many colleagues who have commented on the work reported here when it was presented in pieces at colloquia and conventions.

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[1] Rather than render the next several paragraphs unintelligible by virtue of parenthetical references attached to every sentence, we refer the reader to excellent reviews of the persuasion literature by Insko (1967), Kiesler, Collins, and Miller (1969), McGuire(1969), and Fishbein and Ajzen (1975). Use of the indexes in those references should provide a guide to much of the literature that could be used to document the assertion made here.

members who espouse a discrepant opinion); (e) communication discrepancy (greater persuasion the more remote the communicated opinion is from audience initial opinion, but only if source credibility is high); (f) types of defenses (less persuasion when the audience has knowledge sufficient to refute attacking arguments as compared to knowledge that just supports the attacked opinion); (g) . . . ?

What are the presumably important variables for which persuasion effects are not adequately understood? Here the list runs longer: (*a*) communication medium (are messages more effective in print than with audio, audiovisual, or multimedia presentation?); (b) source attractiveness (is a more likeable person a more effective persuader?); (c) conclusion drawing (is a message more effective when it states its conclusion than when the audience is permitted to draw the conclusion?); (d) fear arousal (is it better to emphasize the dangerous consequences of not adopting a recommendation than the favorable consequences of adopting it?); (e) humor (is a communication that amuses more persuasive than one that gives the same arguments seriously?); (f) acknowledgment of opposing arguments (is it better to present and refute the opposition arguments than not to mention them at all?); (g) warning of impending communication (is persuasion reduced when the audience is forewarned of the attempt?); (h) pretesting prior to persuasion (do opinion pretests reduce subsequent persuasion?); (j) communication repetition (does repeating a message increase its impact?); (k) reward for behavior consistent with communication recommendations (does an incentive to act in accordance with the communication increase its persuasive effect?); (l) audience individual differences (are audience differences in persuasibility correlated with differences in age? sex? intelligence? self-esteem? anxiety? authoritarianism?). Of the variables mentioned in the preceding paragraph as having roles in the persuasion process that *are* generally understood, this understanding often does not extend so far as to include interactions among pairs (let alone larger combinations) of those variables.

Goals and Strategies

It is distressing that so much effort, contributed by many investigators and their research subjects over a period of more than three decades, has not yielded more knowledge. The research reported here was the beginning of an attempt to improve the efficiency and reliability of persuasion research. Our efforts were directed initially at two aspects of most contemporary persuasion research that seemed especially in need of improvement: (a) statistical power of experimental design and (b) standardization of procedures.

Use of within-subjects designs to increase power of persuasion experiments. Statistical power is the probability of obtaining a statistically significant result from an experiment, given a hypothesized magnitude of relationship among variables and a specified Type I error rate, α. Effects observed in persuasion experiments typically are small, corresponding to intertreatment differences of less than half the standard deviation of a dependent measure of opinion (Cohen, 1969). The number of subjects required for a between-subjects design to have good power to detect such modest effects is quite large—often prohibitively large unless data collection is inexpensive and subjects are plentiful. In contrast, the numbers of subjects required for within-subjects experiments to achieve comparable power are typically much smaller. Although within-subjects designs can, accordingly, reduce greatly the cost of obtaining a desired level of power, many considerations other than cost are appropriate to the choice between within- and between-subjects designs (see Greenwald, 1976, for a general comparison of within- and between-subjects designs). Later in this report, we attempt to evaluate the use of within-subjects designs for persuasion research, based on the present results and taking into consideration characteristics other than statistical power.

Use of a computer to standardize persuasion research procedures. Given the decision to employ within-subjects designs, it was necessary to develop a means of presenting multiple communications within a session and of obtaining opinion responses on communication and no-message control topics, while counterbalancing as many irrelevant variables as possible. These goals have previously been achieved in some persuasion research by using booklets containing the instructions and communications (e.g., Anderson & Farkas, 1973). The added virtues of using computerized control of the persuasion procedure were seen as (a) elimination of behavioral interaction between experimenter and subject, a possible source of artifactual findings, (b) counterbalancing by using a different experimental sequence for each subject, (c) automatic recording of opinion responses, eliminating any need for clerical transcription, (d) capacity to record unobtrusively the amount of time spent reading each communication, and (e) assurance that subjects could not retrace steps in the procedure by turning back to earlier booklet pages.

In summary to this point: We have argued that substantial benefits to persuasion research can result from widespread adoption of within-subjects designs and computerized

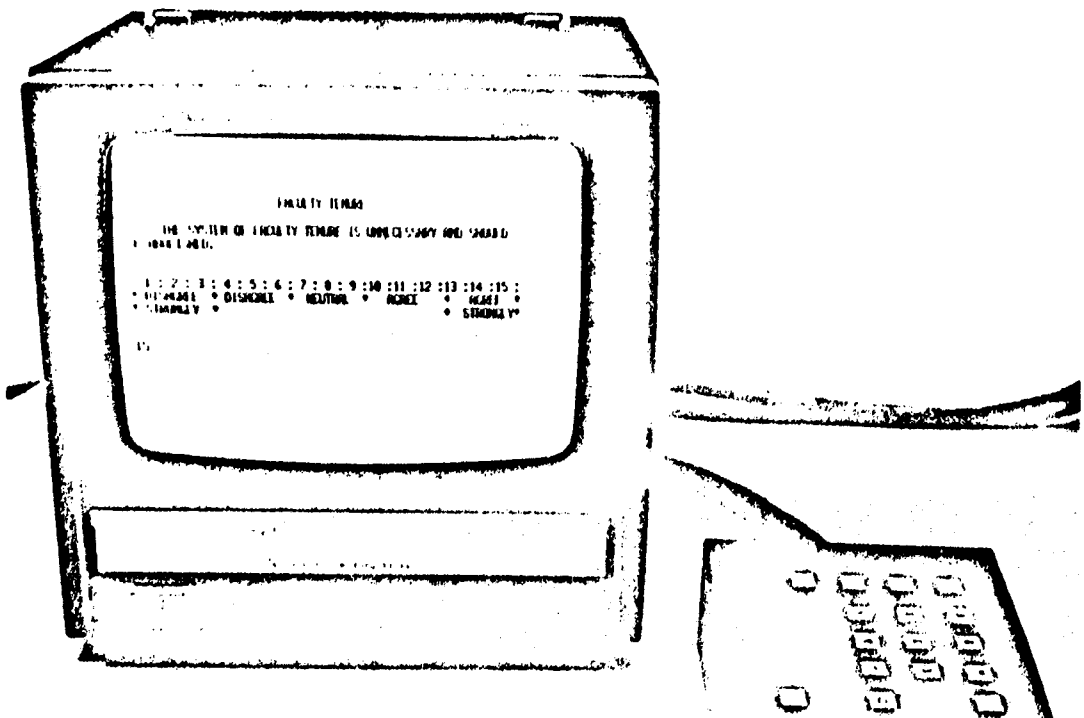


Figure 1. Subject's view of the equipment at each subject station, with video monitor displaying an opinion question and the response keyboard.

procedures. Two balancing remarks are in order: First, the procedures advocated are not as unusual as we make them appear. McGuire (e.g., Watts & McGuire, 1964) used within-subjects designs in several persuasion studies conducted in the 1960s; Anderson has recently developed and standardized a set of persuasive communications (president paragraphs—Anderson, Sawyers, & Farkas, 1972) and has used them in large-scale within-subjects experiments (e.g., Anderson & Farkas, 1973); computers have seen widespread application in other areas of psychological research. Second, although a large amount of data is reported here, it will become apparent that significant methodological problems remain. These problems will be noted in the Evalution and Discussion section.

Method

General Description of Computer-Based Procedure

The equipment used for the research included (a) a minicomputer, (b) a mass storage device that could accommodate libraries of persuasive communications and opinion questions, (c) four video display monitors located at satellite subject stations, (d) response keyboards on which the subject at each station could provide numerical responses or other information to the computer, and (e) an experimenter's console used to initiate and monitor operation of the experiment at the subject stations. The setting from the subject's perspective is shown in Figure 1.

Subjects. In the series of eight experiments, subjects were either unpaid undergraduate volunteers who were enrolled in introductory psychology courses or were paid respondents to classified advertisements in the university newspaper. (Some experiments employed subjects from both of these populations.) Subjects were recruited without regard to sex and were assigned randomly to a persuasion-reversal replication group (see below) and to an identification number within that group upon arrival at the laboratory. In the eight experiments, a total of 616 subjects provided data that were used in analyses. This was 87.1% of the total number of subjects who participated, subject losses being due to experimenter error, equipment malfunction, and failure to follow instructions. The rate of loss of subjects for the first two causes declined through the series of experiments, as the experimenters became increasingly familiar with the apparatus and developed precautions against accidental loss of data.

Timesharing of subjects at different stations. The programs written to control this series of experiments (Greenwald, Note 1) were designed to accommodate subjects running concurrently in different procedures. When a subject arrived at an appointed time for an experiment, the experimenter entered identifying information on the computer console. After minimal orienting comments by the experimenter, the subject was then seated in a chair in the selected subject station (a room about 2m square) and started reading the instructions. These arrangements permitted concurrent participation by up to four subjects, who could be in the same or different experiments, each receiving rapid response of the computer.

Computer function. Most of the computer's work during an experiment consisted of (a) rapidly locating text material (instructions, persuasive communications, or opinion items) and displaying it on the subject's monitor or (b) receiving and recording the subject's responses (opinion responses and reading times).

Instructions. All of the experiments were introduced by a standard set of orienting instructions. The key phrases in these instructions (a) described the purpose of the experiment as "to determine the effectiveness of a variety of communications that are intended for use in future experiments"; (b) explained that opinon questions would be asked on each of the topics for which messages were to be presented as well as on some topics for which no messages were presented; (c) noted that about half the messages would present information about U.S. presidents; (d) assured that opinion responses would remain anonymous; (e) advised the subjects to "read each message once, but make no attempt to study them or remember them since there is not sufficient time"; (f) instructed subjects to press an "enter" button to proceed after having finished reading any message "to your satisfaction"; (g) attempted to reduce demands to report opinion change by instructing, "do not make any assumptions about the accuracy of the messages. Judge them for yourself"; and (h) explained the use of the response keyboard to record opinion responses and provided practice in the keyboard's use.

Independent Variables Used in All Experiments

Topic type. Thirty-six pairs of persuasive messages were available for these experiments, ranging in length from 75 to 175 words. The topics of 18 of these message pairs concerned past presidents of the United States. Seventeen of the president message pairs were adapted from Anderson et al. (1972). The 18th pair of president messages and 18 other pairs of messages concerning contemporary issues were written by the investigators. Sample message pairs are given in Table 1. Of the 18 contemporary-issues topics, 14 had been selected from a group of 40 pairs that had been pilot tested to

Table 1
Sample Pair of Opposed Messages on a Contemporary Issues Topic

[For] Faculty Tenure

It has been suggested that the system of faculty tenure in universities should be abolished. Why, it is argued, should the professor have a form of job security that almost no other occupational group in our society receives? This argument overlooks the basic fact that tenure is justified, not as a form of job security, but rather as the basic means of preserving academic freedom in colleges and universities. Consider what would happen if a professor could be dismissed simply because of having unpopular ideas. Such a system would stifle open expression of ideas and would be unhealthy to the existence of a free and open democracy. Without a system of faculty tenure, academic freedom would likely become a meaningless phrase.

[Against] Faculty Tenure

Tenure for teachers and professors is outmoded and should be abolished. Just as any corporation needs to be able to dismiss workers and executives if their productivity falls, so do universities need to be able to divest themselves of faculty members who are unproductive in teaching and research. Tenure provides a very comfortable job security for the professor—so comfortable in fact that some, upon achieving tenure before 40, slack off in their attention to teaching duties and become 25-year parasites of the system.

Note. Sample president topic messages can be found in Anderson, Sawyers, and Farkas (1972).

identify opposed pairs of equally effective messages. The other 4 pairs were included without pilot testing. The contemporary-issues topics mostly concerned social-political issues (such as busing to achieve racial integration) or consumer questions (such as the hazards of aerosol sprays). In all of the experiments, topic type was employed as an independent variable, with each subject receiving equal numbers of messages and opinion measures concerning presidents and contemporary issues. The topic-type independent variable served an internal replication function, allowing some determination of the degree of generalizability of findings.

Measurement delay. After reading instructions, subjects received an extended sequence of persuasive messages intermixed with opinion questions. Measurement delays ranging from 0 to about 15 minutes were implemented by inserting various numbers of messages and opinion measures on other topics between the message and opinion measure for a given topic. Delay intervals were thus manipulated in terms of units of activity rather than in fixed time units. Reading of a persuasive message was arbitrarily assigned a value of one activity unit, while responding to an opinion question was assigned a value of one-half unit. (The relative values of these activities were assigned on the basis of prior observations that messages were read in an average of about 45 seconds, while opinion items were answered on the average in approximately 25 seconds.) With the exception of Experiment 3, an attempt was made to maintain homogeneous proportions of the two types of events (messages and opinion items) in delay intervals of all lengths.[2]

Control topics. With the exception of Experiment 7, in which presence or absence of pretest was employed as an independent variable, all experiments used after-only opinion measures. However, all subjects responded to opinion items on some topics without having received a message, and these responses provided control opinion levels against which to judge the magnitude of effects produced by the messages.

Other independent variables. Each of the eight experiments incorporated some independent variable(s) other than topic type, measurement delay, and message presence/absence. These additional independent variables are noted in connection with the presentation of results for the separate experiments.

Counterbalancing and Replication

Three types of procedures were used to minimize dependence of obtained results on particular assignments of message topics to measurement delay (or other) treatments or the sequence in which

[2] The possibility of manipulating delay intervals in fixed time units was sacrificed as a concomitant of the decision to allow subjects to proceed self-paced through the experimental procedure. The *only* advantage that we could see of using fixed exposure durations for messages and opinion items was the ability to specify time intervals precisely. Overriding counter-considerations were (a) the suspicion that persistence of persuasion was likely to be impaired not by passage of an interval of time as such, but by what was done during the interval, and (b) the expectation that the use of intervals designed to accommodate the slowest readers would render the procedure very boring to faster readers. Although there are many persuasion situations in which the message duration is fixed (e.g., radio and television advertising), persuasion via print media resembles our chosen procedure in that the reader is self-paced.

treatments were encountered in the experimental session. A counterbalancing procedure was directly controlled by the computer. This procedure had the effect of producing a Latin square design in which each topic appeared at each sequential treatment position once in a group of n subjects, where n was the total number of topics used in the experiment ($n = 36$). A limitation of this procedure was that treatments always occurred in the same order in the session for all subjects in a group. Two replication strategies were employed to reduce this confounding of treatments with sequential positions in the session. First, three of the experiments employed internal replications—the various within-subject treatments were repeated at two or more sequential positions within the experimental session, using a different sequence of treatments in each internal replication. Second, each experiment was conducted with two groups of subjects forming persuasion-reversal replications, each such group employing (a) a different assignment of treatments to sequential positions within the session and (b) a reversal of the direction of persuasion on each topic, achieved by using opposite-direction messages in the two replication groups. Statistical tests for interactions of independent variables (e.g., measurement delay) with the internal (within-subjects) or persuasion-reversal (between-groups) replication factors then provided tests of possible treatment-sequential position confounds. Rejection of the null hypothesis for such an interaction test would indicate that the sequential order of treatments influenced results.

Dependent Measures

Obtaining opinion responses. Single opinion items were used for each of the topics in an experiment. For president topics, the item was always stated in the form "_____ was an effective statesman and president," where the blank was replaced by the name of a past U.S. president. For contemporary-issues topics the item gave a one-sentence statement of the conclusion advocated in one of the pair of opposed messages for the topic (e.g., "The system of faculty tenure is unnecessary and should be abolished"). The 15-point response scale in either case was visually displayed as a horizontal scale showing the numbers 1 through 15 from left to right. Five subsets of three points each, starting at the left, were anchored "disagree strongly," "disagree," "neutral," "agree," and "agree strongly." Opinion responses were entered on the keyboard and shown on the display screen. Subjects were permitted to change a response that had been entered in error or to change their minds after starting the entry of a response.

Scoring opinion responses for subsequent analysis. During the experiment, the computer program reversed scores on opinion items so that a score of 15 always indicated maximum agreement with the position advocated in the message that was used in the subject's persuasion-reversal replication group. (Only in the case of two primacy-recency experiments, Experiments 1 and 6, was it possible for messages that opposed one another to be used in a single replication group. For these experiments, opinion responses were always scored so that high numbers reflected agreement with the message that was used as the more recent of an opposed pair in the replication.) In reporting data for presentation in figures (below) a constant has been subtracted from all opinion scores in each experiment in order to set means for no-message control treatments equal to 0.00. As a consequence of this adjustment, positive numerical means indicate mean differences from control levels in the persuaded direction (or in the direction advocated in the more recent of the opposed messages in the two primacy-recency experiments).

Reading time. Midway through the series of experiments, the automatic recording of time spent reading persuasive messages was incorporated into the computerized procedure. This measure was of interest only in Experiments 4 and 5 and is reported below just for those experiments.

Notes on Data Analysis

Characteristics of designs. Analyses of variance were computed under the assumption that subjects and topics (within topic type) were random-effect factors, while treatments and topic type were fixed-effect factors (cf. Winer, 1971). The Latin square design permitted univariate F tests (see just below) of a priori contrasts on the treatment effects and of the interactions of these contrasts with topic type and with replications. For these F tests to be unbiased, it was necessary to assume as negligible the various interactions among the three variables involved in the Latin squares (subjects, topics, and treatments). Because the validity of these assumptions cannot be assessed from the data, it cannot be assured that the resulting F ratios are unbiased, nor is it clear whether the most plausible biases are of a conservative (Type-II-error-favoring) or nonconservative (Type-I-error-favoring) nature. Accordingly, the reader is advised to interpret cautiously the F tests that are reported. These same considerations have led to a strategy of presenting almost all results graphically. The visual display of patterns of results is likely to provide a better sense of the magnitude and import of the findings than will the F ratios.

Use of univariate F tests. Most hypotheses were formulated as single-degree-of freedom contrasts and accordingly tested by univariate F tests, despite the overall repeated measures design. Thus, a multilevel factor such as measurement delay was typically partitioned into orthogonal polynomial components (e.g., linear trend of measurement delay). Consequently, issues involving biases in F ratios in repeated measures designs (cf. Huynh & Feldt, 1970) or the selection of a criterion statistic for the mul-

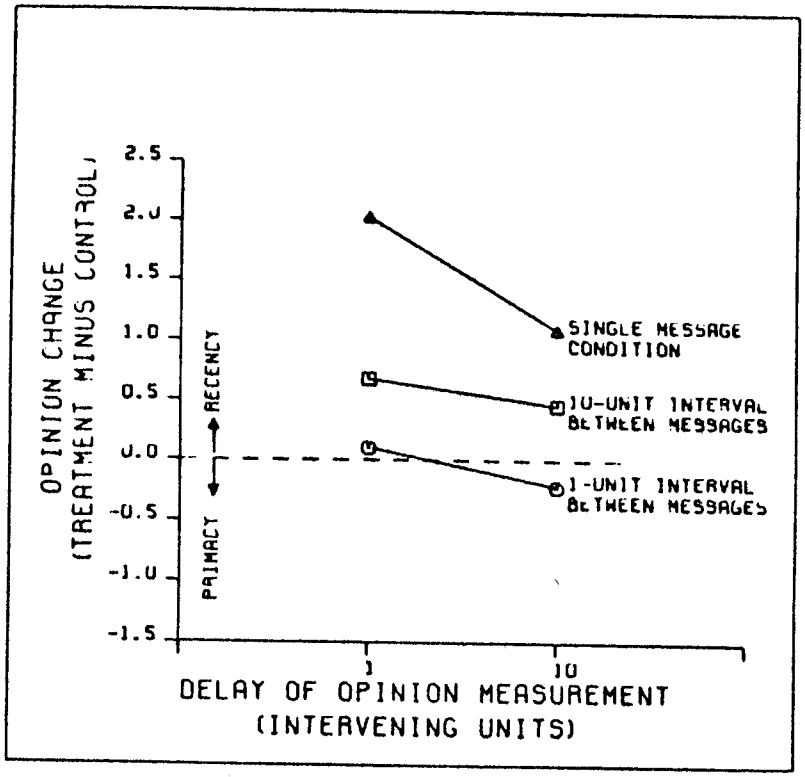


Figure 2. Experiment 1 results: Opinion change toward the more recent message as a function of measurement delay and con–pro message interval.

tivariate analysis of variance (Olson, 1976) were tangential to the present applications of repeated measures designs.

Experiments and Results

Experiment 1—Primacy and Recency

Miller and Campbell (1959) found that persuasive effects of pairs of opposing messages were dependent on both the interval between presentation of the opposing messages and the interval by which posttest opinion measurement was delayed following the second message. Their finding, more specifically, was that the second or more recent message (arbitrarily designated here as the *pro* message) had relatively greater effect (a) the longer the separation between the con and pro messages and (b) the shorter the interval between the pro message and the opinion measure. Similar results were obtained subsequently by Insko (1964). In these previous studies, the con–pro message interval varied from immediate succession to a 2-week separation, while the posttest opinion measurement delay interval varied from immediate to 1 week.

The first experiment in the present series sought to replicate these primacy–recency findings in a single session with maximum intervals of less than 10 minutes. Two levels of each of the two interval independent variables were used: A short interval was either 1 or 1.5 units; a long interval was approximately 10 units. (See the discussion of measurement delay units in the Method section—a 1-unit interval could have been filled either with reading one irrelevant message or answering two irrelevant opinion items.)

Two persuasion-reversal replication groups of 28 subjects each provided data, each subject therefore providing data for 28 topics. Sixteen of the 28 topics defined a 2 × 2 × 2 × 2 factorial design in which the factors

were con-pro message interval (short vs. long), measurement delay following the pro message (short vs. long), topic type (president vs. contemporary issues), and internal replication (the design of the preceding three factors was replicated separately in the first half and the second half of the experimental session). Eight additional topics defined a subsidiary $2 \times 2 \times 2$ design in which only the pro message for each topic was employed. The factors of this latter design were opinion measurement delay (short vs. long), topic type, and internal replication. The remaining four topics (two president and two contemporary-issues topics) for each subject were used to obtain no-message control opinion levels.

The results are presented in Figure 2. The findings were partially consistent with expectations based on the results of the Miller and Campbell (1959) and Insko (1964) studies. In particular, recency was found to be more likely with the long con-pro message interval than with the short interval, $F(1, 54) = 6.65$, $p < .02$. Although recency was also more likely (as expected) with the short than the long measurement delay interval, this effect was not significant, $F(1, 54) = 1.07$. Single message conditions showed substantial decay in persuasive impact when the measurement delay was long as opposed to short, $F(1, 54) = 8.70$, $p < .01$.

The results given in Figure 2 were stable over topic type, internal replication, and between-groups persuasion-reversal replication factors. Altogether, the results were encouraging in regard to the possibility of successful application of the computer-controlled procedure to problems of persistence of persuasion, since (a) substantial decay in persuasive impact was found for intervals that averaged under 10 minutes in clock time, and (b) the pattern of data resembled that of results obtained previously only with much longer time intervals.

Experiment 2—Decay of Impact

Although Experiment 1 demonstrated substantial decay of impact in a short time interval, only two measurement delay points were employed. A variety of curves of decay of impact could be consistent with these data, including linear and nonlinear monotonic as well as nonmonotonic functions of measurement delay. Experiment 2 was designed to assess the shape of the measurement delay function with greater precision by including seven delay intervals.

Two persuasion-reversal replication groups of 32 subjects each provided data. Each subject provided data for 32 topics in a $2 \times 8 \times 2$ design in which the three factors were topic type (president vs. contemporary issues), measurement delay (0, 1, 2, 4, 8, 12, or 20 units, plus a no-message control) and internal replication (first vs. second half of session).

The results are shown in Figure 3, and are given separately for president and contemporary-issues topics because topic type was the source of an important interaction effect. Specifically, the Topic Type × Measurement Delay interaction was significant, $F(6, 372) = 4.16$, $p < .001$. When the president topics were examined separately, measurement delay accounted for a highly significant effect, most of which could be identified with a linear decreasing component, $F(1, 62) = 36.60$, $p < .001$. (A much smaller quadratic component was also significant, $F(1, 62) = 9.03$, $p < .01$, indicating some deceleration of the decay of impact for president topics.) In contrast, an analysis of the contemporary-issues topics alone indicated no significant effect of measurement delay ($F < 1$).

This experiment provided the first indication that topic differences could be a substantial source of systematic variance in the computer-controlled procedure. The nature of topic differences will be considered further after pertinent data from other experiments are presented.

Experiment 3—Effects of Varying the Material Read During the Measurement Delay Interval

Experiments 1 and 2 demonstrated substantial decay of impact, at least for president topics, yet we were unsure about what

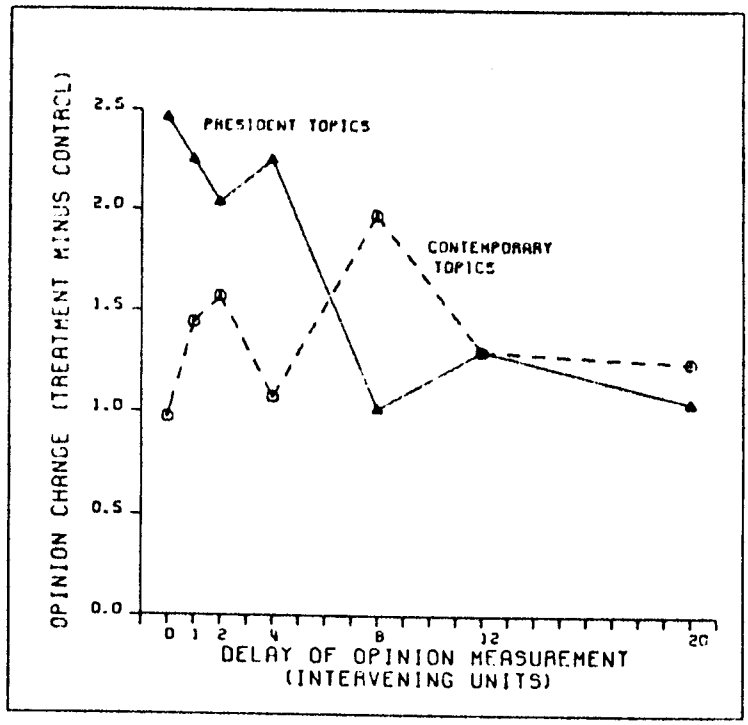


Figure 3. Experiment 2 results: Opinion change as a function of measurement delay separately for president and contemporary-issues topics.

aspect(s) of these topics or of the procedure in which they were employed were responsible for the marked decay. One possibility was an interference hypothesis—persuasive effects of messages suffered interference due to the subject's encountering a number of similar messages during the measurement delay interval. Accordingly, Experiment 3 was planned to vary the similarity between material encountered in the delay interval and the persuasive messages. The intervening material could be either similar (other persuasive messages) or contrasting (entertaining prose).

Thirty-six subjects in each of two persuasion-reversal replication groups provided data for 36 topics. In part, the design was a 2 × 5 × 2 factorial, the three factors being topic type (president vs. contemporary issues), delay interval length and type (0 units, 6 units-entertainment, 6 units-persuasion, 12 units-entertainment, 12 units-persuasion), and internal replication. Additionally, each subject provided no-message control opinion data for each of 4 president and 4 contemporary-issues topics. The 8 remaining topics were used to provide interval-filling messages and were not included in analyses.

The entertaining prose material consisted of either a science fiction (Clarke, 1966), mystery (Christie, 1973), or humorous (DeVries, 1967) short story, each of which had been edited to a length of 51 paragraphs. During initial instructions, the three stories were described and each subject was permitted to choose (by pressing a keyboard button) one of the three. During the experimental session the subject read the 51 paragraphs of the selected story in order, but with interruptions as required to include the persuasive messages and opinion measures. Measurement delay intervals could not be filled purely with material of a single type (entertainment vs. persuasion), since it would then not have been possible to meet all the design requirements. Rather, persuasion intervals (either 6 or 12 units) consisted of 5/6 persuasion material (messages and/or opinion items) and 1/6 story material, while

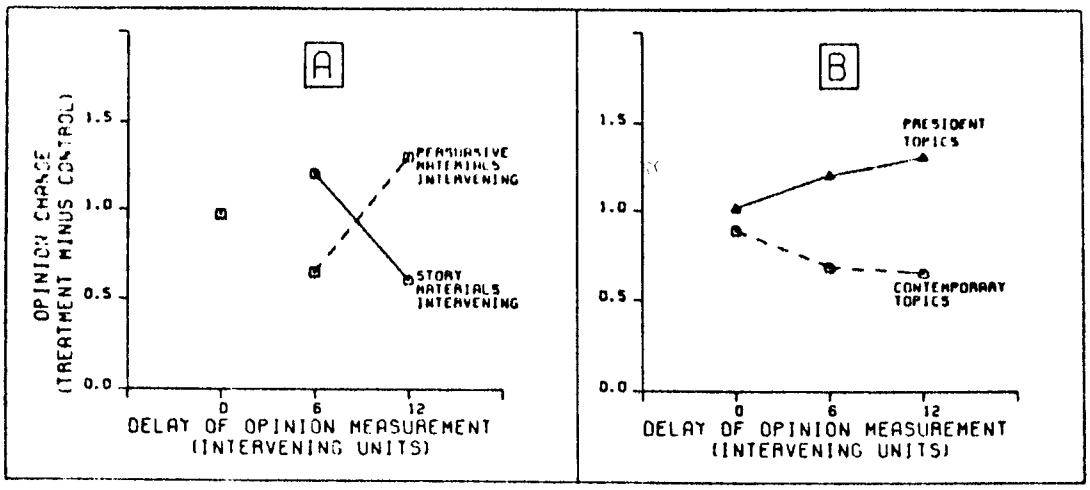


Figure 4. Experiment 3 results: (A) Opinion change as a function of type of intervening material and measurement delay; (B) Opinion change as a function of measurement delay shown separately for president and contemporary-issues topics.

entertainment intervals consisted of 2/3 story material and 1/3 persuasion material.

The results, as shown in Figure 4, conformed to an unexpected and (to date) uninterpretable pattern. For delayed opinion tests, there was a significant interaction between amount of delay and type of intervening material, shown in Figure 4A, $F(1, 70) = 8.14$, $p < .01$. From the interference hypothesis we expected that persuasive materials in the delay interval might result in more rapid decay of persuasion than would the entertaining story material, but this pattern did not appear. On the other hand, if the content of the filler material played no role, then decay functions should have resembled those found in Experiment 2, being different for the two types of topic, but not differing as a function of the type of interval-filling material. This also was not found. The overall pattern of measurement-delay effects for the two types of topics, combined over different types of filler material, is shown in Figure 4B, for which analyses yielded no significant effects. Noticeably lacking was the decay-of-impact effect that was expected (at least for president topics) on the basis of Experiments 1 and 2.

It was not possible to draw any particular conclusions about the interference hypothesis that inspired Experiment 3. However, the results did raise questions about the within-subjects procedure. The use of entertaining material within the session may have provided a context that altered the effects of independent variables as they have been observed in the different (all persuasion) contexts of Experiments 1 and 2. This may therefore be the type of context effect in within-subjects designs that Poulton (1973) has cautioned against; it will be considered further in the Evaluation and Discussion section.

Experiment 4—An Attempt to Manipulate Message Credibility

Having reached a temporary cul-de-sac in the attempt to examine effects of varied intervening material, we retreated to the apparent safety of an all-persuasion context. Our goal was to determine whether the credibility attributed to a message might affect the shape of the persistence-of-persuasion function. In particular, we sought a possible "sleeper effect"—delayed increase in impact of a persuasive message (cf. Hovland, Lumsdaine, & Sheffield, 1949). Although it has been difficult recently to demonstrate sleeper effects in standard laboratory persuasion procedures (Gillig & Greenwald, 1974), we thought it would be worthwhile to search

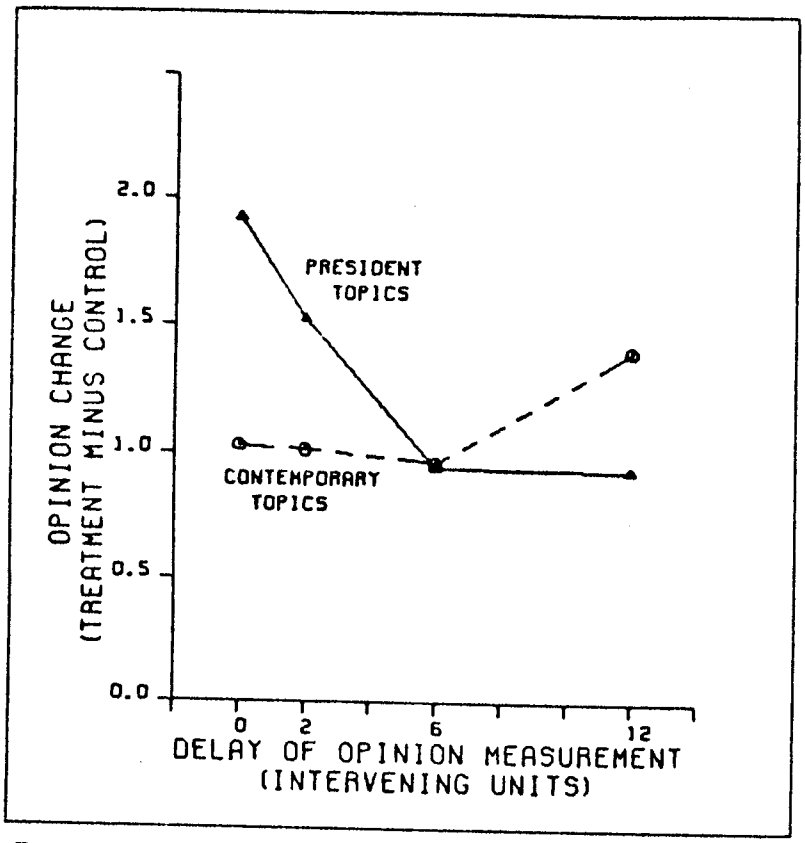


Figure 5. Experiment 4 opinion results: Opinion change as a function of topic type and measurement delay, collapsed over levels of source credibility.

again for such an effect in the computer-controlled single-session procedure.

Attributed message credibility was varied in a way that seemed appropriate for the computerized presentation, albeit a method not used in previous research. Prior to presentation of each message, the subject received and read a one-line statement that provided information about the proportion of prior subjects who had agreed, disagreed, or been neutral on the topic of the about-to-be-received persuasive message. Low, moderate, or high levels of attributed credibility were associated with information indicating majority disagreement, equal proportions of agreement and disagreement, or majority agreement.

The experiment was conducted with two persuasion-reversal replication groups of 32 subjects each. In addition to 8 no-message control topics, 24 topics defined a $2 \times 3 \times 4$ factorial design. The factors of the design were topic type, message credibility (three levels), and measurement delay (0, 2, 6, or 12 units). The number of topics available did not permit an additional internal replication factor in this design.

No effect of attributed message credibility was found. The persistence functions for president and contemporary-issues topics did differ significantly, as shown in Figure 5. The interaction of topic type with the linear component of measurement delay was significant, $F(1, 62) = 5.62$, $p < .03$, as in Experiment 2. As in Experiment 2, this effect again indicated that monotonic decay of impact occurred only for the president messages.

The lack of any effect of credibility in this experiment cannot be interpreted as calling into question the well-established relationship

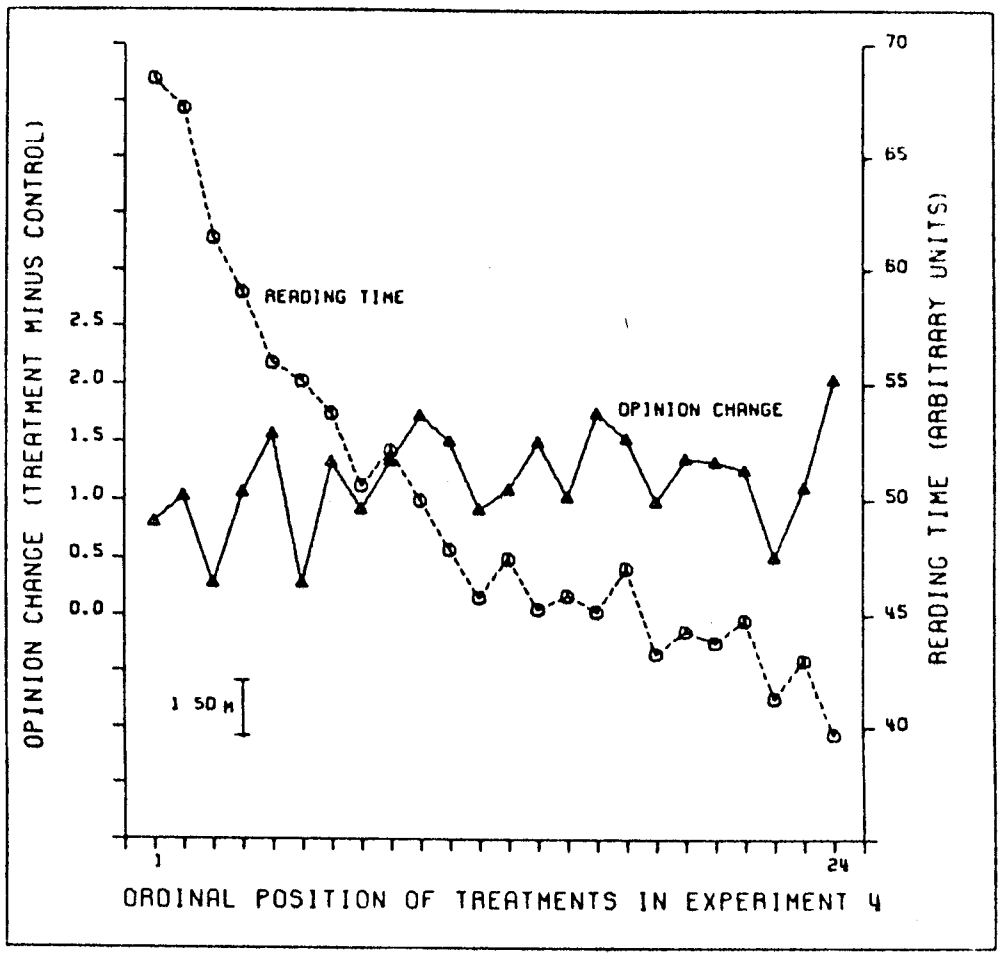


Figure 6. Experiment 4 results: Ordinal position analysis of opinion and reading time data. Reading times were adjusted for message differences, prior to analysis, by dividing each reading time by the mean reading time over all subjects for the same message, then multiplying by an arbitrary constant (50). The figure has been plotted so that the vertical distance shown in the lower left of the graph represents the average standard deviation of means on each measure.

of attributed credibility to message effectiveness. Rather, it was possible that our particular experimental situation made unworkable the type of manipulation of credibility that was employed. It was possible that (a) subjects refused to believe the (fabricated) information provided about extent of prior subjects' agreement with the messages, (b) they did not attend well to the information, or (c) they attended and believed but were not affected in the usual manner by the information. Postexperimental interviewing of a subset of the subjects did not provide a basis for choosing among these alternatives.

Effects of ordinal position of treatments. Because the major independent variable (credibility) failed to produce any effects, the data of Experiment 4 lent themselves to an analysis of the sequential order in which treatments were experienced. This is an important analysis, since each experiment in this series counterbalanced order of topics within only two different sequences of treatments. Reliable sequential effects could therefore prove a serious limitation. There were 24 treatments for which messages were presented in Experiment 4. Figure 6 gives means for opinions and for reading times—a de-

pendent variable used for the first time in this experiment—as a function of the order of occurrence of messages for the 24 treatments. It is apparent that reading times were strongly dependent on ordinal position of messages in the experiment, while persuasive impact was not. A very large proportion of variance in reading times, 79%, was accounted for by the linear trend component of ordinal position, for which $F(1, 62) = 113.29$, $p < .001$; another 9% and 2% of reading time variance were associated with statistically significant quadratic and cubic trend components of reading times, respectively, with no other trend components significant. In contrast, a nonsignificant linear component, for which $F(1, 62) = 3.01$, $p < .09$, accounted for only 4% of the variance of the opinion measure.

In conclusion, regarding the ordinal position analyses: Ordinal effects were virtually nonexistent for the opinion measure, and therefore such effects posed no apparent problem for interpretation; for reading times the effect of position was strong and well defined, allowing the possibility of correcting for it in analyses, as will be demonstrated in Experiment 5.

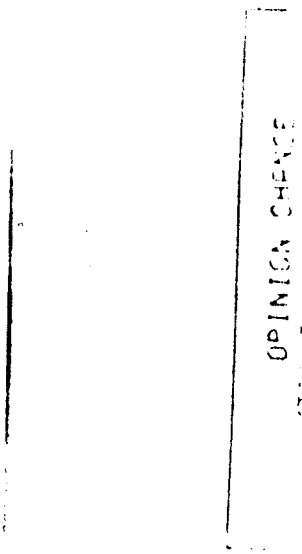
Experiment 5—Effects of Message Repetition and Spacing

Retreating from a second cul-de-sac, we chose to explore an independent variable for which the computerized procedure seemed ideal—message repetition. Johnson and Watkins (1971) found no greater immediate persuasion by a message presented five times versus once, but found that the persuasion induced by five repetitions persisted significantly more strongly over a 4-week interval than did that induced by the same message encountered just once. We sought to replicate this effect within a single experimental session.

Two persuasion-reversal replication groups of 36 subjects each provided data. Thirty-six communication topics were used for each subject, 30 of these defining a $2 \times 5 \times 3$ factorial design. The factors of this design were topic type, message condition (single, two massed, two spaced, three massed, or three spaced presentations), and measurement delay (0, 2, or 10 units). For the two- and three-message treatments, "massed" refers to repetitions being separated by 2 units, while "spaced" indicates their being separated by 10 units. In all cases measurement delay was the interval between the last (or only) presentation and the opinion measure. Six additional (three president and three contemporary issues) topics were used to obtain no-message control opinion data.

The results, as shown in Figure 7, fit closely with expectations based on the Johnson and Watkins (1971) findings. Although there was no effect of number of presentations on immediate (0 unit delay) posttest opinions, $F(1, 70) < 1$, there was greater persistence of persuasion for repeated than for once-presented messages. For the interaction of single versus multiple presentation with the linear component of measurement delay, $F(1, 70) = 5.08$, $p < .03$.

The reading time data were adjusted, prior to analysis, by sequentially: dividing each reading time by the mean reading time over all subjects for the same message (to eliminate differences due to message length), multiplying by an arbitrary constant, and raising each score to the 5/8 power (to eliminate mean-variance correlation). Inspection of these data showed that subjects spent much more time reading first presentations than second or third presentations of the same message. Excluding first presentation data, we computed a regression equation to predict (adjusted) reading times from (a) ordinal position of message within session, (b) the square of ordinal position, (c) presentation number (two or three), (d) distribution (massed or spaced), (e) the interactions among the preceding four variables, and (f) the main effects of the remaining variables (subjects, topic type, and replications). The significant and psychologically interesting results of this analysis are summarized in the lower set of four curves in Figure 8, and were as follows: Reading time was found to decrease as the experiment progressed: ordinal position, $F(1, 1644) = 59.1$, $p < .001$; but this decrease decelerated during the ses-



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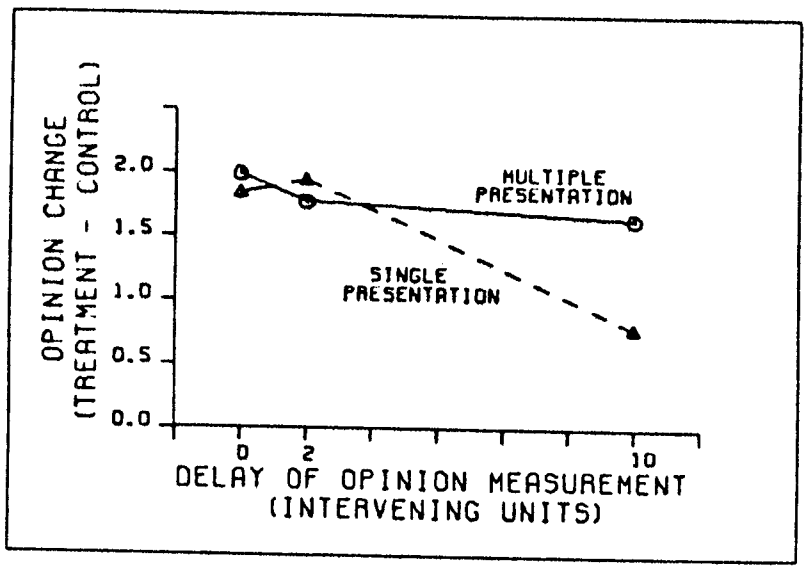

Figure 7. Experiment 5 opinion results: Opinion change as a function of measurement delay and single versus multiple message presentation.

sion: square of ordinal position, $F(1, 1644) = 48.7$, $p < .001$. Reading time was greater for second than for third presentations, $F(1, 1644) = 34.9$, $p < .001$. Also, reading time was greater for spaced than for massed presentation of repetitions, $F(1, 1644) = 20.6$, $p < .001$, but this effect of distribution was weaker at the end of the session than at the beginning: Ordinal Position × Distribution, $F(1, 1644) = 9.66$, $p < .002$. Because treatments were partly confounded with ordinal position, and because the analysis could not correct completely for this confound, the latter three (relatively small) effects should be interpreted with caution.

The results of this experiment were encouraging in regard to use of the computer-controlled procedure for studying repeated presentation effects in persuasion. The apparent sensitivity of the reading-time measure to variations in number and spacing of message repetitions suggests its possible future use as an unobtrusive measure of message retention. If possible, however, future research with this measure should use greater counterbalancing of the sequence of treatment conditions rather than the possibly biased statistical correction for sequence we have employed.

Experiment 6—A Further Investigation of Primacy-Recency

In considering the results of Experiments 2 and 4, we felt there was reason to question the replicability of the primacy recency findings that had been obtained in Experiment 1. The problem, particularly, was that the more recent experiments had given indication that a monotonic decay of persuasion (of the sort originally assumed by Miller & Campbell, 1959) could be obtained reliably only for the president topics. If so, findings that appeared to confirm those of Miller and Campbell should have been reliable in Experiment 1 only for the president topics. Accordingly, in this experiment more levels of the measurement delay factors were employed (much as Insko, 1964, had done in replicating Miller & Campbell), in order to permit more exacting hypothesis tests.

The design employed two persuasion-reversal replication groups of 36 subjects each. Only 32 of the 36 topics employed for each subject participated in the experimental design, the other four being fillers. Twenty-four of the topics constituted a 3 × 4 × 2 design, the factors being con-pro message interval (0 units, 10 units, or no con message

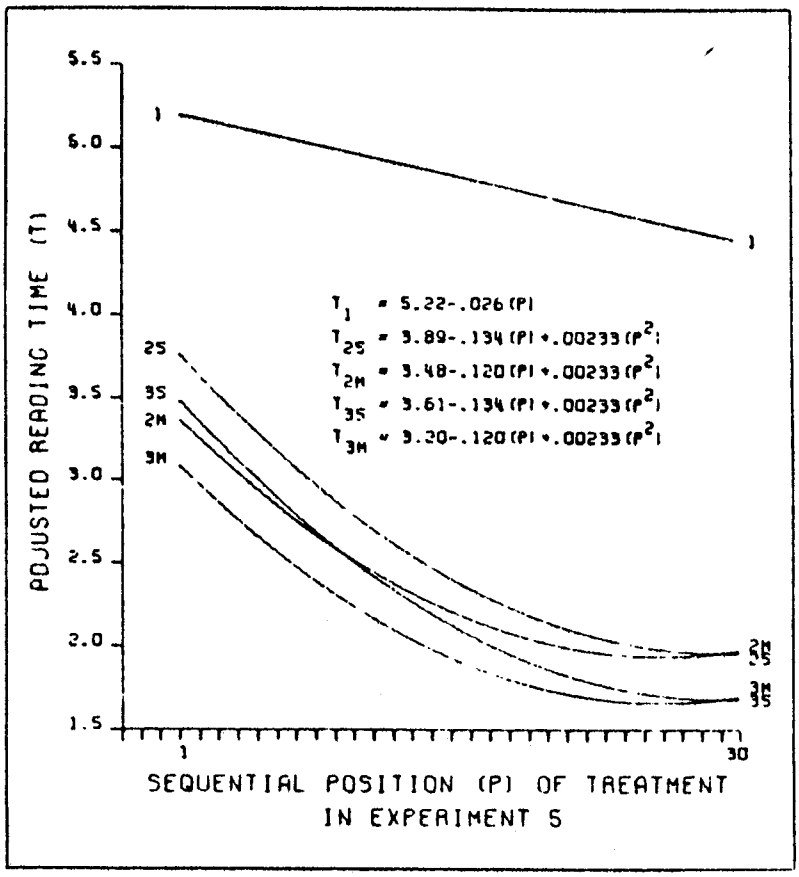

Figure 8. Experiment 5 adjusted message reading times as a function of ordinal position of messages. For clarity, the figure shows computed best-fitting functions rather than actual data points. Separate curves are plotted to describe significant effects obtained in the regression analysis described in the text. Labels designate first, second, or third presentation and spacing (S) or massing (M) of repetitions.

—i.e., pro message only); measurement delay following the pro message (0, 2, 6, or 12 units); and topic type. Each subject also provided no-message control opinion data for 8 other (4 president and 4 contemporary-issues) topics.

Figure 9 shows the effect of measurement delay in the pro-message only conditions, separately by topic type. The shape of functions apparent there was consistent with the expectation that only president topics would manifest monotonic decay of persuasion; however, the interaction effect of topic type with measurement delay was not significant, $F(1, 70) = 1.65$, $p < .25$, for Topic Type × Linear Trend of Measurement Delay. This lack of significance is not disturbing, however, since the test was low in power, with each subject contributing only one observation to each mean plotted in Figure 9.

Examination of the primacy–recency segment of the design showed, as expected (even though not found in Experiment 1), an interaction of topic type and interval between opposing messages, $F(1, 70) = 6.70$, $p < .02$. The results are presented in Figure 10 in a fashion that allows comparison with the findings of Experiment 1 (see Figure 2), including only the two measurement delay intervals that were closest to those used in Experiment 1. The president topics yielded, as expected, the closer approximation to the results of Experiment 1. In particular, the finding for president topics of a primacy effect

at short measurement delay with the long pro–con message interval (but not with the short interval) was similar to Experiment 1, as well as to the results of the previous experiments by Miller and Campbell (1959) and Insko (1964). The decline in recency with longer measurement delay, expected on the basis of the original Miller-Campbell analysis, was found for the president topics for the long pro con message interval, but not for the short interval: $F(1, 70) = 8.30$, $p < .01$, for the interaction of Pro-Con Interval × 2 levels of Measurement Delay. The results for contemporary issues (see Figure 10, right panel) conformed in no significant respect to the expectations based on the Miller-Campbell analysis. While the specific pattern shown for contemporary issues in Figure 10 was not predicted, still it was not surprising given the unusual (nonmonotonic) persuasion decay functions found in this experiment (see Figure 9), and previously in Experiments 2 and 4, for the contemporary issues topics.

In summary, the expectation that topic type would make a difference in a primacy recency design was amply confirmed by the results of Experiment 6. Unfortunately, we were left with no ready explanation for the lack of comparable interactions involving topic type in Experiment 1. This problem with the topic-type variable is central to the overall appraisal of this research offered in the Evaluation and Discussion section.

Experiment 7—Effects of Pretesting Opinion on Impact of Persuasive Communications

Experiments 1–6 relied entirely on after-only designs, in which no subject provided an opinion judgment more than once on any topic. The after-only procedure allows inferences about opinion changes by comparing opinion responses on each topic between

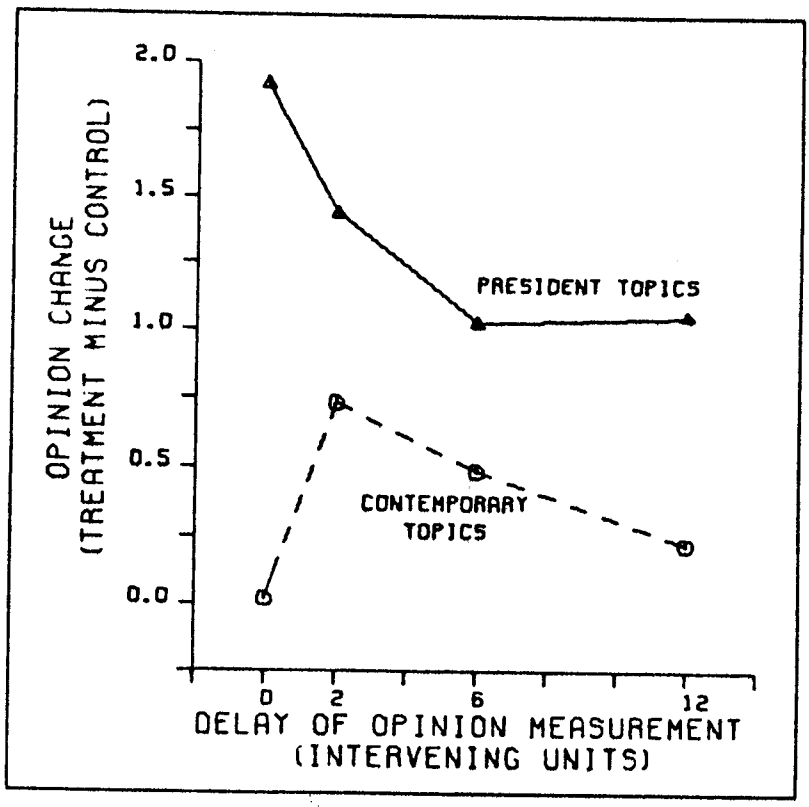

Figure 9. Experiment 6 results: Opinion change as a function of topic type and measurement delay in the pro-message-only conditions.

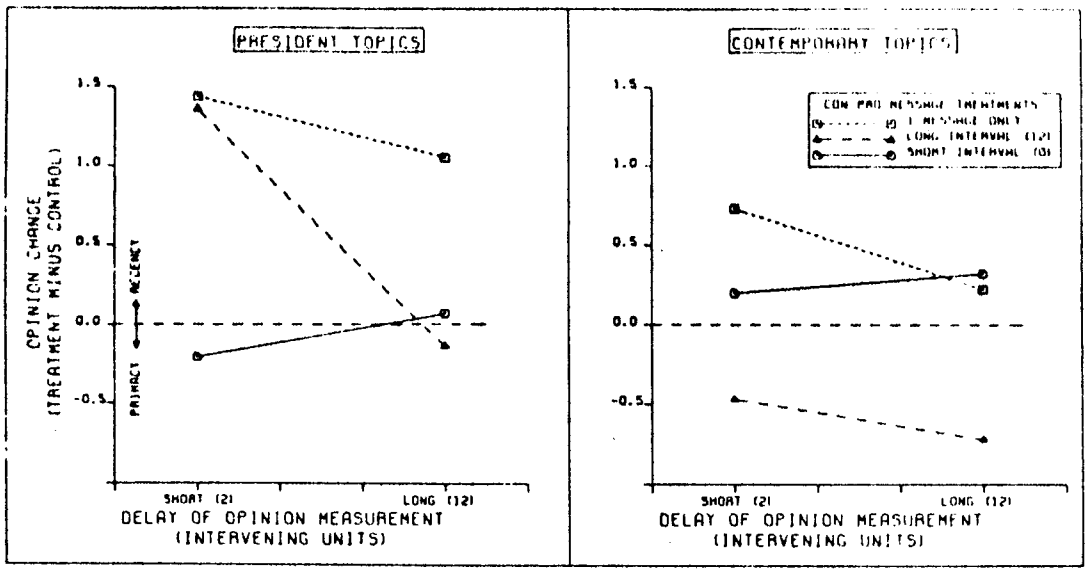

Figure 10. Experiment 6 results: Opinion change toward the more recent of a pair of opposing messages as a function of topic type and treatment intervals, presented to permit comparison with Experiment 1.

message treatments and no-message control treatments. It is possible to infer change more directly by comparing responses made by the same subject before and after receiving a message. However, researchers are often reluctant to employ before–after designs because of the possibility that pretesting may affect responsiveness to persuasive communications artifactually. This concern is widely shared even though most evidence shows no effect of pretesting (Lana, 1969). Experiment 7 explored the effect of opinion pre-

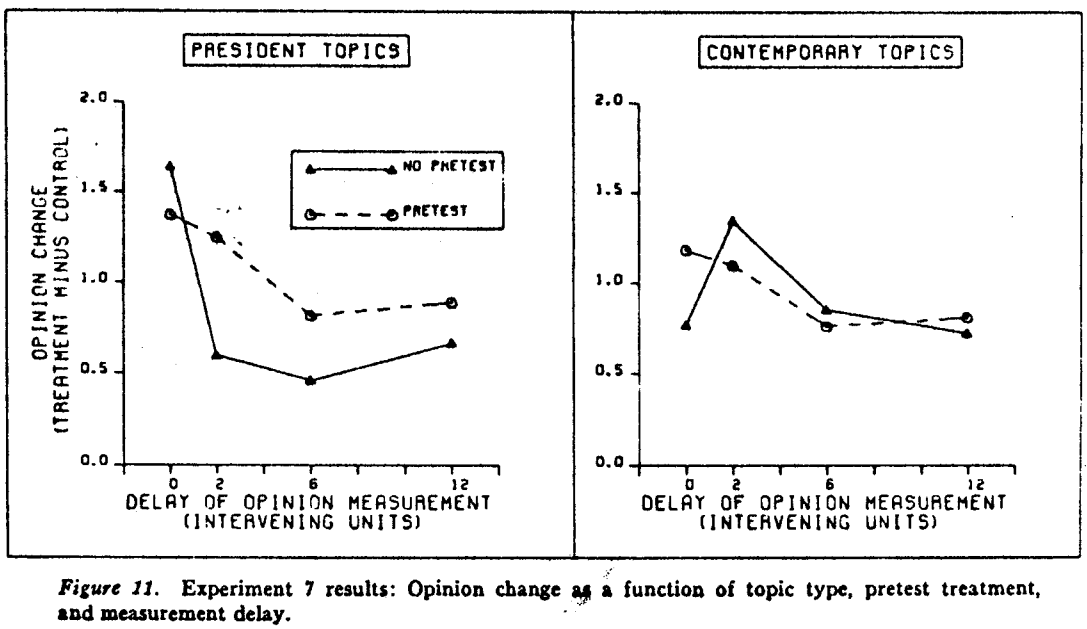

Figure 11. Experiment 7 results: Opinion change as a function of topic type, pretest treatment, and measurement delay.

tests administered at varying intervals prior to exposure to a persuasive communication.

Thirty-six subjects participated in each of two persuasion-reversal replications. Of the 36 topics used for each subject, 24 constituted a 2 × 3 × 4 factorial design. The factors of this design were topic type, pretest condition (pretest either 0 units or 12 units prior to the message, or no pretest), and posttest measurement delay (0, 2, 6, or 12 units). The 12 remaining topics for each subject provided various control observations. Four of these topics (2 president and 2 contemporary issues) were used for each of three types of control conditions: (a) no message with test–retest interval of 1 unit, (b) no message with test–retest interval of 12 units, and (c) message with two posttests, the first immediate (0 units) and the second after 13 units.

No differential effects on posttest opinion due to presence versus absence of pretest, or of interval between pretest and message, were observed either immediately following the message or at the various measurement delay intervals. The results are given in Figure 11, with data for the two pretest interval conditions (0 or 12 units prior to the message) combined, since these yielded no differential effects. The traditional suspicion that pretests may reduce persuasive impact is not at all supported by Figure 11.

Experiment 7 was quite encouraging in regard to the possibility of conducting pretest–posttest designs in the computer-controlled procedure. To determine further the practical consequences of using pretests, we compared the sensitivity to opinion change of pretest–posttest differences with that of posttest-control differences in our procedure. Using all the data of Experiment 7, we estimated the magnitude of such differences that could be detected as statistically significant ($\alpha = .05$) with probability (power) = .90. The results of these computations indicated that, with 72 subjects, each contributing just one pair of scores on a single treatment, a true pretest–posttest difference of 1.14 scale points on the average could be detected with .90 probability. In comparison, when the pair of scores were from an after-only posttest and a control topic, the true difference that was detectable at .90 probability was 1.75 scale points. The 35% [(1.75 − 1.14)/1.75] improvement in sensitivity is potentially a considerable factor in practical application of the computer-controlled procedure to the testing of hypotheses about changes in opinions.[8]

Experiments 8A and 8B—Warning of Impending Persuasion

The last experiment in the present series was conducted as another attempt to demonstrate, with the computer-controlled procedure, a finding that has appeared in several experiments that used more traditional persuasion research procedures. This was the finding that, after warning of an impending persuasive communication, opinion shifts are observed, usually toward the position of the expected communication. Other researchers have suggested that this effect may be due to self-esteem protection (McGuire & Millman, 1965) or moderation of position toward neutrality when the impending message is counterattitudinal (Hass, 1975). The present experiment assessed the effects of warning both on opinion directly and on the persuasive impact of the ensuing communication.

Two experiments were conducted, each employing 72 subjects in two persuasion-reversal replication groups of 36 subjects each (a total of 144 subjects). The designs of the two experiments were the same; only the manner of providing the warning information differed. Of the 36 topics employed for each subject, 18 constituted a 2 × 3 × 3 factorial design, the factors being topic type, forewarning condition (no warning or warning either 0 or 12 units prior to the message), and postmessage measurement delay (0, 4, or 12 units). An additional 8 topics formed a 2 × 4 design for which factors were topic type and measurement delay following warning (0, 2, 6, or 12 units). For the latter

[8] Separate computations showed that the sensitivity gain due to before–after measurement for president topics (37%) was slightly greater than that for the contemporary issues (32%).

design, no messages were presented, so that any opinion effects could be due only to the warning itself. Finally, 10 topics (5 of each type) were used to obtain no-message/no-warning control data.

In Experiment 8A, the warning consisted of a one-sentence statement of the conclusion of the impending message, presented alone on the subject's display monitor (e.g., "James Monroe was an effective statesman and president" or "Faculty tenure is outmoded and should be abolished"). Subjects were instructed that this meant that a message with the stated conclusion would later be presented. In Experiment 8B, the one-sentence warning explicitly referred to an impending communication (e.g., "Later you will receive a message arguing that James Monroe was an effective statesman and president").

The patterns of results for the two experiments were closely similar, as a consequence of which only combined data are presented in Figure 12. The most interesting aspect of the Figure 12 data is the small yet quite reliable postwarning opinion shift in the direction specified in the warning. For the warning anticipatory effect, $F(1, 140) = 16.32$, $p < .001$, combined over postwarning measurement delays. No temporal trend was apparent across the four measurement delays for these anticipatory shifts. The data for postmessage persuasion as a function of warning contained a suggestion that warning increased immediately measured persuasion, but this was not a reliable effect. The monotonic decrease in persuasion after receipt of messages, combined over warning and no-warning conditions, was significant, $F(1, 140) = 13.99$, $p < .001$, and there was no interaction of postmessage measurement delay and topic type for the combined data of the two experiments.

In sum, the computer-controlled procedure yielded results that were quite consistent with prior findings of anticipatory shifts due to warnings of impending persuasion.

Evaluation and Discussion

The strategy of this series of eight experiments was one of probing a large and unknown domain in a variety of directions. This strategy was consistent with the primary aim of evaluating the methodological inno-

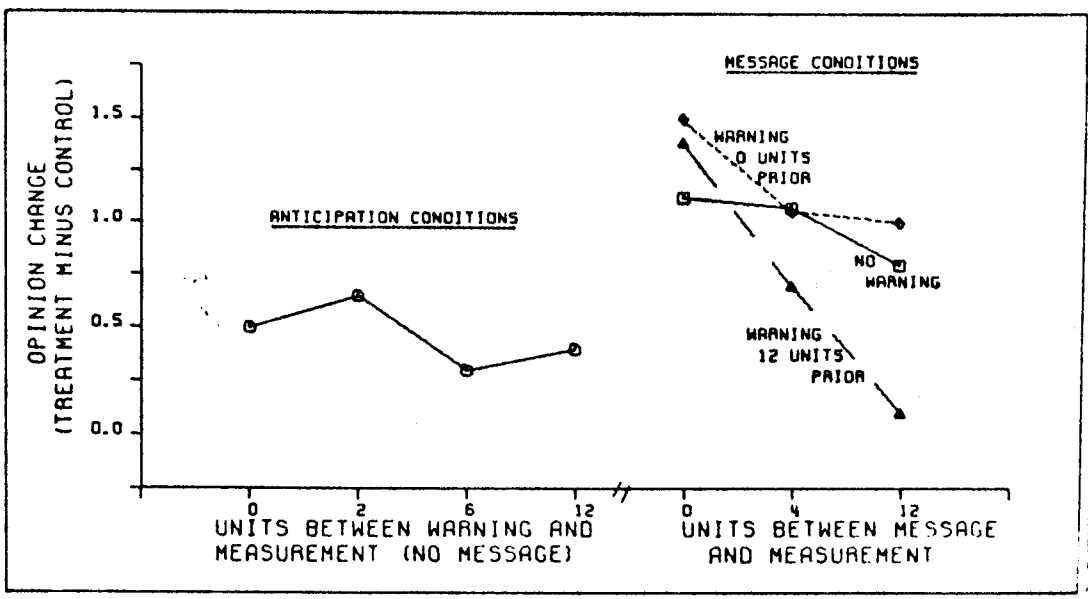

Figure 12. Results for Experiments 8A and 8B combined: Opinion change as a function of measurement delay following warning only (left side) and measurement delay following a persuasive message (right side, shown separately for the two warning treatments and the no-warning treatment).

vations incorporated in this research. With eight experiments completed, it is now appropriate to attempt an evaluation of the computerized procedure for persuasion research.

Social Aspects of Data Collection

The computerized method assures a standardized experimental procedure for each subject. Further, it minimizes interaction of the subject with a human experimenter. These characteristics are responsible for a desirably high degree of situational control and replicability and an assurance that possible sources of experimenter bias are minimized. This should not, however, be read as a conclusion that computerized research is free of experimental artifacts. Although no data have been collected specifically to test this hunch, throughout the series of eight experiments we have regarded it as plausible that subjects are concerned about how their responses in the experimental situation may be interpreted as reflecting on themselves. In other words, although the relationship of experimenter to subject is mediated by the computer, that relationship nonetheless exists; the computer provides no assurance that an experimental situation is free of demand characteristics.

One of our initial concerns was that a procedure involving a long succession of persuasive messages and opinion measures might prove to be boring, and if so, subjects might provide imprecise data because of low levels of motivation. Gratifyingly, this concern was unfounded. The computer was effective in commanding the attention of subjects. This may have been due in part to the rapid responsiveness of the equipment to the subject. Immediate visual feedback was provided for any response made by the subject, and new messages or opinion items were presented at a very rapid rate, in excess of 1,000 characters per second, when the subject was ready for them. We suspect that if we had used a nonresponsive procedure—such as one that enforced fixed intervals of message reading or fixed times to generate opinion responses—the subjects might indeed have become bored.

Advantages and Disadvantages of Within-Subjects Designs

Power. The power advantage of within-subjects designs has been noted earlier in this manuscript. The sensitivity of the present procedures to small effects (e.g., the anticipatory shift effect in Experiments 8A and 8B) bears out this expectation of relatively high levels of statistical power.

Context effects. Recent treatments (Greenwald, 1976; Poulton, 1973) have provided reminders that the power gain of a within-subjects design may be purchased at the cost of altering the phenomenon being studied. That is, results observed in a within-subjects design may be dependent on the context provided by other treatments in the design. In the present experiments, there have been some indications of context effects that are potentially troublesome. In particular, (a) the nature of the function relating persuasion to posttest measurement delay seemed to be altered fundamentally in Experiment 3 when some of the delays were filled with entertaining prose rather than with persuasive messages, and (b) it was possible that the attributed message credibility manipulation failed in Experiment 4 partly as a consequence of the use of a within-subjects manipulation of credibility.

Nonetheless, several of the experimental independent variables acted very much as anticipated in the within-subjects designs. These included measurement delay (for president topics), spacing of sequenced, opposed messages (again, particularly for president topics), repetition of arguments, and warning of impending persuasion.

A cautious positive evaluation seems warranted: It may be possible with the computerized procedure to study a large number of independent variables as they affect the persuasion process, but it can be expected that the effects of some manipulations will be altered by the context of a within-subjects experiment, and experimenters must be alert to this possibility.

External validity. Did the within-subjects designs import sources of artifactual or uninteresting findings that might have mas-

queraded as effects of the conceptual variables being investigated? A partial answer to this question came from tests for interactions of the effects of the conceptually relevant *treatment* factors (measurement delay, warning, repetition, etc.) with irrelevant *procedural* factors (internal replication and persuasion-reversal replication). These interaction tests were computed by multivariate analysis of variance in which the dependent variable set included opinion data for all the treatment conditions of conceptual interest in each experiment. The internal replication factor, which appeared only in Experiments 1, 2, and 3, was involved in no significant multivariate interactions, while the persuasion-reversal replication factor (used in all eight experiments) yielded a significant ($p < .05$) multivariate interaction F ratio only once. From these analyses and the ordinal position analysis of Experiment 4, it seems that position in the experimental session and direction of persuasion were not sources of worrisome artifacts, at least for opinion data.

However, as was noted in connection with Experiments 4 and 5, ordinal position of treatments strongly influenced reading times for the persuasive messages. Inclusion of ordinal position as a predictor of reading time permitted estimation of the effects of substantive design factors on reading times. Nonetheless, it remains necessary for reading time results to be interpreted cautiously in a procedure such as ours, which partly confounded treatments with sequential position in the experiment.

A second component of the external validity question has to do with the extent to which the within-subjects procedure is representative of interesting nonlaboratory persuasion settings. The density and brevity of communications in this setting, and the impersonal character of the communication medium, are aspects of similarity to natural *mass media* persuasion settings. There is another common type of persuasion setting to which the computer-controlled procedure bears much less resemblance. In *interpersonal* persuasion settings, such as a debate or a sales negotiation, communications may be much more lengthy, only one topic may be considered in a substantial time period, and the medium of communication is highly personal. Accordingly, we are reluctant to apply results from the computer-controlled within-subjects experiments to the analysis of interpersonal persuasion. At the same time, the computer-controlled procedure may have greater similarity to mass media persuasion settings than do other laboratory procedures that involve fewer communications on fewer topics or greater interpersonal interaction of communicator with audience.

Single-Session Studies of Persistence of Persuasion?

With the exception of Experiment 3, monotonic decreasing persuasion as a function of measurement delay for president topics was found in all experiments. The contemporary-issues topics yielded persistence functions that were statistically indistinguishable from those for the president topics in five of the experiments (1, 3, 5, 7, and 8), but were significantly different in Experiments 2, 4, and 6. Collectively, the findings established that (a) decay of persuasion can be observed over quite short time intervals within a single-session experiment, and (b) differences in temporal trends of persistence as a function of topic type or treatments (e.g., the number-of-presentations treatment in Experiment 5) can be detected. Beyond this, the experiments contributed little to understanding of the determinants of persistence of persuasive impact. Nonetheless, continued use of the single-session procedure to study persistence of persuasion is certainly justified given the potential the method has for manipulating independent variables that should affect persistence and the capacity of the researcher to control the events that intervene between original persuasion and measurement of its impact.

Topic Selection in Persuasion Research

Our several findings of differences between effects for president and contemporary-issues topics have been a source of concern. A part of this concern is that we are obliged to limit the generalization of the findings. Much more troublesome is the fact that we cannot specify the dimensions of the limitations on

generalization. In particular, we would like to know what characteristics of topics are associated with rapid decay of persuasion in the computer-controlled procedure. Perhaps most plausible is the hypothesis that the greater similarity of the president topics, one to another, was responsible for a greater level of mutual interference among them. That is, subjects might have been persuaded immediately by a president-topic message, but the persuasive impact may have dissipated rapidly as similar messages about other presidents were heard and interfered with the impressions left by earlier ones.

It is impossible to do more than speculate about the critical decay-facilitating characteristics of the president topics because the topics employed in this research were not selected systematically. Therefore, a most important conclusion is that future research using the present procedure must be done with topics having better known characteristics. Topic selection for persuasion research might proceed by (a) defining a population from which topics can be selected and (b) selecting topics randomly (or by some other reproducible procedure) from the population so defined. When this has been done, it will be possible to generalize confidently from the topics sampled to the larger population of topics. The next report in this series will describe experiments, currently in progress, that employ topics selected in this systematic fashion.

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Received November 24, 1976 ■