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AN ACTIVATION HYPOTHESIS OF COGNITIVE BEHAVIOR

by

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I hereby recommend that this dissertation prepared under my direction by Javad Emami

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TABLE OF CONTENTS

TABLE OF CONTENTS--(Continued)

LIST OF TABLES

LIST OF TABLES--(Continued)

LIST OF FIGURES

ABSTRACT

Several "consistency" constructs have been proposed in social psychology to account for the intensity, direction, and modification of attitudes. Heider's "balance theory," Newcomb's "strain-toward-symmetry" hypothesis, Cartwright and Harary's "structural balance" theory, and Osgood and Tannenbaum's "congruity" hypothesis all assume "consistency" to be a desirable attitudinal state. Festinger's "theory of cognitive dissonance" is logically in line with the preceding conceptions. It postulates that inconsistency between pairs of "cognitive elements" (i.e., attitudes, beliefs, and opinions) results in dissonance which is psychologically noxious. Hence there is a drive toward reduction of dissonance and achievement of consonance.

Recent empirical research has provided data inconsistent with certain derivations from Festinger's theory. This study was undertaken to test the "activation hypothesis of cognitive behavior" which was proposed to provide an explanatory framework for the various cognitive data which cannot be unequivocally fitted into Festinger's model.

The "activation" hypothesis attempts to apply the concept of "optimal level of activation" to data in the cognitive realm. The fundamental proposition in this formulation is that cognitive activity

is seldom in a quiescent state. There is a constant flow of activity in the individual's cognitive system. When the flow of cognitive activity is experienced as either too congruent (homologous or monotonous) or too incongruent (heterologous or discrepant), the individual will tend to revert from both extreme conditions to an experientially optimal level of activity.

This research tested a basic derivation from the activation hypothesis: Induced states of cognitive congruence and incongruence will tend to result in <u>comparable</u> amounts of cognitive activity in the individual. For purposes of comparison the relevant prediction from Festinger's theory was also tested: Induced states of cognitive congruence and incongruence will tend to result in <u>more</u> cognitive activity in the individual in the incongruent (dissonant) than in the congruent condition.

There were three male and three female groups of subjects in this study: "consistent-information," "inconsistent-information," and "no-information" (control) groups. The independent variable consisted of feedback of information consistent or inconsistent with subject's verbalized judgments in a series of ten trials during which subject was instructed to count the number of geometric forms projected on a screen and announce his counts to the experimenter who verbally agreed or disagreed with subject's counts. The subject's number of relevant (to the experimental task) statements, number of words, number of word association responses, reaction time, and deviation from the correct counts of stimuli constituted the measures of cognitive activity.

It was found that subjects in the "inconsistent-information" and "control" groups, respectively, produced more cognitive activity than subjects in the "consistent-information" groups. The results do not support the "activation hypothesis." This was argued to be attributable to the possibly inadequate experimental design of the study in which the intensity of induced congruence and incongruence was non-equivalent. There is, however, partial support for Festinger's prediction if it is assumed that "control" subjects experienced "dissonance."

Since the obtained evidence supports Festinger's theory only partially and rather equivocally it was suggested that the gestalt notion of "closure" and the information theory notion of "certainty" can explain the results with more precision and economy.

The relation of the "activation hypothesis" to variables of "creativity" was discussed and it was suggested that because of its potential utility this conception may deserve further elaboration and research.

INTRODUCTION

Concern with the modification of attitudes, beliefs, and opinions has been one of the dominant spheres of theoretical and research interest in social psychology during the past few decades. Social psychologists have employed a vast array of conceptual schemes and methodological approaches in this area; their efforts, however, have not been outstandingly successful.

When Leon Festinger's A Theory of Cognitive Dissonance (1957) appeared many workers in this field hailed it as a major breakthrough. The author had proposed an engagingly simple and crisp theoretical scheme which purported to predict, with economy and precision, the outcome of certain types of intra-individual events in a matrix of cognitive parameters. The book also contained a number of experimental studies which confirmed some of the derivations from Festinger's theory.

The bipolar concepts of dissonance and consonance were advanced by Festinger to refer to relations that obtain between pairs of elements in the person's cognitive structure. A comprehensive treatment of the theory of cognitive dissonance will involve a description of its proper historical antecedents. To this we now turn.

An Historical Overview

The various constructs that deal with consistency are not novel. Nor are they peculiar to attitudes, opinions, behavior, or personality. Such constructs invariably are subsumed by all scientific and theological paradigms. Consistency underscores order, pattern, or structure on a universal level. Consistency in man is a sub-class of the concept of universal consistency. In this sense, the concept of consistency underlies human rationality since it implies that the individual's behavior and attitudes are not merely consistent to the objective observer: The individual also attempts to make them appear consistent to himself.

When William James (quoted in Berlyne, 1960, p. v) wrote, "The philosophic brain responds to an inconsistency or a gap in its knowledge, just as the musical brain responds to a discord in what it hears," he was presumably implying the significance of consistency. Yet, the means which men use to achieve consistency often betray a conspicuous lack of rationality. Quite often this phenomenal consistency or, by implication, rationality is achieved by drastically irrational methods. Freud's concept of rationalization seems to be a special case of this.

Festinger's notions of consonance and dissonance have several historical parallels in psychology. The first explicit formulation of consistency seems to go back to Heider's concept of balance

(1944 & 1946) which he further elaborated in a later book (1958). The subsequent and analogous constructs of "strain-toward-symmetry" (Newcomb, 1953), "congruity" (Osgood & Tannenbaum, 1955), and "structural balance" (Cartwright & Harary, 1956) all assume inconsistency to be a psychologically noxious state except that the level of generality implied by each of these conceptions varies somewhat (Zajonc, 1960).

Let us look at the logical progression and refinement of these concepts at closer range.

Theory of Balance

Heider is perhaps responsible for the earliest formalization of the consistency construct from within a gestalt framework (1946). He was concerned with the consistencies in the ways individuals perceive their interpersonal and environmental relations. His analysis was in terms of two persons, P (person) and O (another person), with P as the focus of analysis and with O as some other person and X which is the label for an object, an event, an impression, an idea, etc. Heider's conceptual schema was stated as such:

> Attitudes toward persons and causal unit formations influence each other. An attitude toward an event can alter the attitude toward the person who caused the event, and, if the attitudes toward a person and an event are similar, the event is easily ascribed to the person. A balanced configuration (italics added) exists if the attitudes toward the parts of a causal unit are similar (1946, p. 107).

Heider's intent was to describe how relations among P, O, and X are structured in P's cognitive matrix and whether P experiences recurrent and systematic tendencies in these relations. He postulated two types of relations: L or liking, and U, or unit relations (e.g., similarity, cause, possession, etc.). The proposition was advanced that P's cognitive structure, which represents relations among P, O, and X is either in a what he called "balanced" or an "unbalanced" state. In particular, he proposed, "In the case of three entities, a balanced state exists if all three relations are positive in all respects, or if two are negative and one positive" (1946, p. 110). For instance, a balanced state exists when P likes O, P likes X, and O likes X; or when P likes O, P dislikes X, and O dislikes X; or when P dislikes O, P likes X, and O dislikes X. To generalize, a cognitive system is balanced if there are _no_ negative relations or if there is an _even_ number of negatives; it is unbalanced if there is only _one_ or any other _odd_ number of negatives.

The fundamental assumption of balance theory is that unbalanced cognitive systems give rise to tension and create forces to shift toward a state of balance. In a test of this hypothesis Jordan (1953) presented subjects with hypothetical situations involving two persons and an impersonal entity to rate for "pleasantness." The experimenter manipulated the situations according to Heider's scheme so that half of them were balanced and half unbalanced. The results indicated that the unbalanced situations received somewhat lower pleasantness ratings than the balanced situations.

Theory of Structural Balance

In 1956 Cartwright and Harary formally elaborated Heider's theory in terms of the mathematical theory of linear graphs (hence the the label of "graph theory") and derived certain deductions not explicitly formulated in Heider's version of the theory. This formulation is constructed in accordance with a more general and comprehensive definition of balance. Where balance in Heider's scheme was treated as an either/or event and did not allow of an intensity dimension, Cartwright and Harary have quantified balance in terms of an intensity range from 0 to 1. In addition, these investigators enlarged the theory to include any number of events and entities. Morrisette (1958) designed an experiment, similar in design to Jordan's experiment which supported Heider's theory, to test Cartwright and Harary's derivations. College women who served as subjects in this experiment were presented with a paper-and-pencil test situation. Three hypothetical female students who had been sharing an apartment for several months were described on paper to the subject who was informed that because of illness in the family, one of the female students had to leave school and the two remaining girls advertized for another student to share the apartment with them. The subject was then asked, assuming this living arrangement was to her advantage, to predict as accurately as she could the relations she would establish between herself and the other students. She was further informed that on meeting one of the students, Carol, she would be favorably impressed. Carol would seem to her to be socially adaptable and fair-minded, and from talking to her it would be evident that Carol liked Helen (the other room-mate). The subjects's task was to predict, then, how she believed she would feel toward Helen after she has lived with her for two weeks. Morrisette found that 91 per cent of his subjects predicted that they would like Helen. He argued that liking Helen results in a balanced system in which all three relations are positive. This finding was interpreted to mean that people use information (e.g., Carol's attitude toward Helen) to form attitudes toward objects and persons that are harmonious with pre-existing, interrelated attitudes, an inference that follows directly from the balance theory. Despite the significance of the obtained results, the design biases in the study do not seem to warrant such a straightforward inference. First, subjects did not have any other information about Helen to go on; second, only in rare occasions do people express overt dislike for someone they do not know; third, sex differences, which may have proved significant, were not taken into consideration. In view of these biases it is hardly appropriate to derive a supportive inference from the data for the balance theory, as Morrisette has done.

Theory of Strain-Toward-Symmetry

Newcomb (1953) removed Heider's notion of balance from its intra-individual context and spplied it to inter-individual communication, and substituted A for P and B for O. His postulate of "strain-toward-symmetry" is assumed to lead to a commonality of attitudes of A and B (two persons) oriented toward X (an object). The strain-toward-symmetry affects communication between A and B in order to bring their attitudes toward X into congruence.

Two studies seem to provide support for Newcomb's model. In one study (cited by Newcomb, 1953) a questionnaire was administered to college students in 1951 after President Truman had dismissed General MacArthur. The students' attitudes toward Truman's decision and their perception of the attitudes of their closest friends were analyzed. The results: Of the pro-Truman subjects 48 said that their closest friends favored Truman and none that their closest friends were opposed to his decision; of the anti-Truman subjects only 2 said that their friends were generally pro-Truman and 34 that they were anti-Truman. Further support for the strain-toward-symmetry hypothesis was adduced in a longitudinal study (Newcomb, 1956).

Newcomb set up a house around the campus of the University of Michigan which offered free rent for one semester for 17 students who were willing to serve as subjects in experiments. The volunteers who finally resided in the house were observed, questioned, and rated for four to five hours each week for the duration of the semester. The study was later replicated with another 17 volunteer subjects. The results supported Newcomb's hypothesis: Those subjects who were attracted to one another tended to agree on many matters, their own self-perceptions and ideal selves, and their attractions for other group members. It was also noted that these real and perceived agreements tended to increase over time. The work of Festinger and his colleagues (1950) on problems in social communication is also cited by Newcomb to support his hypothesis. Festinger et al. studies have indicated that the tendency to influence other group members toward one's own opinion increases with the degree of attraction.

In two other studies by Burdick and Burnes (1958) skin resistance indices (GSR) were correlated with emotional reaction in balanced and unbalanced situations. The findings in the first experiment showed that there are significant differences in skin resistance depending on whether the subjects agreed or disagreed with a "well-liked experimenter." In the second experiment it was found that the subjects who liked the experimenter tended to change their opinions toward greater agreement with his and those subjects who disliked him, toward greater disagreement.

The productivity of "balance" theories have thus far been rather meager. The experiments set up to test the theories, with the possible exception of Newcomb's longitudinal study (1956), have mostly dealt with subjects who interacted with hypothetical situations, and hence the obtained results which bear on the theories have mostly given circumstantial support to the theories concerned. The Burdick and Burnes experiments (1958) are perhaps the only ones that bear directly on the central notion of these theories.

One difficulty in the evaluation of these theories is that there has not been a serious experimental attempt to disprove the theory. It is quite plausible to maintain that some situations which are unbalanced according to these theories may indeed remain stable and produce no significant pressures toward balance. Festinger once asked in a humorous vein whether it followed from balance theory that since he likes chickens, and since chickens like chicken feed, he must also like chicken feed or else be troubled by imbalance (Zajonc, 1960). This jestful example does seem to emphasize some of the logical difficulties inherent in the conceptions of balance. Heider's theory of balance and Newcomb's theory of strain-toward-symmetry do not seem to specify what kinds of prediction can be expected when, e.g., both P and O are attracted to X but the origin and nature of these attractions are different. Furthermore, it is not clear from these theories what the consequences are when the relation between P and O is cooperative and when it is competitive.

Theory of Congruity

The principle of congruity in human thinking was stated by Osgood and Tannenbaum (1955) as, "*changes in evaluation are always in the direction of increased congruity with the existing frame of reference*" (authors' italics, p. 43). This is in effect a special case of balance and deals specifically with the *direction* of attitude change. Two assumptions are associated with this principle: First, it is stated that "*judgmental frames of reference tend toward maximal simplicity*" (authors' italics, p. 43). That is, since extreme "black-and-white," "all-or-nothing," judgments are simpler than refined ones, evaluations tend to move toward extremes or, as the authors state, there is "a continuing pressure toward polarization." Second, "since assumption of identity is a simpler process than maintenance of distinction, this also implies a continuing pressure toward elimination of differences among concepts. . ." (p. 43). It follows that related concepts tend to be evaluated in a similar manner.

On the basis of these assumptions, the congruity principle states that when change in evaluation or attitude occurs it always occurs in the direction of increased congruity with the existing frame of reference.

An instance of this principle is a person who is faced with an assertion concerning a specific event about which he believes and feels in a certain way, made by a person toward whom he also has some attitude: E. g., assuming that both person X and event Y are evaluated positively, and given that X supports Y (+), congruity is said to exist. Now, assuming that X is evaluated negatively, and given that X (-) supports Y (+), incongruity is said to exist. It is evident that in terms of their formal attributes, the concepts of "balance" and "congruity" are identical. Thus, when the attitudes toward the "source" and the "object" are similar and the "assertion" (a statement about someone, something, some event, etc.) is negative, or when they are dissimilar and the assertion is positive, incongruity is said to exist. When unbalanced states are defined in terms of one or more negative relations it is clear that they are equivalent to incongruity. As long as a person's attitudes are congruent with those implied in the assertion, the cognitive system remains stable. When the attitudes toward the person and the assertion are incongruent, the person will tend to change his attitude toward the other person and the object of the assertion in the direction of increased congruity.

To test the congruity principle in attitudinal change Tannenbaum (1953) designed an experiment in which he employed the "semantic-differential" technique, developed by Osgood et al. (1952 & 1957), as a measurement device. This technique essentially involves asking a subject to rate a concept on each of a number of bipolar adjective dimensions (e.g., fair-unfair, strong-weak) defined in terms of a 7-step scale on which "0" is the neutral point with 3 degrees of polarization in each direction, i.e., +3, +2, +1, 0, -1, -2, and -3. The meaning of the concept for a subject is his pattern or profile or ratings on the bipolar adjective scale. Following a pre-test of 36 potential objects of judgment, three "source-concept" pairs were selected on the basis of these criteria:

a) Approximately equal numbers of subjects holding favorable, neutral, and unfavorable original attitudes toward them, and

b) Lack of correlation between attitude toward the source and the concept making up each pair.

The following three source-object pairs were finally selected:

1) "Labor Leaders" with "Legalized Gambling"

2) Chicago Tribune with "Abstract Art"

3) "Senator Robert Taft" with "Accelerated College Programs"

Tannenbaum then obtained semantic-differential measures on 405 college students concerning their attitudes toward labor leaders, the Chicago Tribune, and Senator Robert Taft as sources, and toward legalized gambling, abstract art, and accelerated college programs as objects (or "concepts," in accordance with the parlance of the study) along with 4 "filler" concepts for control purposes. This constituted the before-test phase of the experiment. Five weeks later the same subjects were given "highly realistic" newspaper stories consisting of positive and negative assertions presumably made by the various sources regarding the concepts. Immediately afterward the subjects were given the after-test which consisted of obtaining semantic-differential measures of attitude concerning the same concepts in the same manner as during the before-test.

The results indicated that when the original attitudes toward the source and the concept were both positive and the assertion presented in the newspaper stroy was also positive, no significant attitude changes occurred in the subjects. When the original attitudes toward the source and the concept were negative and the assertion was positive, again no changes were obtained. However, as the congruity principle would predict, when a positively evaluated source was seen to make a positive assertion about a negatively evaluated concept, the attitude toward the source became less favorable, and toward the concept more favorable. Conversely, when a negatively evaluated source was seen to make a positive assertion about a positively evaluated concept, attitudes toward the source became more favorable and toward the concept less favorable. Thus the entire set of predictions based on the congruity principle was confirmed in Tannenbaum's data. The data are summarized in Table 1, in which the direction of change is represented by either a plus or a minus sign, and the amount of change by either one or two such signs.

TABLE 1--Change of Attitude Toward the Source and the Object When Positive and Negative Assertions are Made by the Source (Modified version based on Tannenbaum's 1953 data).

Original Attitude Toward the Source	Positive Assertion about an Object Toward Which the Attitude is		Negative Assertion about an Object Toward Which the Attitude is	
	Positive	Negative	Positive	Negative
	Change of Attitude Toward the Source			
Positive	+	− −	− −	+
Negative	+ +	−	−	+ +
	Change of Attitude Toward the Object			
Positive	+	+ +	− −	−
Negative	− −	−	+	+ +

Although the principle of congruity does not contain any new ideas it carries a decided advantage over the earlier formulations: It is formalized in quantitative terms which allow precise predictions, some already confirmed by Osgood and Tannenbaum research, concerning the intensity and direction of attitudinal change. While balance theory simply postulates a dichotomy of attitudinal elements (positive or negative), the congruity hypothesis provides for refined measurements of the intensity of attitudes by means of the semantic-differential technique; moreover, while Heider's balance scheme cannot predict directionality, the congruity viewpoint can in fact make predictions concerning the direction of change in attitudes (Zajonc, 1960).

The notions of consistency, dealt with in the preceding discussions, have all assumed that the "achievement" of consistency in the cognitive field is the focus of all activity that can be grossly classified as "cognitive." It is this same fundamental logic that characterizes the concepts of consonance and dissonance advanced by Festinger (1957). Thus, logically there is very little distinction between notions such as balance, congruity, symmetry, and Festinger's concept of consonance and its obverse, the concept of dissonance. The former set of notions can, therefore, be regarded as the logical antecedents of Festinger's concepts.

We can now turn our attention to a more exhaustive treatment of Festinger's theory of cognitive dissonance.

Theory of Cognitive Dissonance

In order to provide a general conceptual framework to aid the understanding of the detailed discussion that follows a synopsis of the salient features of Festinger's *A Theory of Cognitive Dissonance* (1957) will be presented first.

Dissonance is a relation of discrepancy between *two* beliefs or actions. The principle states that dissonance produces a pressure toward reduction of dissonance. Dissonance reduction may take many forms: Changing one's action, changing some condition in the environment, or changing one of the discrepant beliefs. The magnitude of dissonance and, therefore, of the pressure to reduce it, is a function of the significance of the disharmonious contents, and of their number.

The term "cognition" (used interchangeably with "cognitive element") is defined by Festinger as "any knowledge, opinion, or belief about the environment, about oneself, or about one's behavior" (p. 3). Hence "dissonance" is conceptualized as the existence of nonfitting or discrepant relations among pairs of cognitions, a condition which is seen to lead to certain kinds of activity aimed at the reduction of dissonance, i.e., to make the relation among cognitions fitting once more. Festinger's choice of this concept, which is synonymous with inconsistency, was due to his effort to avoid the logical connotations of the concept of inconsistency. The structure of the theory is based on two hypotheses proposed at the outset:

1) The existence of dissonance, being psychologically uncomfortable, will motivate the person to try to reduce the dissonance and achieve consonance.
2) When dissonance is present, in addition to trying to reduce it, the person will actively avoid situations and information which would likely increase the dissonance.

There are two root assumptions in Festinger's scheme. First, dissonance and consonance are ultimately a function of the person's phenomenal experience of reality, i.e., "The reality which impinges on a person will exert pressures in the direction of bringing the appropriate cognitive elements into correspondence with that reality" (author's italics, p. 11). Second, dissonance and dissonance reduction are paradigmatically equivalent to basic physiologic drives (e.g., hunger and thirst) and their reduction, i.e., the goal of dissonance-reduction is escape from tension.

A number of deductions are inferred from the theory:

(1) The process of decision-making gives rise to dissonance.

(2) Persons will attempt to avoid information or to reduce its impact when it increases dissonance.

(3) Persons will avoid others who are in disagreement with their beliefs, and will approach those who share them.

In order to prevent the inclusion of concepts such as motives, drives, conflict, frustration, thinking, and emotions into his rubric Festinger has tried to contain the connotative reach of the concept by emphatically reiterating that dissonance occurs among cognitions. The question arises, then, as to the operational identification of dissonance itself. It seems that Festinger's primary concern is with situations in which one cannot readily change either one's actions or the environmental events. Yet, Festinger's insistence that dissonance only occurs among cognitions presumably carries the implication that these have become independent of the antecedent motivational states. This is undeniably a controvertible implication in view of the accumulated data which seem to speak against such simplistic explanatory schemes for highly complex social behaviors. As Asch (1958) has suggested, "It would have been more accurate to describe the dissonance in question, which could hardly arise without some reference to what is desired, as a cognitive-affective process. . . (and) to have a more systematic account of how it might differ from outright motivational processes" (p. 194).

Moreover, dissonance reduction can be conceived, by implication, as an essentially habitual process of distortion and in this specific sense it does not seem to differ from similar processes described in "dynamic" theories of motivation (e.g., psychoanalysis): The mechanisms involved could hardly be distinguished from the ones involved in projection, rationalization, suppression, denial, intellectualization, and so on.

The core of Festinger's thesis can be re-interpreted to read that cognitive-affective inconsistencies create the same outcomes which are generally attributable to arousal-affective discrepancies. Thus, "They can all be conceptualized as reductions of discrepancy" (Asch, 1958).

To further pursue this re-interpretation of the logical fabric of Festinger's theory it can be argued that the process of dissonance reduction is in essence the consequence of ego processes which are governed by the "reality principle" (Freud, 1922 & 1949, p. 19) in view of Festinger's emphasis on intra-individual conditions (consistency) vis-a-vis the environmental demands (reality) in the resolution of dissonance. It seems that such psychoanalytic notions as "fantasy" (Freud, 1959, pp. 16-17) and the so-called ego mechanisms of defense (Anna Freud, 1946) and the role they play in the management of psychological states could be considered equivalents of the cognitive-system concepts dealing with dissonance resolution. Moreover, given that a person's cognitions are "conscious," it does not follow that the person himself is aware of all the relations among these same cognitions. Nor, in view of clinical and experimental observations can it be assumed that the elements that make up a person's cognitive "pool" are all consistent with each other even though it is plausible to assume that within each grouping of cognitive elements one might expect varying degrees of consistency and uniformity.

Then, dissonance reduction as a logical postulate need not presuppose "conscious" and voluntary properties and since, paradigmatically, it is equivalent to a general drive-reduction model, its theoretical utility seems debatable, especially when it does not seem to have an advantage over the drive-reduction model in terms of predictive efficiency (Myers, 1963). (It can be argued, however, that the cognitive dissonance theory possesses the apparent advantage of conceptual economy, specially in view of the complexity of the universe of observations which the theory purports to systematize and explain.)

Thus, Festinger's emphasis that dissonance reduction should not be contaminated with concepts such as drive, motive, emotion, and so on, seems superfluous and naive.

Several of these objections are highlighted by the criticism and research generated by the cognitive dissonance theory. In a review of Festinger's (1957) book Asch (1958) returns a verdict of <u>not proven</u> for Festinger's thesis, based on his evaluation of the research contained in the book in support of the theory. Asch discusses several objections to this research:

(1) Some studies provide supportive data for the theory; the data, however, are not consistent and despite statistical significance the trends in the data are weak.

(2) Those studies which tend to confirm the thesis most strongly are characterized by defects in analysis and design.

(3) The studies are heterogeneous in content and method, and consequently they fail to answer many crucial questions.

(4) There is a lack of description of phenomena as observed by the investigators which tend to retract from the richness of the data (1958).

Chapanis and Chapanis (1964) have assembled a critical review of the empirical evidence in this area up until 1961. These authors contend that, "The magic of Festinger's theory. . . seems to lie in the ease with which imponderably complex social situations are reduced to simple statements, most often just two such statements," (p. 2) and they go on to imply that even in such a simple format the research findings cannot be interpreted unambiguously.

They criticize the research on cognitive dissonance theory in terms of both methodological and inferential defects. Dissonance is said to function as "an intervening variable whose antecedents are the private internal cognitions of a person." It is this formulation of dissonance which most of the research studies have employed to predict complex social events and by means of elaborate instructions and intricate relationships between the experimenter and the subject they have attempted to create dissonance among subject's cognitive elements. One can, therefore, legitimately inquire whether the experimental manipulations have created dissonance and only dissonance.

In most of these studies if the relations between independent and dependent variables are considered regardless of the mediating steps it is possible to describe these relations via a pain principle, according to Chapanis and Chapanis (1964). This seems to follow from some of Festinger's inferences which, when reduced to their logical frame, seem to indicate that the more rewarding a situation, the more negative is the consequence; and conversely, the more painful a situation, the more positive is the consequence. This line of reasoning appears to follow from a quotation from Festinger (1961): "Rats and people come to love the things for which they have suffered."

The most general defect of some dissonance studies, according to these critics, is that their design is such that it precludes the derivation of meaningful inferences. To illustrate this point the authors describe and analyze an experiment on cognitive dissonance. The questions they attempt to answer are: Did the experimenter really produce the dissonance he claimed he did? Did the experimental manipulations give rise to other conditions that could contaminate or account for the findings?

Aronson and Mills (1959) put the following problem to experimental test: "Is severity of initiation positively related to group preference when motivation for admission is held constant?" Sixty three college women, who had volunteered to participate in a series of group discussions of the psychology of sex, were randomly assigned to one of three experimental conditions: A Severe Initiation condition, a Mild Initiation condition, and a Control condition. The two initiation conditions were presented as "screening devices to rule out women who could not talk about sex without embarrassment." In the Severe condition, the women read aloud to a male experimenter 12 obscene words and two vivid descriptions of sexual activity from contemporary novels. In the Mild condition the women read aloud five words related to sex which were not obscene. In the Control condition, the women were not required to read anything.

Each subject then listened as a silent member to a tape recording of what was ostensibly an "ongoing discussion" by the group which she had just joined. The discussion, about the sexual behavior of animals, was deliberately made as dull and worthless as possible.

At the end of the simulated group discussion the subjects were asked to rate the discussion and the women whose voices they heard during the discussion, on 14 different semantic-differential evaluative scales, e.g., dull-interesting, intelligent-unintelligent, etc. The means of the sums of the ratings are given in Table 2 in which the higher the score, the more favorable is the evaluation.

TABLE 2--Means of the Sums of Ratings by Subjects in the Aronson and Mills study (1959).

Item Rated	Control	Mild Initiation	Severe Initiation
Discussion by the Group	80.2	81.8	97.6
Discussion Group Members	89.9	89.3	97.7

It is clear that the ratings made by the Severe Initiation group are somewhat more favorable than those made by the other two groups. Now, how can these results be interpreted in terms of cognitive dissonance theory? Aronson and Mills infer that: The more painful the initiation, the more the subjects like the discussion group and its members (Cf. Festinger's notion that: "Rats and people come to love the things for which they have suffered.") They predicted the outcome for the Severe Initiation group in this manner: In successfully passing the embarrassment test these girls "held the cognition that they had undergone a painful experience" in order to join a group; the discussion, however, was so dull that they realized the unpleasant initiation procedure was not worth it. This resulted in dissonance since "negative cognitions about the discussion. . . were dissonant with the cognition that they had undergone a painful experience." One of the ways they could reduce this dissonance was by re-evaluating the group discussion as more interesting than it really was.

Chapanis and Chapanis (1964) argue that in order to accept this inference it must be shown that the subjects did in fact entertain such dissonant cognitions and these alone. Thus, the following conditions should be conclusively ruled out before Aronson and Mills' interpretation can be considered valid:

(1) Subjects felt no relief when they found the group discussion banal instead of embarrassing.

(2) Success in passing a difficult test (the embarrassment test) did not result in a change of their evaluation of the task.

(3) The sexual material did not evoke any vicarious pleasure or expectation of pleasure in the future.

(4) The group discussion was indeed so dull that the subjects would have regretted their participation.

There is no direct way of controlling for the first three conditions in this study. To check on the fourth condition the data from the Control group could be used: The discussion was, in fact, more interesting than not for this group (it received an average rating of 10 on a 0-15 scale). Consequently, the fourth condition is difficult to rule out.

Thus, Aronson and Mills' interpretation cannot be unequivocally accepted since the experimental design does not rule out the possibility that "pleasurable" cognitions were created at some point during the experimental manipulations and also the existence of "painful" cognitions is not sufficiently demonstrated. Chapanis and Chapanis further argue that in terms of the relation between severity of initiation and liking for the group, it is a feeling of successful accomplishment which is the crucial variable: The more severe the test, the stronger is the pleasurable feeling of success in overcoming the obstacle. If this line of reasoning obtains, a distinct dissonance drive need not be postulated since a pleasure principle can explain the results adequately. It may be this same "feeling of successful accomplishment" that is operative in some of the "effort" experiments conducted by the Festinger group (e.g., Cohen, 1959).

The overall evaluation of the cognitive dissonance theory and the pertinent literature presented by Chapanis and Chapanis underscores the observations that follow:

(1) As a body of literature, the experimental work adduced for the theory is "downright disappointing."

(2) The popularity of the theory is due to a "seductive allure" it has for many social scientists, an allure which is not shared by the similar, but logically more complex, formulations by Newcomb (1953), Osgood and Tannenbaum (1955), or Heider (1946, 1958).

(3) The appeal of the theory is due to its simplicity of formulation and application, a simplicity which is only apparent and largely deceptive. It is highly improbable that the essentials of a complex social situation can be reduced to two generally bipolar parameters. Thus, the simplicity of the theory has in fact proved to be a liability.

(4) Along with Asch (1958) Chappanis and Chapanis return a verdict of not proven concerning Festinger's theory of cognitive dissonance.

In defense of Festinger's theory Silverman (1964) has replied to the charges of Chapanis and Chapanis in this manner:

(1) Chapanis and Chapanis do not attempt to replace dissonance with a more plausible intervening variable to explain the empirical findings. Despite the abundance of critical arguments against the theory there is very little in the way of constructive explanation.

(2) If the theory of cognitive dissonance is to survive it should become less "simple" to allow for more critical tests of its deductions. In response to Silverman's argument it should be noted that this particular "refinement" could improve any theory--the question remains, however, whether a theory in its <u>initial</u> stages of empirical validation can indeed incorporate this type of refinement for, as Lachman (1963) has suggested, "The distinction between simple and complex phenomena is predicated upon the more extensive knowledge accumulated concerning the former: What is relatively well understood is simple; that which is less well known is complex" (p. 85). Thus the simplicity of the dissonance theory cannot be but deceptive, because the range of data it is meant to deal with is conspicuous in its complexity, despite the apparently simple formulations of the theory. One cannot sufficiently emphasize the difficulties involved in creating experimental dissonance. This will have to be attempted

slowly by a process of successive approximations during which the essential can be teased out of the non-essential.

(3) Although Rosenberg and Abelson (1960) have presented a model to predict the mode of dissonance reduction, this entire problem, which Festinger's theory left largely nonexplicit, should be theoretically and experimentally worked out.

It is the universal fate of almost all scientific theories to be met with ambivalent and fanatical attitudes of the investigators in their domain of intent. Cognitive dissonance theory is no exception, although in view of its apparent testability one would expect it to fare better and to lead to more clear predictions and results. (It should be recalled that the same laws that govern the behavior of experimental subjects describe the behavior of theorists and research workers as well.) Yet, one reason for the buoyancy of this theory is perhaps that "it predicts what will happen when it fails to predict correctly" (White, 1958, p. 656).

What can we conclude from the gamut of diverse evidence and controversy created by dissonance hypotheses and research? Weick (1965) in an up-to-date and comprehensive review of the salient problems of cognitive dissonance theory and research observes that the utility of the dissonance hypotheses does not rest in their explanatory potency: Rather, these hypotheses furnish means of thinking about problems. The theory of dissonance is one of many that concern themselves with "quasi-logical constraints on thought processes" (p. 1272). The constraint treated by dissonance theory is in Weick's view that of psychological implication. It is assumed that the implications that arise from beliefs and their interactions impose order on cognitive processes: If a person holds belief X it is implied that there are other specific beliefs correlated with belief X. "As fewer and fewer of these implications are realized, there will be increasing pressures to re-establish them. Without reliable implications, action would be difficult" (p. 1273).

Much has happened in dissonance research since 1961 which contrasts with the research done prior to this date and which formed the basis of Chapanis and Chapanis' (1964) critical review. The more recent studies seem to be directed toward testing increasingly specific derivations from the theory. A few examples: The work by Brock (1963) on post-decision dissonance; by Adams (1963) on inequity; by Aronson and Carlsmith (1962) on expectancy; by Walster (1964) on decision making behavior; by Jenker (1964) on exposure to new information; by Davidson and Kiesler (1964) on pre- and post-decision behavior; and by Zimbardo (1965) on effort. Yet, one cannot be altogether enthusiastic about this growing concern with specificity in dissonance research for as Abelson (1964) has commented, "Areas of science which are at the center of the stage at one time are destined to be mined out in a few years. As the mining process nears completion many concern themselves with even more specialized and trivial aspects. Ultimately they discover that the rest of the world has passed them by, that few others are even slightly interested in what they are doing" (p. 371). In the midst of this push toward specificity the connotative status of the theory does not seem to have changed to any considerable extent.

Perhaps the notoriety of dissonance theory for predicting the non-obvious is another liability in the further exploration of its derivations (Myers, 1963), although Margoshes's (1964) observation that ". . . connotations of <u>any</u> theory, when stretched far enough, will generate unwarranted implications" (p. 886) seems an appropriate reply to this criticism. (Also compare Jordan's, 1964, argument on this score.)

Like any other psychological theory, the concern of the cognitive dissonance theory with conceptual economy and simplicity is characterized by the <u>ceteris paribus</u> preamble. But, the question is just what variables must be held constant and how important, relative to consistency, they are.

Summary of Theories of Consistency

In this section a synoptic analysis of the several theories of consistency will be presented in terms of certain common and differential features for purposes of comparison. The main features of this analysis were advanced by Osgood (1960) in an effort to provide a mooring for the various conceptions of consistency.

These different formulations have, according to Osgood, provided for differing treatments of the following theoretical statements:

(1) "*Cognitive modification results from the psychological stress produced by cognitive inconsistencies*" (p. 345, author's italics).

This is a statement about a drive state, analogous to other drive states such as hunger, sex, thirst, but in terms of its origin it is purely cognitive. Definition of such states of cognitive "consistency" and "inconsistency" varies among different theorists with respect to its operational precision.

Heider (1958) conceptualizes these states in terms of *balance and imbalance* but he does not make any statement about how one precisely defines these states. Newcomb's (1953) concern is with the process of human communication as a means of striving for or maintaining *symmetry* in the orientation of individuals concerning their environment. Festinger (1957) employs the *consonance* and *dissonance* constructs to account for the relations which may exist between pairs of cognitive elements. Cartwright and Harary (1956) attempt a formal definition of the concept of balance in terms of mathematical graph theory. This type of formal analysis is further elaborated by Harary (1959) in a recent publication. There is a clear difference among these theorists concerning the locus of the cognitive maps and relations. Festinger, and also Heider to some extent, conceive of cognitive processes as resting within the individual perceiver; both Newcomb and Cartwright and Harary seem to adopt an objective (group structure and dynamics) framework. Osgood et al. (1957) assert that cognitive elements are equivalent to the meanings of signs, and these are indexed in terms of n bipolar dimensions or factors. According to these investigators, congruity is said to exist when the evaluative meanings of interacting signs are equally polarized or intense; incongruity is said to exist when there are differences in polarization.

(2) "If cognitive elements are to interact, they must be brought into some relation with one another" (p. 349).

Contiguity is a necessary, but not a sufficient, condition for cognitive elements to interact. Festinger's theory does not give explicit recognition to this condition, although there is some implicit recognition of the need for such relationships in the design of some of his experiments i.e., dissonance occurs only when a person has been forced to make a choice, when he has been exposed to consistent or inconsistent information, and so forth.

Heider does explicitly deal with this condition in terms of gestalt perceptual factors such as similarity, proximity, common fate, continuity, set, and past experience.

Osgood et al. provide an absolute distinction between structure and content in the representation and analysis of cognitive interactions. It is stated that two cognitive elements interact only when they are related in some kind of <u>assertion</u>. Assertions may be linguistic or behavioral and they may be either associative or dissociative. However, congruity of a particular cognitive pattern depends on both the structure <u>and</u> the content.

(3) "<u>Magnitude of stress toward modification increases with the degree of cognitive inconsistency</u>" (p. 353).

Most of the consistency theories suggest this relationship although they do not permit its quantification. Heider's original theoretical statements remain largely qualitative. Newcomb makes some attempts at quantification, e. g., "The stronger the forces toward A's co-orientation in respect to B and X, . . . the greater A's strain toward symmetry with B in respect to X. . . ," (1953) but he does not specify the units that are to be used in actual measurement. Festinger states that the magnitude of dissonance increases (a) with the importance of the dissonant elements, and (b) with the weighted proportion of all elements in a cognitive cluster that are dissonant. Yet, it is not clear how these are to be quantified.

Osgood and Tannenbaum's congruity hypothesis has been quantitatively tested by means of the semantic-differential technique, although the types of situation that can be studied with this technique are limited. Cartwright and Harary determine the degree of balance of an S-digraph by taking the ratio of the number of positive semicycles to the total number of semicycles.

(4) "The dynamics of cognitive interaction are such that modifications under stress always reduce the total cognitive inconsistency" (p. 354).

This conception is analogous to a kind of "mental homeostasis" (Stagner, 1951): Cognitive inconsistencies tend to exert pressures toward their own elimination. Yet, it must be stated that these modifications do not have to occur at all. People may simply not think about the inconsistency or they may avoid exposing themselves to information that tends to arouse dissonance. Moreover, as Heider has remarked, there are people who seem to be able to "live with it" or even actively seek out discrepant cognitions; they seem to be more "tolerant of ambiguity." Such tolerance might perhaps be positively correlated with intelligence, experience, and certain individual difference parameters.

Altogether, these various cognitive consistency theories seem to have been more explicit with respect to stating alternatives than deciding among them, according to Osgood.

THE ACTIVATION HYPOTHESIS AND THE PRESENT STUDY

General Observations

The concept of consistency and its equivalents seem to have given rise to a number of logical difficulties due to their inherent circularity and a lack of clear-cut operational anchorage. It may very well be that this concept has outlived its theoretical usefulness.

Parallels have been noted in the history of science: One such parallel is the concept of vacuum. Conant's (1947) excellent treatment of the history of this concept informs us that the idea that nature abhors a vacuum (horror vacui) was used as an explanatory principle for such phenomena as the action of pumps, behavior of liquids in joined vessels, suction, and so on. The anecdotal evidence was so imposing that the validity of the principle was left largely unquestioned, although it was known that water cannot be drawn to a height of more than 34 feet. The explanation of this event was formulated very simply, indeed: "Nature abhors a vacuum below 34 feet." This explanation was accepted until it was discovered that "nature abhors a vacuum below 34 feet only when we deal with water." Torricelli objected to this principle on the grounds that it had to be modified with exceptions whenever a previously unexplained phenomenon challenged it and instead he formulated the principle that it is the pressure of air acting upon the surface of the liquid which determines the height to which one could draw liquids by the action of pumps. This novel principle provided an adequately general schema to incorporate a comprehensive range of data.

Psychological principles and constructs are not exceptions to Conant's observations. Human nature, in the realm of psychology, is said "to abhor inconsistency" (Zajonc, 1960). This principle appears adequate for the time being although exceptions are already pressing for some explanation. The people who put their money into buying insurance and also into gambling seem to be at once protecting themselves from and exposing themselves to risks. (It should be noted that concepts of consistency invariably assume that intra-individual cognitions are _conscious_--or at least, they are _not_ unconscious.) And the magician whose job is in essence creation of dissonance does not seem to have lost much of his attraction. Zajonc (1960) has appropriately inquired, "If decisions are necessarily followed by dissonance, and if nature abhors dissonance, why are decisions ever made?"

Aside from its explanatory purpose, the _horror vacui_ principle served the important goal of emphasizing the need for a more inclusive principle due to the discomfort of "exceptions to the rule." Thus, "If a formulation has. . . a virtue in being wrong, the theories of consistency do have this virtue. They do organize a large body of knowledge. Also, they point out exceptions, and thereby they demand a new formulation. It will not suffice simply to formulate them so as to accommodate the exceptions" (Zajonc, 1960, p. 290).

Let us turn our focus now to some of the specific "exceptions" which were suggested in the course of the previous discussion in order to determine whether any reformulations are called for. Aside from the bulk of anecdotal exceptions, the majority of the observations that do not seem to receive an adequate theoretical treatment in the rubric of cognitive dissonance principles come from empirical studies.

Rosen (1961) conducted a modified version of the Mills, Aronson, and Robinson (1959) study. Mills et al. hypothesized that the more important the decision the greater the preference for positive information about the chosen alternative than for positive information about the rejected alternative, and that the converse would hold for negative information. Dissonance reduction principle received support only in the "positive information" condition. Rosen (1961) speculated that the lack of support for the theory in the "negative information" condition was partly due to the fact that choosing among unfavorably described pieces of information is something that people are disinclined or unaccustomed to do. Moreover, studies of risk avoidance and individual and sex differences in cognitive behavior (Kogan & Wallach, 1960; Pettigrew, 1958; Wallach & Kogan,

1959) have demonstrated that the two variables of decision certainty (Kogan and Wallach) and category width in cognitive judgments (Pettigrew) are inversely linked to risk avoidance, which includes behavior such as avoidance of negative information and so on. Thus, in addition to Mills et al.'s original hypothesis Rosen also tested the hypothesis that dissonance reducing tendency is inversely related to decision certainty and to category width. This latter hypothesis is in effect a statement concerning the relation of individual and sex differences (Cf. Wallach & Kogan, 1959) to dissonance-reduction tendencies.

The results indicated that decision importance was not significantly related to dissonance reduction; a significant majority of subjects preferred the information which advised change from the chosen to the nonchosen alternative; male subjects used wider categories and expressed more certainty regarding their judgments than female subjects; and decision certainty was not found to be linked to dissonance reduction.

The Rosen data show that the effects of dissonance reduction tendencies are varied, and provide support for a tendency, already noted by Mills et al. (1959) to the effect that people seek more information about their choice despite the negative valence of the information. It is difficult to reconcile this finding with the contention that people prefer to receive information which supports their own views.

In another study, Steiner (1962) re-analyzed the data reported by Brodbeck (1956) in support of the conclusion that individuals whose views have been shaken by counterpropaganda tend to revert to their original opinions after a free discussion because they "tend to listen preferentially to persons who agree with them and to ignore the arguments of their opponents" (p. 170).

Steiner (1962) concludes, in line with Rosen's interpretation, that the two studies by Brodbeck (1956) and Mills et al. (1959) which are widely quoted in support of the notion that people are more receptive to supportive than to adverse information do not provide sufficient support for that proposition. Steiner observes that it is plausible that "there are conditions which induce people to seek supportive information, and other conditions which create a susceptibility to adverse information" (p. 267).

In line with these observations Feather (1962 & 1963), in two studies dealing with cigarette smoking and lung cancer with young university students and older male subjects, demonstrated that regular smokers were more interested in information about the relation between cigarette smoking and lung cancer (i.e., dissonant information, by definition) than were nonsmokers. The greater interest was shown by the subjects <u>regardless</u> of whether the information supported or did not support a positive relation between cigarette smoking and lung cancer. This result is difficult to reconcile with dissonance theory; but the finding that regular smokers regarded the evidence for a relationship between cigarette smoking and lung cancer as less convincing than did nonsmokers tends to support the theory.

Feather's second study (1963) was performed on older subjects in order to replicate his first study (1962) done with university students who presumably were not yet committed to the habit of smoking cigarettes. The rationale for this replication is stated as: "The predicted influence of cognitive dissonance on sensitivity to information may occur only after commitment to a belief, attitude, course of action" (p. 158). This is considered a direct derivation from Festinger's theory. Also, Feather's second study (1963) attempted to explore the possibility that individual differences in dissonance reduction may be related to Eysenck's (1957) dimensions of neuroticism and extraversion; this latter possibility was not borne out by the results.

The results of the first study had suggested that cognitive dissonance may have more influence on a person's evaluation of information than on his sensitivity to information. The results of the second study paralleled these rather closely:

(1) Cognitive dissonance does not have the predicted influence on sensitivity to information, since both young and older smokers

expressed greater interest in an article concerning cigarette smoking and lung cancer than did nonsmokers, regardless of the kind of relationship which the article supported.

(2) Cognitive dissonance does have the predicted influence on evaluation of information, since both young and older smokers were more critical of the evidence that cigarette smoking leads to lung cancer than were nonsmokers.

Feather (1963) proposes an alternative explanation for the data: ". . . regular smokers may have a stronger and more general fear of lung cancer than nonsmokers. . . and information about lung cancer may be consonant with the knowledge that one is afraid" (p. 161). Feather then concludes that cognitive dissonance seems to influence a person's evaluation or interpretation of information rather than his sensitivity to information.

Krech and Crutchfield (1948) have proposed a similar principle: "Other things being equal, a change introduced into the psychological field will be absorbed in such a way as to produce the smallest effect on a strong structure" (authors' italics, p. 108).

Finally, in a study by Freedman (1965) dealing with preference for dissonant information subjects were asked to make a decision and then they were given a choice between reading a consonant or a dissonant communication. The results indicated that all 17 subjects chose to read the dissonant communication and "this is not simply a lack of preference such as has often been reported in the past. . . but clear preference for dissonant information" (p. 288). This is explicitly contrary to Festinger's theory.

Freedman offers the following explanations for the results:

(1) Subjects may have perceived the dissonant information as more useful.

(2) Subjects' preference may have been due to the perceived interest of the dissonant information.

(3) In addition to usefulness and interest the size of the effect (i. e., all subjects preferred the dissonant information) indicates that other factors may be involved. One such factor may be subject's confidence in his own decision, i. e., if he is very confident about the correctness of his decision, it may well be that the best course of action is to seek out dissonant information and attempt to refute it.

(4) Preference between dissonant and consonant information is considerably more complex than it has generally been assumed.

Need For An Alternative Explanatory Model

The studies that have been presented so far point to the existence of a certain range of data that the cognitive dissonance theory cannot coherently explain. Although there have been attempts to reconcile these data with derivations from the theory, they are essentially in the nature of post hoc explanations and apologies and do not substantially enhance the predictive efficiency of the theory as it was originally conceived by Festinger (1957). Another difficulty with notions of consistency and inconsistency and their equivalents is the practical impossibility of their _a priori_ operationalization. As a compensatory measure, however, one can operationally determine _what_ parameters are used and _how_, _why_, and in what _conditions_ from the vantage point of subject's _own_ experience as revealed by his verbal and expressive behavior. This is an especially acute problem in cognitive dissonance theory, which is, parenthetically, phrased in terms of intra-individual events but has failed to utilize the wide scope of descriptive and phenomenological data. It is veritably impossible to determine whether certain information is dissonant in an individual's cognitive schema _unless_ the individual reveals this directly by various behavioral indices. Hence, the significance of individual differences. An illustration of this is the behavior of postoperative cancer patients in relation to "going home" feelings reported by Bard (1966) in a descriptive account of the adaptation problems faced by cancer patients who presumably have been "cured" of the dread disease:

> Some patients are eager to see their children and families again and feel that only at home will they be able to recuperate properly. For these patients, family and friends represent the support and warmth they miss in the sterile and symbolically mutilative atmosphere of the hospital. Others, however, are not eager to return home, either because of feelings of weakness or because they feel unable to face people. They are relating the results of their therapy to a

> social context, and to them the impersonal hospital environment is less threatening than their fears of social inacceptability (p. 12).

Also:

> Many patients complain that their families and friends refuse to let them discuss their hospital experiences. These patients are usually resentful; they feel they would be relieved if people would accept the fact that they had cancer, and refer to it as they would to other diseases. However, there are other patients who never want to discuss their experience, fearing they might become depressed. These tend to become hyperactive, doing everything to keep from thinking about it and "feeling blue" (p. 13).

These increasing empirical "exceptions" seem to require some other conceptual scheme which possesses more explanatory potency. It is likely that constructs of consistency and inconsistency have been overworked to such an extent that their connotative properties have assumed greater significance than their denotative structure.

One such formulation is the activation hypothesis of cognitive behavior with which this study is specifically concerned. We will begin, first, by a theoretical and empirical account of the construct of activation in psychology; we will proceed next to the presentation of the activation hypothesis which this study was designed to explore.

The various notions pertaining to "activation," "excitation," and "energy mobilization" were initially proposed by Duffy in 1932 and further elaborated by her in various publications (1941a, 1941b, 1951, 1957, and 1962) to refer to motivated behavior and specifically to "emotional" behavior. The concept of activation, instead of treating emotion as a special qualitative state, located emotional behavior along a continuum that includes <u>all</u> behavior, ranging from sleep at the low end, to alert attention at medium level, and strong emotions at high levels. The concept came into vogue, however, when Lindsley (1951) formulated what he called an "activation theory."

Duffy's recent book (1962) has attempted to show the place of the concept of activation in psychology and also to demonstrate the significance of this concept for the understanding of individual differences in behavior. Application of the concept of activation to emotional behavior has involved three main lines of approach: (1) through electroencephalography and neurophysiology, (2) through physiological studies of "behavioral energetics," and (3) through the search by learning theorists for a satisfactory measure of drive (Malmo, 1957).

The principle of an optimal level of activation or excitation has in recent years been advocated by a number of workers as a more inclusive notion to replace the drive-reduction concept, or for some investigators, to supplement it with the notion of drive-induction into one unitary scheme (Berlyne, 1963; Fiske and Maddi, 1961; Hebb, 1955; Leuba, 1955). Seward (1963) has argued that, "Such a concept could be a handy tool for explaining why rats turn on lights, monkeys open windows, children play, and adults visit museums--or why so many people read about space flights and so few volunteer" (p. 707).

It is likely, for instance, that the rats in the controversial experiment by Olds and Milner (1954) concerning the electrical self-stimulation in the rat hypothalamus, were not responding to an hypothetical pleasurable reinforcement but instead they sought stimulation to compensate for the reduction of stimulus variation produced by confinement in the experimental box. Partial support for this stimulus-induction hypothesis comes from an unpublished study by the author in 1962 in which rats, which were confined for a period of two weeks in small and tightly covered cages kept in isolation in a totally dark and relatively sound-proof room, engaged in significantly more varied activities than control rats which were kept in the normal laboratory conditions for a comparable period of time. Other supportive evidence for this formulation derives from the animal studies by Kish (1955) and Marx et al. (1955) in which mice and rats showed a marked increase in the rate of bar-pressing response the only consequence of which was to turn on a weak light that had never been paired with need reduction. In another study by Kish and Antonitis (1956) mice, in a situation which contained four platforms in its four corners, preferred to step on the one that moved and clicked.

Seward (1963) sees two problematic implications in some versions of the notion of optimal level of activation: First, they imply some kind of positive feedback which could produce serious troubles in terms of organismic adaptation and survival, and second, if drive level is lowered below its optimum as a function of stimulus reduction where does the energy for increased activity originate from? According to Seward these objections are met by Berlyne (1963) by assuming that "below some middle value falling stimulation sends the arousal level _up_, not down" (p. 707).

Another line of supportive evidence for this conception comes from the recent studies on "sensory deprivation" beginning with an experimental exploration by Bexton, Heron, and Scott (1954). These investigators paid students $20.00 a day to remain in an isolated cubicle, wearing translucent goggles that excluded patterned vision and cardboard cuffs and heavy gloves that minimized tactual stimulation, and with no auditory stimulation aside from a monotonous hum. This treatment produced a large number of intriguing phenomena, from hallucinations to a deterioration in cognitive abilities. The most significant findings from the standpoint of activation level were these: Subjects could stand no more than a few days of this treatment, despite the high rate of pay and the complete lack of exertion; they resorted to desperate and far-fetched measures for providing themselves with increased stimulation, e.g., by talking or whistling to themselves or tapping the cardboard cuffs together; when given the opportunity, they would call again and again for stimulation that they would normally have ignored as intolerably dull, e.g., a recording of an old stock-exchange report or of a speech on the evils of alcohol intended for six-year-olds; they showed growing irritability and other indications of emotional stress; in short the subjects found the experience unpleasant in the extreme.

Dramatic findings of this nature have been increasingly reported by other empirical studies in the field (Cf. Fiske and Maddi, 1961, and Solomon et al., 1961, for a comprehensive review of the pertinent theories and research) and the bulk of this research indicates that in terms of motivational states varied environmental stimulation seems to be vital in maintaining the efficiency and stability of behavior.

Seward (1963), in his effort to derive some meaningful explanatory principles to account for the accumulated mass of data in this field, advocates a new conception of motivation which he assumes will embody both <u>endogenous</u> motives such as hunger, thirst, sex, and so on, and <u>exogenous</u> motives such as curiosity, play, exploration and the like, the necessity for theoretical treatment of which was initially heralded by Allport's (1937) arguments concerning the "functional autonomy of motives." Seward seems to prefer a biological explanatory scheme and goes on to observe:

> . . . let us assume that the central nervous system has the primary function of correlating stimuli and integrating appropriate responses. Its business is, as Woodworth (1958) put it, to "learn the environment." In the course of life it does so by interiorizing the external world, both as perceived and as altered by the organism's

> own reactions. Here I refer to what Bartlett (1932) called a _schema_; call it a life space (Lewin, 1936) if you prefer, or a situation-set (Woodworth, 1937), or a cognitive map (Tolman, 1948), or an image (Miller, Galanter, and Pribram, 1960). By any name, such a construct can be built and maintained only by a continuing transaction with the environment. To do its job the brain must have materials to work with, sensory input to organize, information to process (Glanzer, 1958). It is at critical points in this transaction that exogenous motives arise: when novel stimuli call for revision of the schema, or when radical incongruities threaten its stability, or _when a routine conforms too closely to the schema for too long_ (final italics added, 1963, pp. 707-708).

In sum, as endogenous motives or "basic" drives subserve the organisms' survival and adaptation, so do exogenous motives promote the organisms' attempts to predict and modify their environments.

We will next turn to a full treatment of the "activation hypothesis."

The Activation Hypothesis of Cognitive Behavior

The activation hypothesis proposed in this study is the outgrowth of the integration and elaboration of some of the preceding ideas and formulations, specially those recapitulated by Seward (1963). It is a conceptual framework which subserves the application of the concept of "optimal level of activation" to data in the cognitive realm. There is no specific assumption in this conception regarding the hypothetical neurophysiological anchorage of the constructs (in contrast to;, say, Lindsley's formulation which is directly linked to the processes in the reticular activation system of the brain stem).

It is granted at the outset that this formulation is indeed sketchy and vague in many respects. It is hoped, however, that it can be gradually tightened and its loose ends tied together as it is put to more empirical tests.

Formal Statement of the Hypothesis

Cognitive behavior is the function of a phenomenal continuum of activation at the extremes of which congruent (homologous) and incongruent (heterologous) cognitive states are located. When cognitive states are activated in the direction of either one of these extremes as a result of internal and/or external stimulus variations psychologic tension will ensue and the individual will consequently endeavour, overtly and covertly, to restore his cognitive state to an experientially optimal level of activity by active manipulation of the amount and the direction of internal and external stimulus variations. In either instance active cognitive interchange with the intra- and extra-individual environment will result. The optimal level of cognitive functioning is the condition which is experienced as psychologically comfortable and desirable.

The fundamental proposition in this formulation is that cognitive activity is seldom in a quiescent state. There is a constant flow of activity in the individual's cognitive system. When the flow of cognitive activity is experienced as either too congruent (homologous or monotonous) or too incongruent (heterologous or discrepant), the individual will tend to revert from both extreme conditions to an experientially optimal level of activity.

Several implications follow from this formulation:

(1) Cognitive congruity and incongruity are functions of the perceived arrangement of cognitive data in terms of stimulus variations which activate and control the flow of cognitive behavior.

(2) Prolonged or widespread congruity and incongruity are both assumed to be psychologically noxious.

(3) The optimal level of activation is considered to be a moderate one, represented symbolically by the middle portion of a 'U' curve which expresses the relation between the degree of activation and overt and covert responses. Specifically, at the extremes an increase in congruity or incongruity of cognitive input is accompanied by an increase in cognitive output. The middle region of the 'U' curve (◡) is regarded as the optimal or "normal" range of functioning which is the "goal" of adaptive cognitive behavior; this hypothetical goal, however, is rarely achieved, and hence the constant flux in the direction and intensity of cognitive behavior.

(4) The continuum referred to in this paradigm is that portion of the general arousal continuum that embodies behavior in a wakeful state; hence, only in death and, observably, in sleep and coma there is

an absence of meaningful cognitive activity.

(5) The notion of activation used in this model is logically distinguishable from other notions such as Duffy's (1957) and Lindsley's (1951), although in line with notions such as Berlyne's (1963), in the sense that it is less inclusive in terms of the possible range of behaviors and also it assumes equivalence of activation intensity at either end of the continuum.

(6) The model is comprehensive in terms of the variety of data it attempts to integrate, i.e., it deals with responses of greater inclusiveness and of higher integration. Also, cognitive variables are assumed to interact at a higher level of complexity in this model than, say, in Festinger's model which emphasizes diadic relationships.

(7) Since the experientially optimal level of cognitive activity is basically an intra-individual event, individual, and implicitly sex, differences are assumed to be one of the most important classes of variables that determine the structure and function of cognitive behavior.

(8) Hence, certain derivations from this hypothesis are not operationally describable since the cognitive patterning that is perceived as congruent by one individual may not be perceived as such by another individual.

(9) It follows that the aim of cognitive behavior is not always drive reduction, as it is assumed, for instance, by Festinger's theory; cognitive behavior may also involve purposeful drive-induction.

(10) The optimal level of cognitive activity for a specific individual cannot be determined _a priori_; its existence and consequences are inferred from other behaviors in the individual's repertoire such as talking, facial expressions, overt decisional responses and the like, since variations in the degree of activation are assumed to be correlated with variations in behavior.

(11) The model is conceived within a phenomenological framework.

The present research was undertaken to test a basic derivation from the activation hypothesis: It is predicted that induced states of cognitive congruence and incongruence will tend to result in _comparable_ amounts of cognitive activity in the individual.

For purposes of comparison the relevant prediction from Festinger's theory (1957) will also be put to test: It is predicted that induced states of cognitive congruence and incongruence will tend to result in more cognitive activity in the individual in the incongruent (i. e., "dissonant") than in the congruent condition.

METHOD

Briefly, the plan of this research involves the feedback of "consistent" and "inconsistent" information (independent variable) to the subjects in these respective groups in order to compare their performance with the subjects in the control groups. The number of relevant (i.e., to the experimental task and situation) statements produced by the subject, the number of words contained in the total statements of the subject, the reaction time of the subject in the experimental task, the total number of word association responses given by the subject, and the deviation of subject's counts from the correct counts of the stimulus patterns presented to him constitute the class of dependent variables which is used to gauge the intensity of subject's cognitive activity. The prediction is that the subjects in both the "consistent" and "inconsistent" information groups will produce considerably more cognitive activity, as measured by their various performance indices, than the subjects in the control groups.

A series of pilot studies was conducted prior to the execution of the final experiment in order to discover empirical solutions to several methodological problems which had resulted from the original design of the experiment. Since these pilot studies were performed to answer specific design questions they will be referred to in their appropriate contexts.

Materials

The materials used in this study consisted of the following items:

(a) A series of ten "negative" 35 mm slides representing photographic reproductions of the ten stimulus patterns (Figures 1 through 10). Each stimulus pattern consisted of a large number of specific geometric forms represented on a two-dimensional surface. The patterns were prepared in this manner: The eraser at the end of a No. 2 pencil was cut, by a razor blade, into a specific geometric form, say a triangle; then, by means of a black stamp-pad, a large number of the impressions of the geometric form on the eraser were printed on an $8\frac{1}{2}$ by 11 inches sheet of white bond paper. The geometric forms consisted of full triangles, circles, half circles, quarter circles, and retangles. The series of stimulus patterns which was finally chosen for experimental use is presented in Table 3.

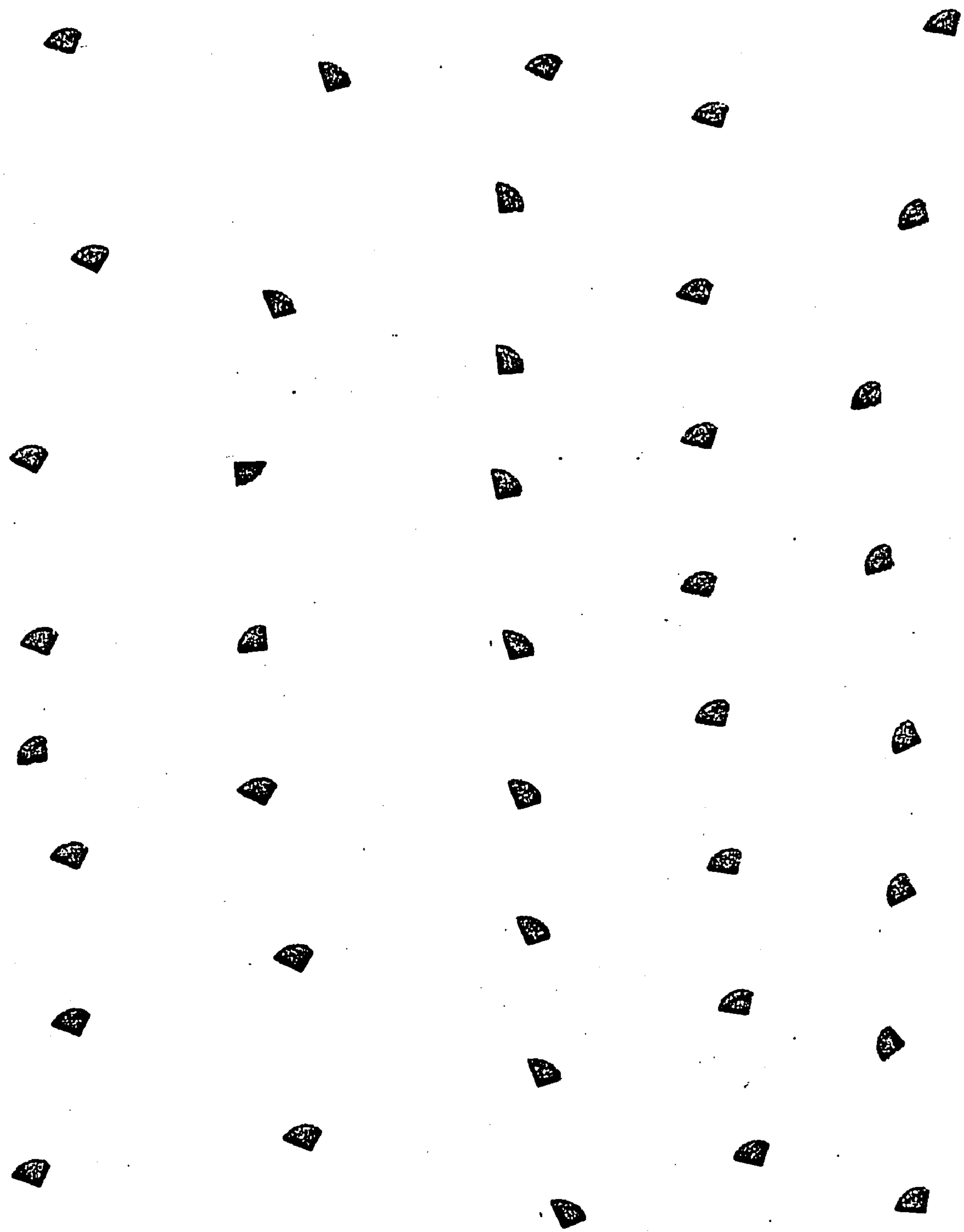

Fig. 1.--Stimulus Pattern No 1 Consisting of 40 Quarter Circles.

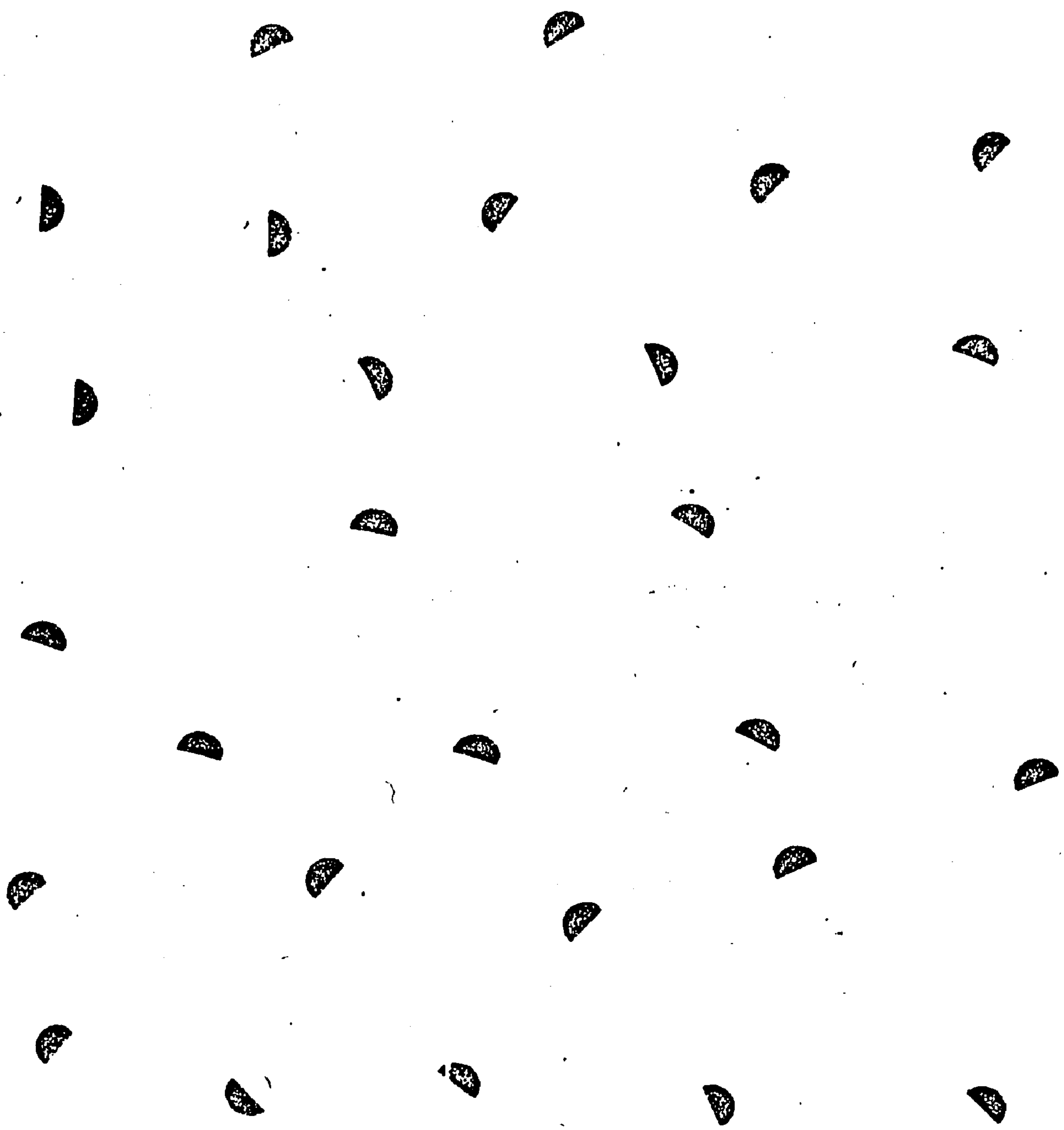

Fig. 2.--Stimulus Pattern No 2 Consisting of 27 Half Circles.

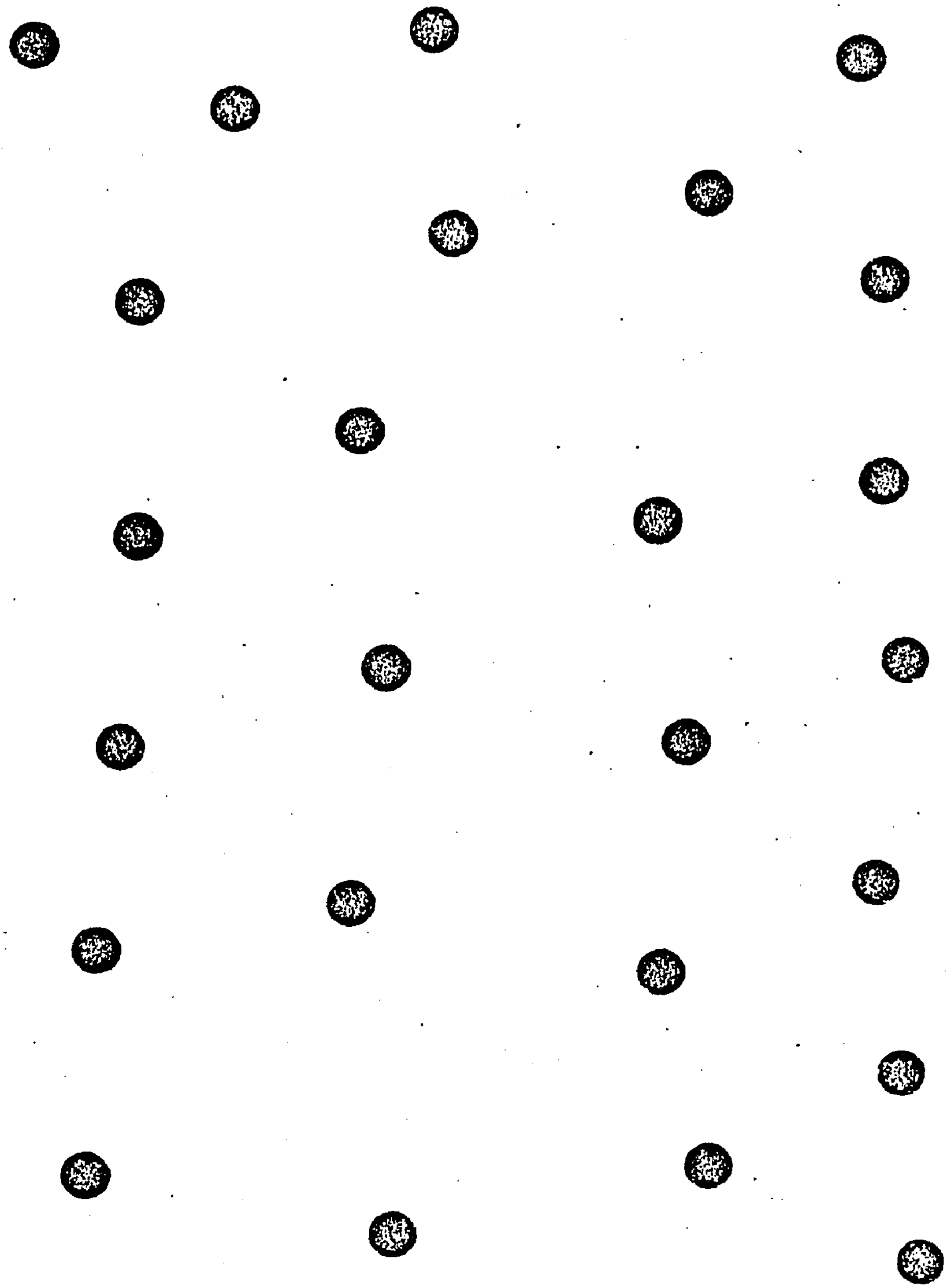

Fig. 3.--Stimulus Pattern No 3 Consisting of 25 Full Circles.

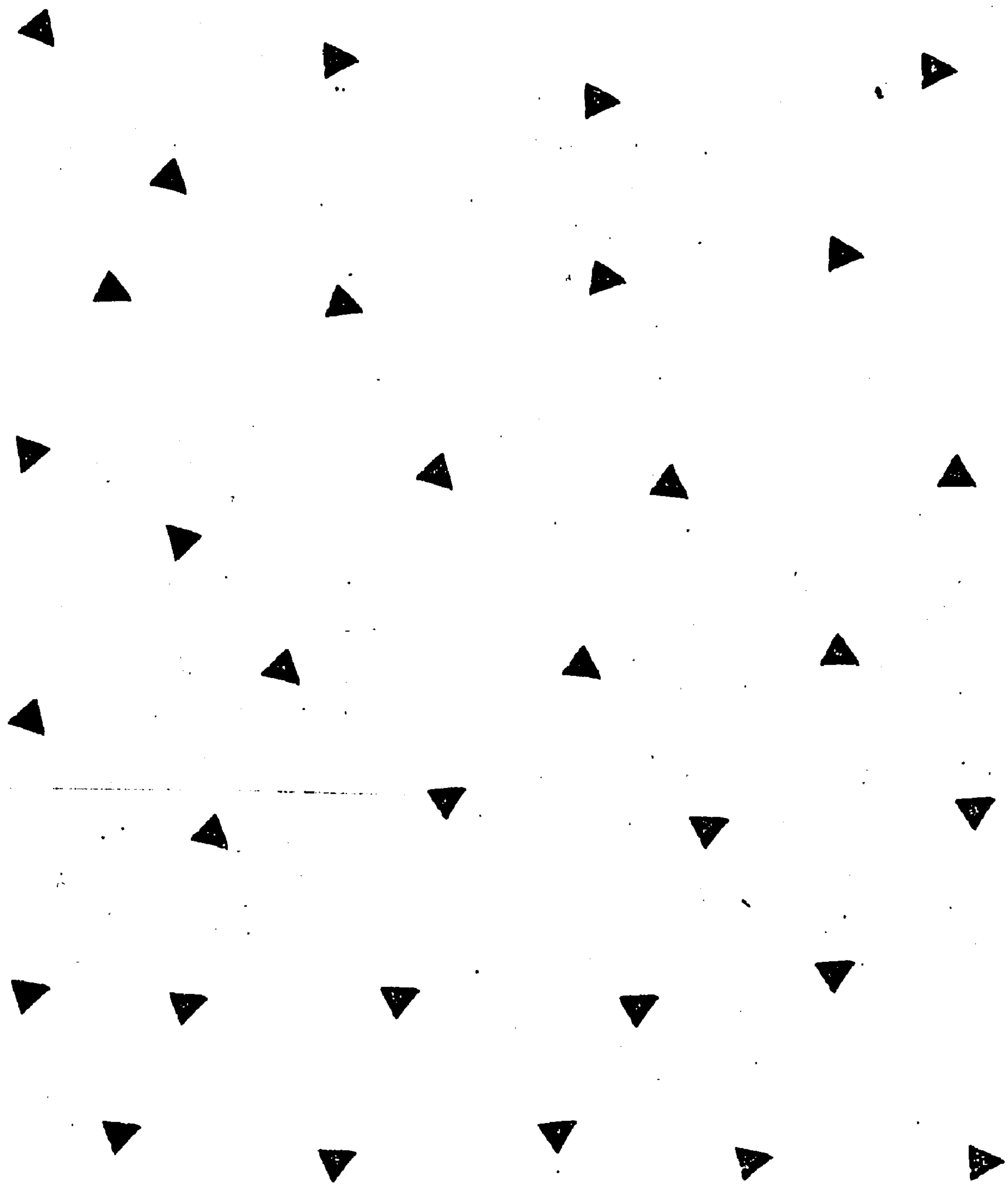

Fig. 4.--Stimulus Pattern No 4 Consisting of 32 Triangles.

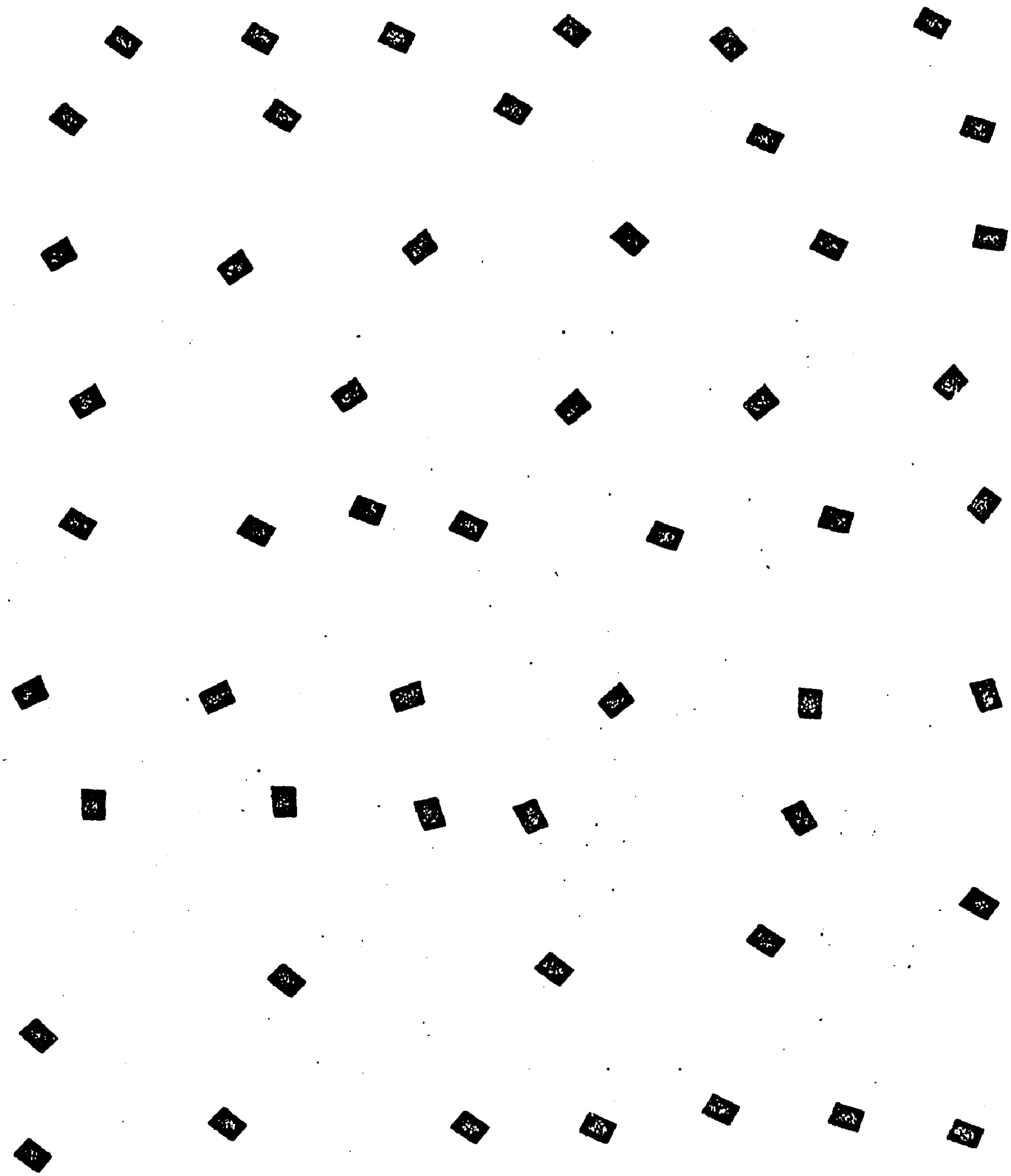

Fig. 5.--Stimulus Pattern No 5 Consisting of 52 Rectangles.

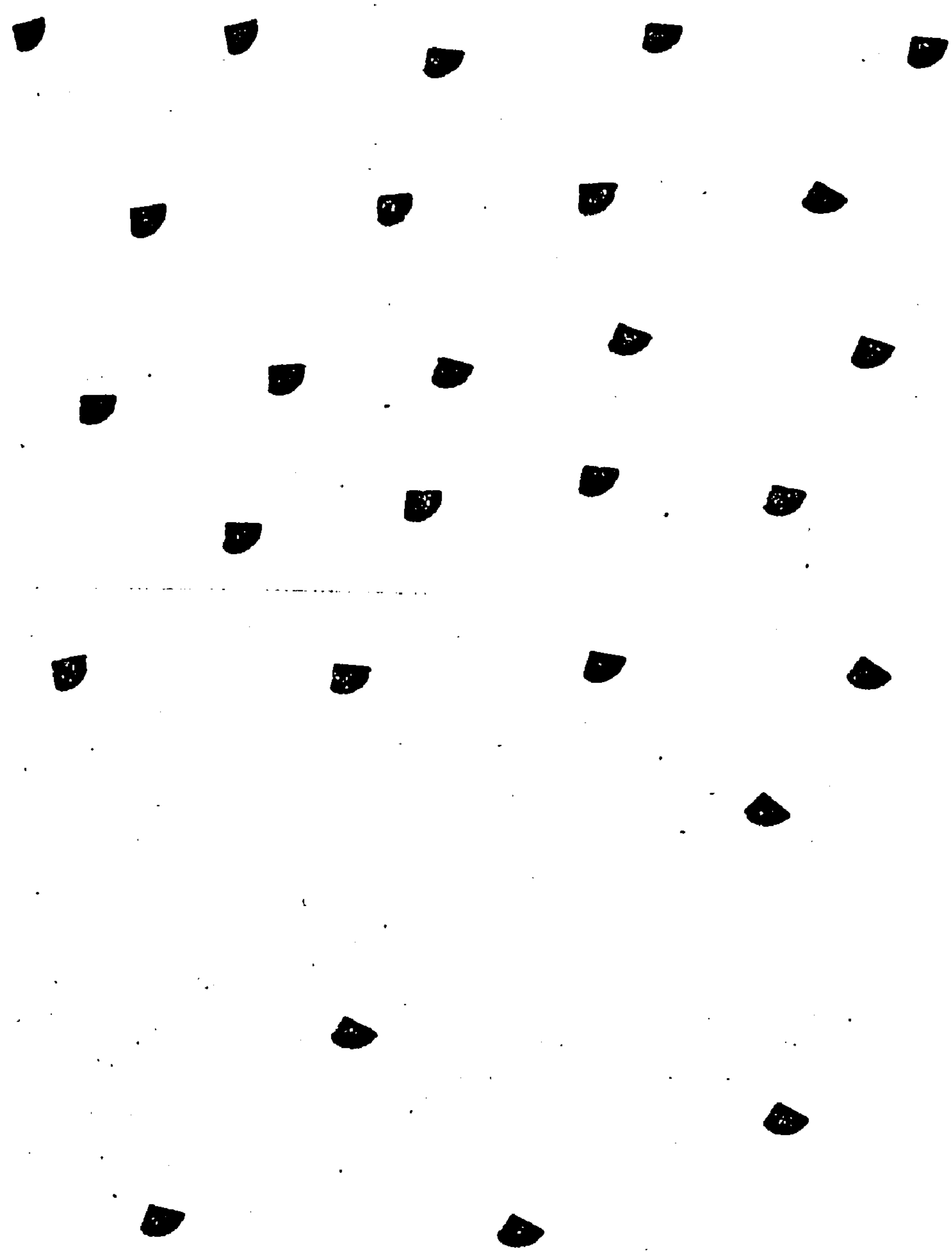

Fig. 6.--Stimulus Pattern No 6 Consisting of 27 Quarter Circles.

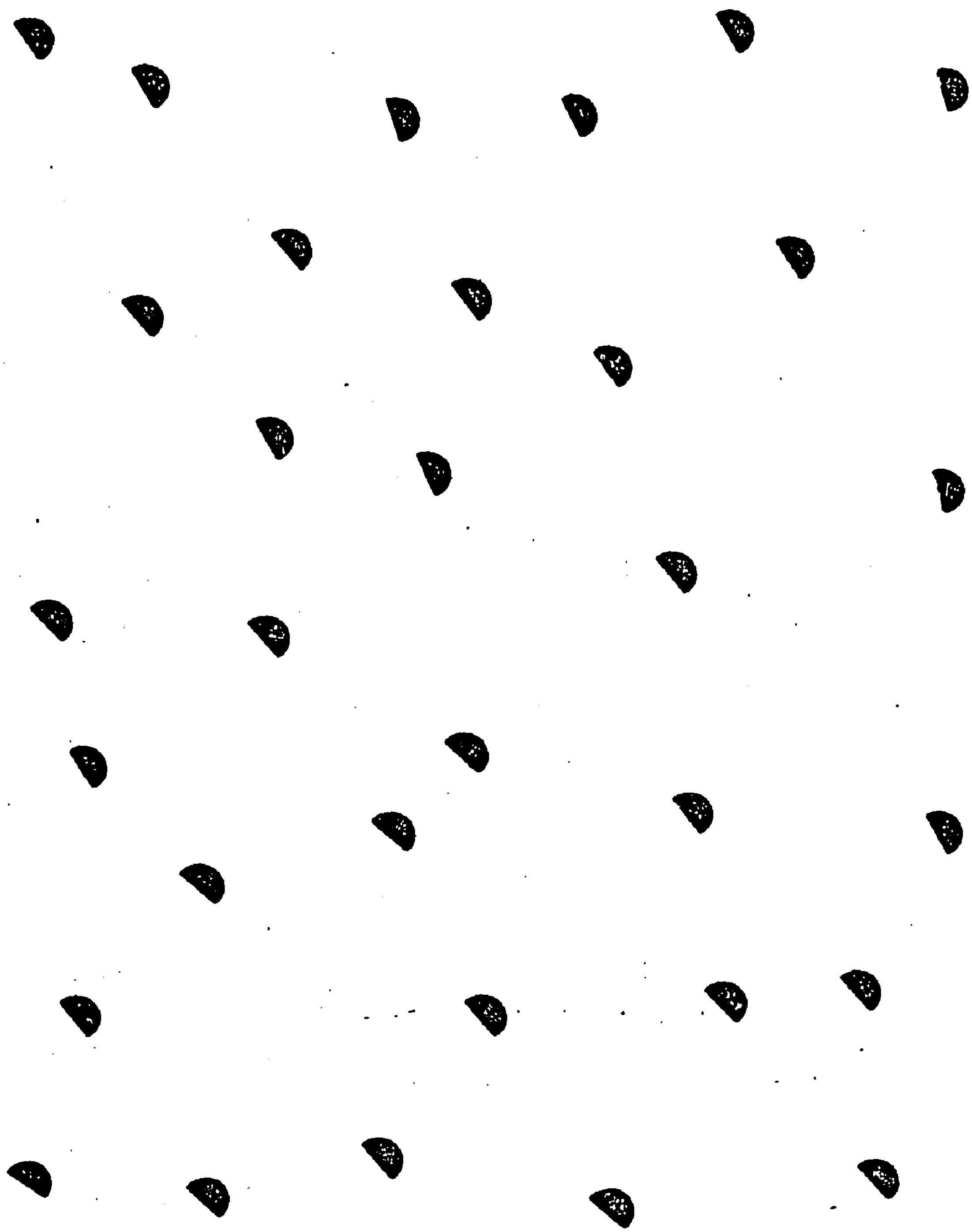

Fig. 7.--Stimulus Pattern No 7 Consisting of 32 Half Circles.

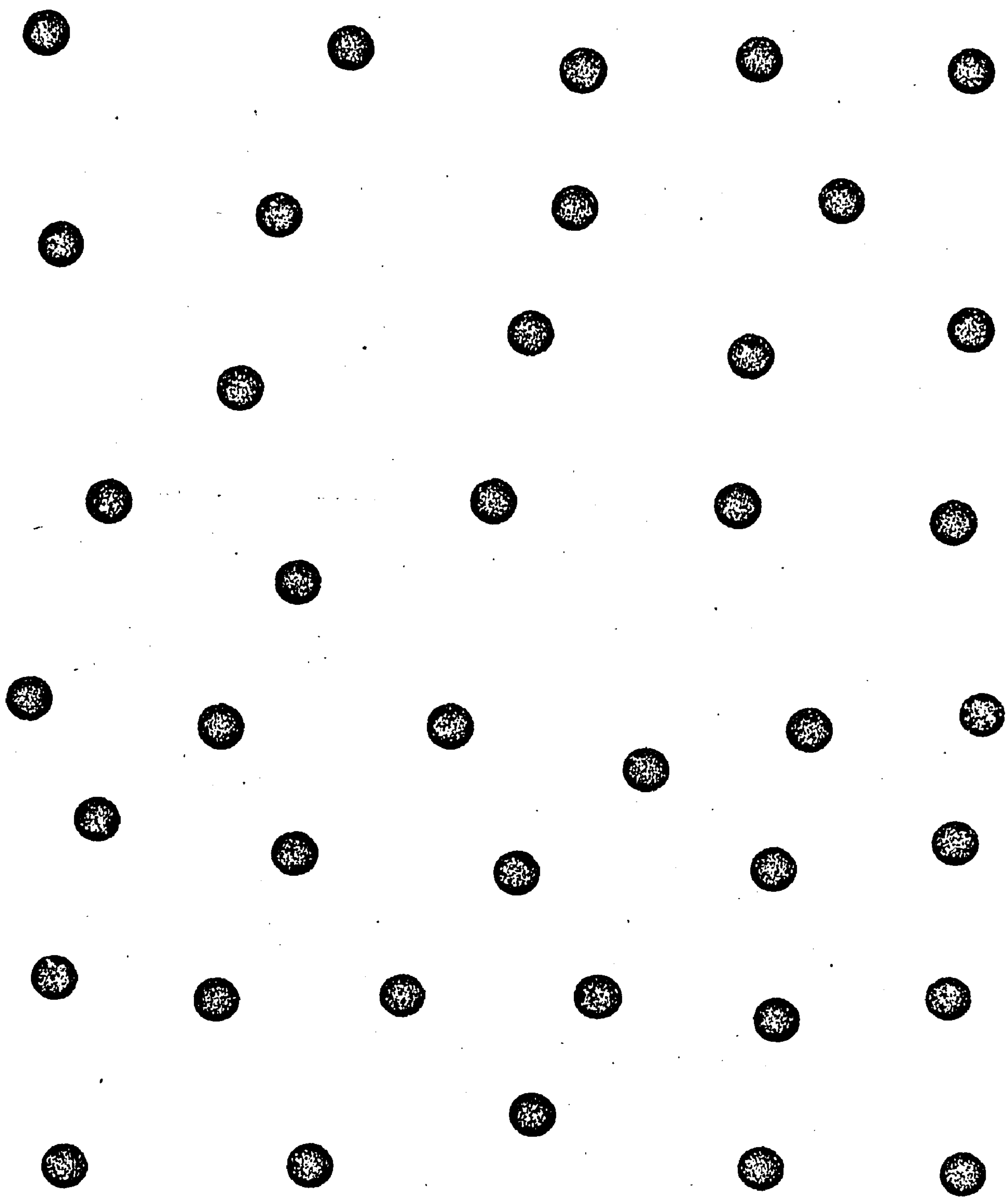

Fig. 8.--Stimulus Pattern No 8 Consisting of 40 Full Circles.

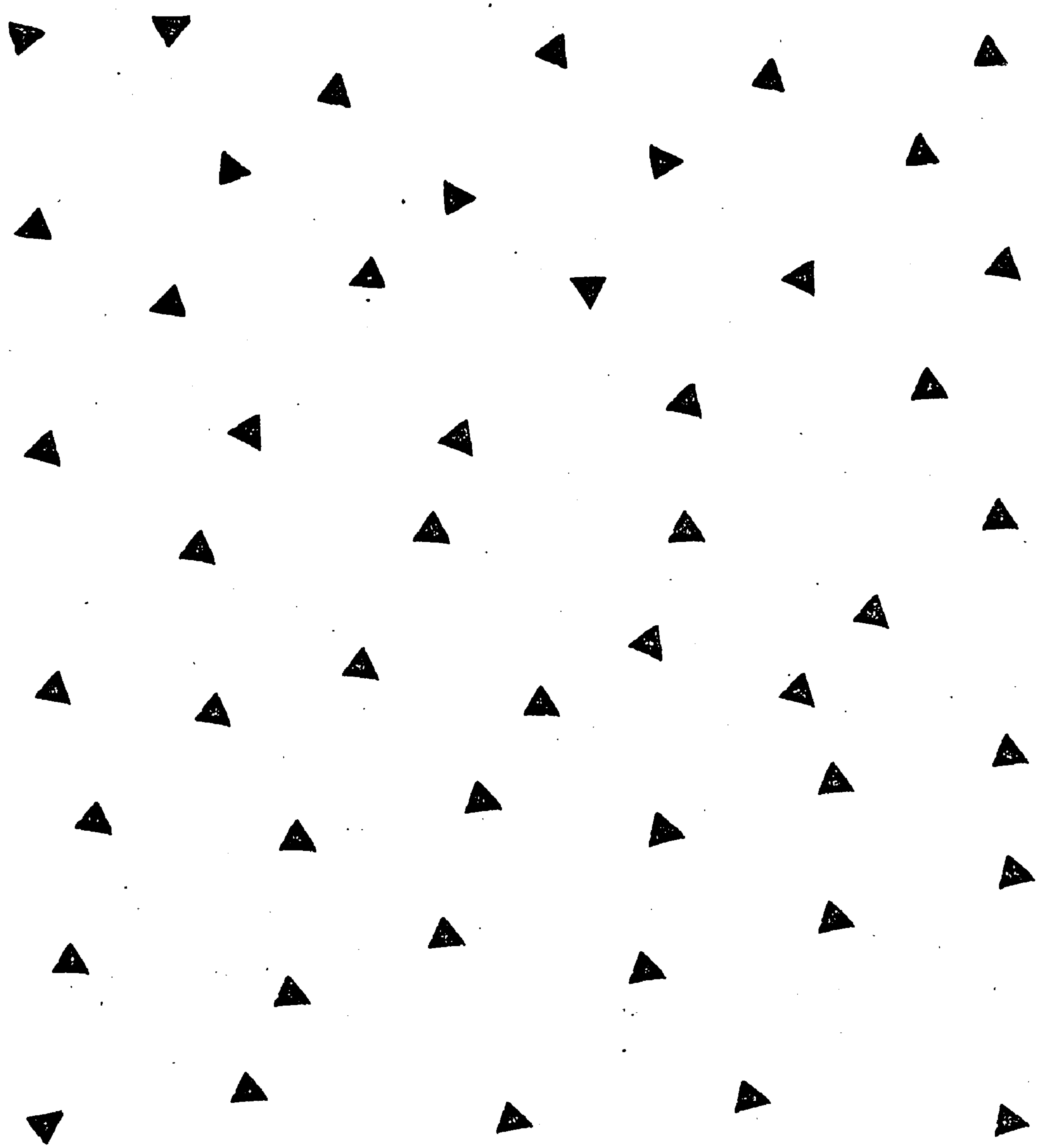

Fig. 9.--Stimulus Pattern No 9 Consisting of 49 Triangles.

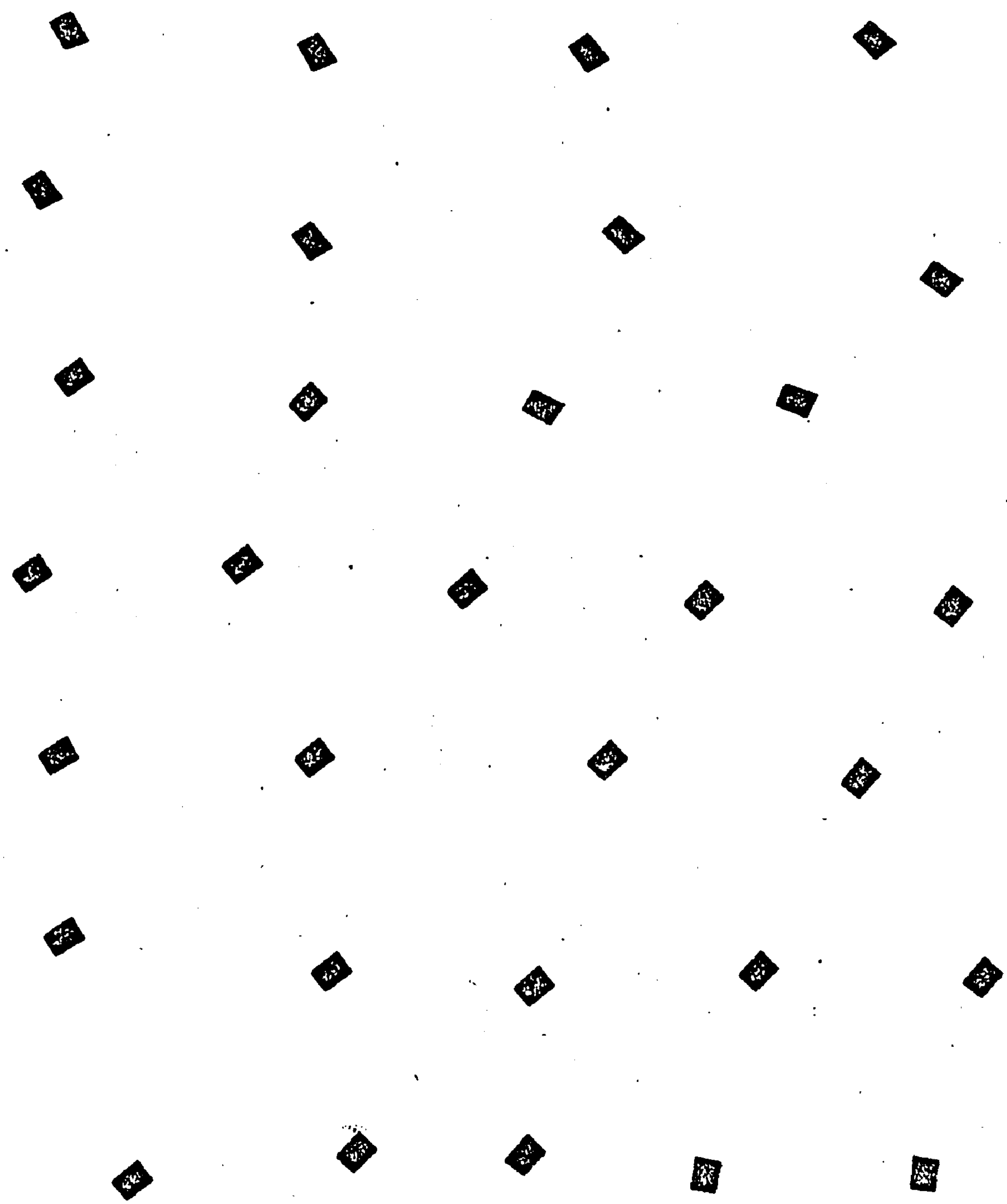

Fig. 10.--Stimulus Pattern No 10 Consisting of 31 Rectangles.

TABLE 3.--The Series Number, Frequency, and Type of Geometric Form of The Ten Stimulus Patterns.

Series Number	Frequency	Geometric Form
1	40	Quarter Circles
2	27	Half Circles
3	25	Circles
4	32	Triangles
5	52	Rectangles
6	27	Quarter Circles
7	32	Half Circles
8	40	Circles
9	49	Triangles
10	31	Rectangles

These 10 stimulus patterns which were printed on white sheets of bond paper were subsequently photographed on Kodak 35 mm Panatomic-X negative film by means of a 35 mm single-lens reflex camera. The processed negatives were then inserted into 35 mm slide-mounts. The series of these 10 slides was the stimulus material used in the experiment.

(b) The stimulus patterns were presented to the subject by means of a Sawyer 500-S slide projector and subject's reaction time was recorded by means of a Sidam anti-magnetic stop-watch.

(c) Subject's verbalizations and his responses to the list of 15 words used in the word association test (Table 4) were recorded on a Wollensak 1500 magnetic tape recorder.

TABLE 4.--List of Word-Association Stimuli.

1.	Tape	6.	Count	11.	Thought
2.	Mike	7.	Subject	12.	Watch
3.	Slide	8.	Vision	13.	Film
4.	Light	9.	Test	14.	Right
5.	Figure	10.	Square	15.	Negative

Subjects

The subjects were 30 male and 30 female students in an introductory psychology class with a student population of over 300 at the University of Arizona. The mean age was 19.3 years for the male and 18.6 years for the female subjects. The subjects were randomly selected from a list of volunteers in the class. The students in the class were given the following information at the time their assistance was solicited:

> This is an experiment on "perceptual judgment". It has to be run after 5:00 P.M. due to lack of rooms during the day. It will take you only 15 to 20 minutes to participate in this experiment. Please put down your name, sex, telephone number, and your quiz section on the attached sheet. You will be contacted by the experimenter to set up a specific time to participate in this experiment which will take place in . . . (room number and the name of the building in which the experimental situation was set up).

Each subject was later individually contacted by telephone to arrange for a specific time. All the 60 subjects were recruited in this manner.

Experimental Design and Variables

The basic design of the experiment consisted of two experimental conditions and one control conditions:

1. Consistent-information Condition (A). Subjects received information in complete *agreement* with their verbalized judgments (congruity).

2. Inconsistent-information Condition (B). Subjects received information in complete *disagreement* with their verbalized judgments (incongruity).

3. Control Condition (C). Subjects received *no* information concerning their verbalized judgments.

Since both male (M) and female (F) subjects were used there were 6 cells in the design, each consisting of n = 10 subjects for a total of N = 60 subjects (Table 5).

TABLE 5.--Tabular Representation of the Experimental Design in Which A = Consistent Information Condition, B = Inconsistent Information Condition, C = Control Condition, M = Male Subject, and F = Female Subject.

	Treatment Conditions		
Subject's Sex	AM	BM	CM
	AF	BF	CF

Briefly, the experimental routine consisted of the steps described below:

(a) Instructions. Depending on the treatment condition to which he was randomly assigned, the subject first familiarized himself with the written instructions (see the section on procedure).

(b) Task. He was presented with the task which consisted of 10 trials. Each trial involved the projection of a slide containing a number of geometric forms which the subject counted and then announced his count to the experimenter.

(c) Feedback. For the entire series of 10 trials the subjects in the AM and AF conditions were informed that the "correct count" was whatever number they had said it was for each slide. The subjects in the BM and BF conditions were informed that the "correct count" was some number other than the number they had announced. The subjects in the CM and CF conditions received no feedback for the entire series.

(d) Interview. During this phase the subject read the written instructions asking him to speak out his thoughts and impressions concerning the experiment.

(e) Word Association Test. The subject read the appropriate instructions; then the stimulus words were read to him aloud and his responses to each stimulus word were recorded.

The independent variable, i.e., an induced cognitive state, was operationally defined in terms of feedback of consistent and inconsistent information, which was theortically assumed to induce the appropriate cognitive states in the subject. Since "cognitive activity" per se has the status of an intervening variable it was defined in terms of three dependent variables in order operationally, albeit indirectly, to infer and measure as much of its compound variance as possible. From among the indirect indices of cognitive activity, verbal behavior is perhaps most readily amenable to quantification. The implicit assumption in the choice of the dependent variables was that the amount of cognitive activity in an individual is reflected indirectly in the amount of relevant verbal behavior he engages in. Thus, the three dependent variables which were assumed to function as indices of the amount of cognitive activity were:

(a) Frequency of relevant statements (i.e., statements concerning <u>any</u> aspects of the experiment and the performance of the subject himself) verbalized by the subject and recorded in his protocol.

(b) Frequency of single words contained in subject's total number of statements.

(c) Frequency of word association responses elicited by means of stimulus words which have some connotative reference to the task (see Table 4).

In addition, it was decided to include these other two dependent variables in an explorative attempt to determine whether they show a meaningful correlation with the independent variable or the other three dependent variables:

(d) Total task reaction-time of the subject.

(e) Sum total of the deviations of subject's counts from the actual correct counts of the stimulus patterns.

Procedure

The plan of the experiment was executed through the successive steps described below:

The subject was called into the experimental situation which contained several desks and chairs, a slide projector, and a tape-recorder. He was asked to sit on a chair facing one of the white walls of the room that served as a projection screen due to lack of a standard screen large enough to accommodate the projected image of the slides at a distance which permitted sufficient enlargement of the small geometric forms on the slides. Subject's name, age, sex, and group assignment were noted on a data sheet.

The subjects assigned to the AM, AF, BM, and BF groups were requested to read the instructions which are reproduced below in their original form:

INSTRUCTIONS

> This experiment is designed to test the accuracy of "perceptual judgment" in human subjects. A set 10 slides will be projected on the wall facing you for a fixed period of time. Each slide contains only one kind of geometric form. Your task is simply to <u>count</u>, as <u>accurately</u> and <u>rapidly</u> as you can, the <u>exact</u> number

> of forms on each slide and announce your count to the experimenter as soon as you can.
>
> As soon as you have indicated your count to the experimenter he will announce the correct number of each count for your information.

The subjects assigned to CM and CF groups read the instructions in a modified form, i.e., the same as the above instructions set except that the last paragraph dealing with information feedback had been deleted from it.

After the subject had read the appropriate instructions he was asked whether he had any questions regarding the task ahead. If any uncertainties were expressed by the subject concerning the instructions, an attempt was made to clarify these before proceeding with the task.

During the "task" phase of the experiment the room lights were turned off. The first slide was projected on the wall and was left on until the subject announced his count, at which time the projected image was blacked out, the relevant feedback was given to the subject, and his count along with his reaction time (i.e., time elapsed between the projection of the image on the wall and subject's announcement of his count, recorded by the stop-watch) were noted on the data sheet, opposite the number of the appropriate stimulus, in the dim illumination of the slide projector. The feedback was administered in accordance with the following scheme:

(a) Subjects in the AM and AF conditions were informed that the "correct count" was whatever number they had said it was for each slide, i.e., regardless of the accuracy of their count. For example, if a subject in one of these two groups indicated that his count for slide No 7 was 40 (which is in fact an incorrect count) he was informed that the correct count was 40.

(b) Subjects in the BM and BF conditions were informed that the correct count was some number other than the number they had announced, i.e., regardless of the accuracy of their count. The pattern of feedback for these two groups was based on this formula: the algebraic sum of subject's count and the appropriate integer (i.e., parallel to the series number of the stimulus) out of a series of 10 integers corresponding to the series of 10 trials. The series of 10 integers and their accompanying signs had been randomly predetermined as: (1) +3; (2) +2; (3) -4; (4) +5; (5) -6; (6) -1; (7) +3; (8) +4; (9) -3; and (10) -2. For instance, if a subject in one of these two groups announced 25 (which is in fact the correct count) as his count for slide No 3, he was informed that the correct count for that slide was 21 (i.e., 25 – 4= 21).

(c) Subjects in the CM and CF conditions received no feedback.

The above procedure was carried out uniformly for all the subjects throughout the series of 10 trials. After the completion of the 10th trial the room lights were turned on.

Next, the subject read the interview instructions (the same for all treatment conditions) which are reproduced below in full:

INTERVIEW INSTRUCTIONS

> In this phase of the experiment we are interested in the relationship between perception and thinking. What you are requested to do is simply to <u>speak out freely</u> whatever thoughts you have regarding this experiment.
>
> You should try to speak out whatever thoughts pass through your head, whether you are sure they are relevant or not. You can, of course, include any questions that you may have, although the answers to these questions will not be given until after you have spoken your thoughts.

Then the subject was questioned to insure that he had understood the instructions; if he indicated doubt about any portions of the instructions, minimal explanation was offered, using caution to exclude any leading remarks which might specifically direct subject's thoughts and ideas.

Since an earlier pilot study had indicated that subjects tend to "block out" in the direct presence of a tape-recorder into which they have to speak, it was decided to try to minimize the interference produced by the presence of a tape-recorder in the experimental situation. Another pilot study, conducted partly to find a solution to this problem, showed that if the presence of the tape-recorder is justified to the subject as "incidental" so that he would not have to talk "into" it directly the interference effect would be considerably decreased. Consequently, in the present experiment, after the subject had finished reading the interview instructions, he was informed in a matter-of-fact way by the experimenter, "I'll try to jot down your statements as fast as I can--at the same time I'll use this tape-recorder in case I missed something I can go back and check it." All the subjects were informed, verbally but uniformly, about the tape-recorder in this manner.

The experimenter then sat on a chair facing the subject, asked him whether he was ready, and started the stop-watch. All through the interview session the experimenter pretended he was busy writing down the subject's statements when in fact he merely scribbled conscientiously on a sheet of paper, out of subject's sight, while subject's verbalizations were being consistently recorded on the magnetic tape.

During the interview sessions whenever a subject explicitly demanded answers to his questions, he was reminded that all of his questions will be written down and will be answered only after he had spoken out his thoughts and the interview had been terminated. Apart from such incidental comments, the experimenter did not interact with the subject, either verbally or through gestures and the like, throughout the duration of the interview.

In order to obtain a uniform base for the comparison of subjects' frequency of verbalizations a temporal cut-off criterion had been established as a result of another pilot study which demonstrated that under the task and interview conditions of the present experiment subjects, on the average, do not engage in more than 3 minutes of "free verbalization." However, since considerable individual differences were noted with respect to this time interval in the same pilot study, it was finally decided to establish a temporal cut-off point such as this: The interview will be considered terminated if 40 seconds are elapsed since subject's <u>last</u> statement, but if subject's pause lasts less than 40 seconds, say 30 seconds, he will be allowed to talk until the 40-second criterion is reached. It should be noted that the subject was not made aware of this temporal cut-off criterion.

As soon as the 40-second criterion was reached, on the basis of stop-watch measurement, the subject was presented with the written instructions for the word association test. The exact instructions for subjects in all the treatment conditions are reproduced below:

INSTRUCTIONS FOR THE WORD ASSOCIATION TEST

> I am going to read to you a list of 15 words. Your task is to indicate, during a 10 second interval for each word, all the words that you are reminded of. You should try to speak out the words as fast as you can and do not think too much about any specific word.

The stimulus words (Table 4) were then successively read to the subject who was allowed 10 seconds of response time for each word. All of the subject's responses were recorded on the magnetic tape following the recording of his interview verbalizations.

Finally, all the subjects were individually requested by the experimenter not to divulge any detail of the experiment to their acquaintances and friends, and the effects of data contamination were explained to them in simple terms. They were also told that the exact nature of the experiment and the answers to the questions they had asked during the course of the interview, could not be explained to them right then since this might contaminate the data still further. They were, however, assured that after the termination of the experimental project the entire research will be described to them during one of their lecture hours in class.

Since an earlier pilot study had indicated that despite the simplicity and low charge of the experimental task some subjects in the BM and BF (inconsistent information) groups tended to show mild but aversive emotional reactions, all the subjects in these two groups were informed after the termination of the experiment that their failure to count the figures correctly was indeed feigned for experimental purposes and they should not, therefore, relate this to any deficiencies in themselves. All these subjects seemed to have accepted this explanation and left the experimental situation with observable relief.

RESULTS

Each subject's verbalizations were transcribed from the magnetic tapes recorded during the experimental sessions and designated as his "protocol." The protocols for all subjects in the six treatment conditions are systematically arranged in Appendices A through F.

In order to obtain a reliable measure of the frequency of relevant statements in each subject's protocol four judges were requested to carefully read all the individual protocols and determine the exact number of relevant statements contained therein. The instructions written for the judges are reproduced here in full:

INSTRUCTIONS FOR THE RATING JUDGES

> The purpose of this rating is to obtain the exact number of <u>relevant statements</u> contained in each of the enclosed protocols. First of all you should familiarize yourself with the materials, instructions, and methods of data collection used in this experiment. Then, by means of the following arbitrary guidelines determine the frequency of relevant statements in each individual subject's protocol:
>
> (1) For the purposes of this rating a "unit" relevant statement is arbitrarily defined as any grammatical structure containing an idea or a reference concerning <u>any</u> detail of the experiment (materials, method, experimenter, experimental set-up, other subjects, subject's own feelings and impressions about any of these, and the like) regardless of its grammatical form and accuracy.

(2) There should be specific reference to some aspect of the experiment. For example, a statement such as "I really don't know what to say about this experiment" is considered to be relevant although a similar statement such as "I really don't know what to say" is not considered relevant.

(3) There may be more than one relevant statement in each sentence structure. The variable to be rated is an "idea," or "reference," or "impression," several of which may be built into a long and somewhat disconnected sentence. You are not rating each particular sentence per se, but rather its ideational content. Thus, you often have to break up an overworked sentence into its referential components.

(4) Do not score closing statements such as "That's all," "No further comments," etc., unless they are in the form of a specific relevant statement or question.

(5) Relevant questions are scored as relevant statements.

(6) Score any statement that goes to qualify another, or a preceding, statement.

The ratings made by the four judges for all 60 subjects in the six treatment conditions, together with the respective means are presented in Appendix G.

Ebel's method for estimating the reliability of ratings by intraclass correlation (Guilford, 1954) was applied to the judges' ratings for all the subjects. The results of these analyses are summarized in Table 6. The reliability estimates were obtained by means of the formula:

$$r_{44} = \frac{\text{Mean Square Subjects} - \text{Error Term}}{\text{Mean Square Subjects}}$$

TABLE 6.--Summary of the Analyses of Variance for Intra-Class Correlation of Ratings by Four Judges for Six Treatment Conditions.

Treatment	Source	Sum of Squares	d.f.	Mean Square	r_{44}
AM	Subjects	319.6	9	35.5	.96
	Judges	24.6	3		
	Error	31.4	27	1.2	
	Total	375.6	39		
AF	Subjects	95.2	9	10.6	.90
	Judges	9.7	3		
	Error	26.1	27	.97	
	Total	131.0	39		
BM	Subjects	4163.1	9	462.5	.97
	Judges	38.5	3		
	Error	207.3	27	7.7	
	Total	4408.9	39		
BF	Subjects	1096	9	121.8	.96
	Judges	17	3		
	Error	93	27	3.4	
	Total	1206	39		
CM	Subjects	499	9	55.4	.95
	Judges	44	3		
	Error	55	27	2.03	
	Total	598	39		
CF	Subjects	348	9	38.7	.96
	Judges	69	3		
	Error	35	27	1.3	
	Total	452	39		

where r_{44} is defined as the intra-class correlation coefficient of reliability of ratings by the four judges.

The obtained correlation indices are quite high indicating that the average inter-judge reliabilities for the ratings in all treatment conditions are high indeed.

Analysis of The Number of Relevant Statements

The mean number of the four ratings of relevant statements for the six treatment conditions were statistically evaluated by means of a two-way analysis of variance in which columns represent the treatment effects and the rows represent the main effects due to sex differences. The summary of this analysis of variance is presented in Table 7.

TABLE 7.--Summary of the Analysis of Variance for the Mean Number of Rated Relevant Statements for Six Treatment Conditions

Source	Sum of Squares	d.f.	Mean Square	F
Sex	156.81	1	156.81	5.17*
Treatments	3033.1	2	1516.55	50.04**
Interaction	80.37	2	40.18	N.S.
Error	1636.97	54	30.31	
Total	4907.25	59		

* Significant at the .05 level of confidence
** Significant at the .001 level of confidence

It is evident from Table 7 that both treatment and sex effects are statistically significant. Inspection of the plot for this set of data (Figure 11) indicates that subjects in the inconsistent-information group (B) produced more relevant statements than the subjects in the no-information (C) and consistent-information (A) groups. Also, subjects in the C groups produced more relevant statements than the subjects in the A groups who gave the least number of relevant statements. Furthermore, male subjects as a whole produced more relevant statements than female subjects, and the sex difference was highest for the BM and BF groups.

In order to further refine the between-group differences with respect to the mean number of relevant statements for all six treatment conditions, a multiple comparison was performed by means of Duncan's new multiple range test (Edwards, 1960). The summary of the results is presented in Table 8.

The differences between all pairs of treatment means of the number of relevant statements are statistically significant at the 77 per cent protection level except for the AM—AF, CM—CF, and AM—CF pairs (i.e., any two treatment means not underscored by the same line are significantly different and any two treatment means underscored by the same line are not significantly different). Since the AM—CF pair of means showed a significant statistical trend it was analyzed separately by means of a t-test for independent means which

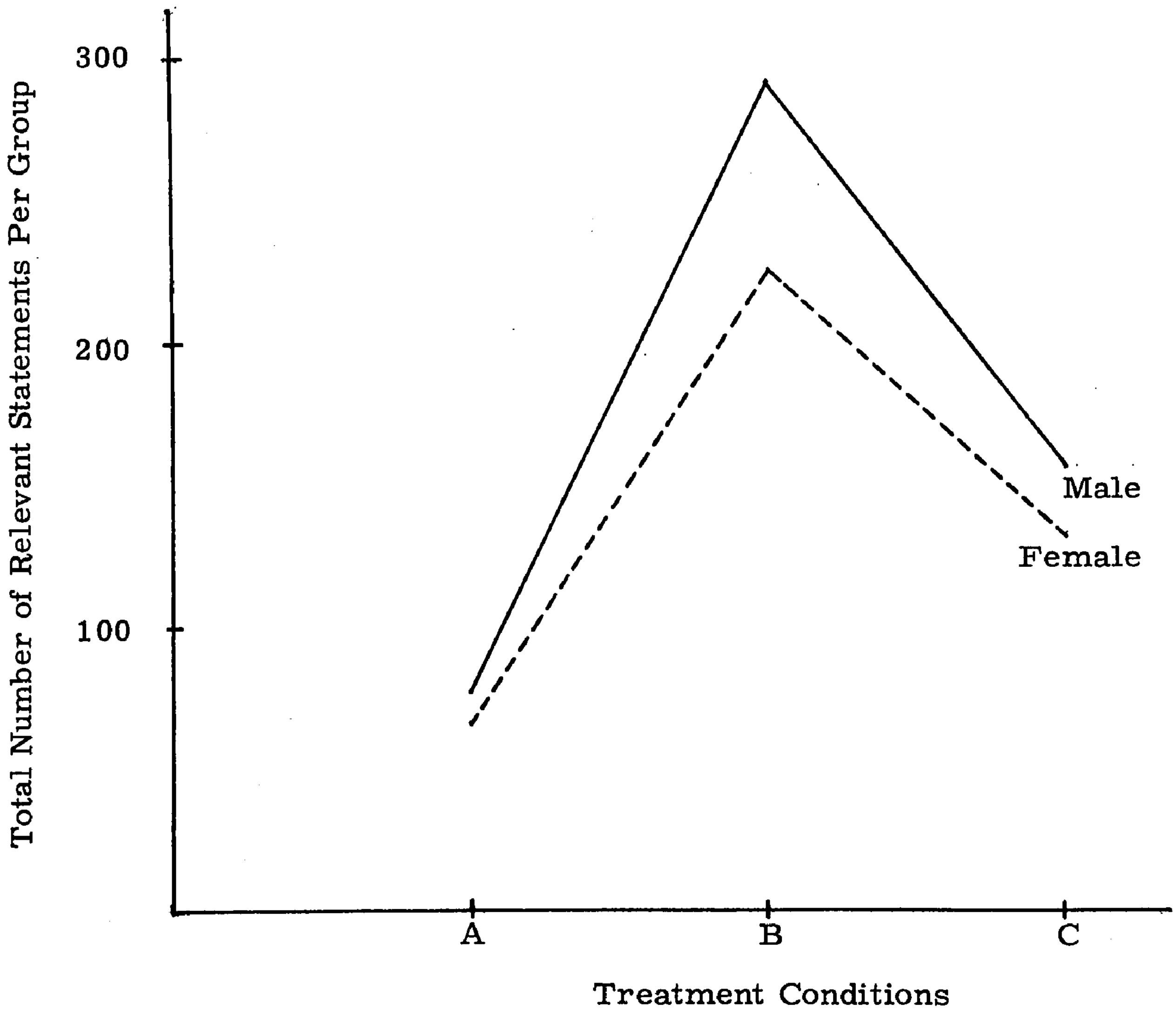

Fig. 11. --Plot of the Total Number of Relevant Statements for all Treatment Conditions.

TABLE 8.--Summary of Duncan's Test Applied to the Differences Between Six Treatment Means for the Number of Relevant Statements.

Means		AF 7.0	AM 8.1	CF 12.4	CM 14.5	BF 21.4	BM 27.9	SSR*
AF	7.0	.	1.1	5.4	7.5	14.4	20.9	R_2=4.87
AM	8.1	.		4.3	6.4	13.3	19.8	R_3=5.13
CF	12.4	.			2.1	9.0	15.5	R_4=5.28
CM	14.5	.				6.9	13.4	R_5=5.4
BF	21.4	.					6.5	R_6=5.5

AF AM CF CM BF BM

*Protection level with α = .05 is given by the formula $(1-\alpha)^{k-1} = (1-.05)^{6-1}$ = 77 per cent.

is a more powerful test than Duncan's range test. A t = 3.1 for the AM—CF pair of means was found to be statistically significant at .01 level of confidence with 18 degrees of freedom.

Analysis of the Number of Words

The total number of individual words in each subject's protocol were counted and tabulated (Table 9). A two-way analysis of variance was performed on these data. The summary of this analysis is shown in Table 10.

Table 10 gives clear evidence of significant effects due to sex and treatment with respect to the total number of individual words in each treatment condition. The plot for these data (Figure 12) shows a

TABLE 9.--Total Number of Single Words in Each Subject's Protocol for all Six Treatment Conditions.

Subjects	AM	BM	CM
1	50	181	104
2	57	446	176
3	65	144	242
4	128	306	163
5	43	229	121
6	58	402	125
7	111	262	151
8	45	322	156
9	112	331	120
10	113	289	202
Subjects	**AF**	**BF**	**CF**
1	77	164	94
2	113	368	82
3	51	238	134
4	53	186	164
5	63	268	98
6	74	196	147
7	69	224	171
8	109	157	152
9	84	145	166
10	78	267	91

TABLE 10. --Analysis of Variance for the Total Number of Individual Words in Subjects' Protocols for the Six Treatment Conditions.

Source	Sum of Squares	d.f.	Mean Square	F
Sex	15747	1	15747	5.32*
Treatments	326692	2	163346	55.16**
Interaction	12121	2	6060.5	N.S.
Error	159906	54	2961	
Total	514466	59		

*Significant at the .025 level of confidence
**Significant at the .001 level of confidnece

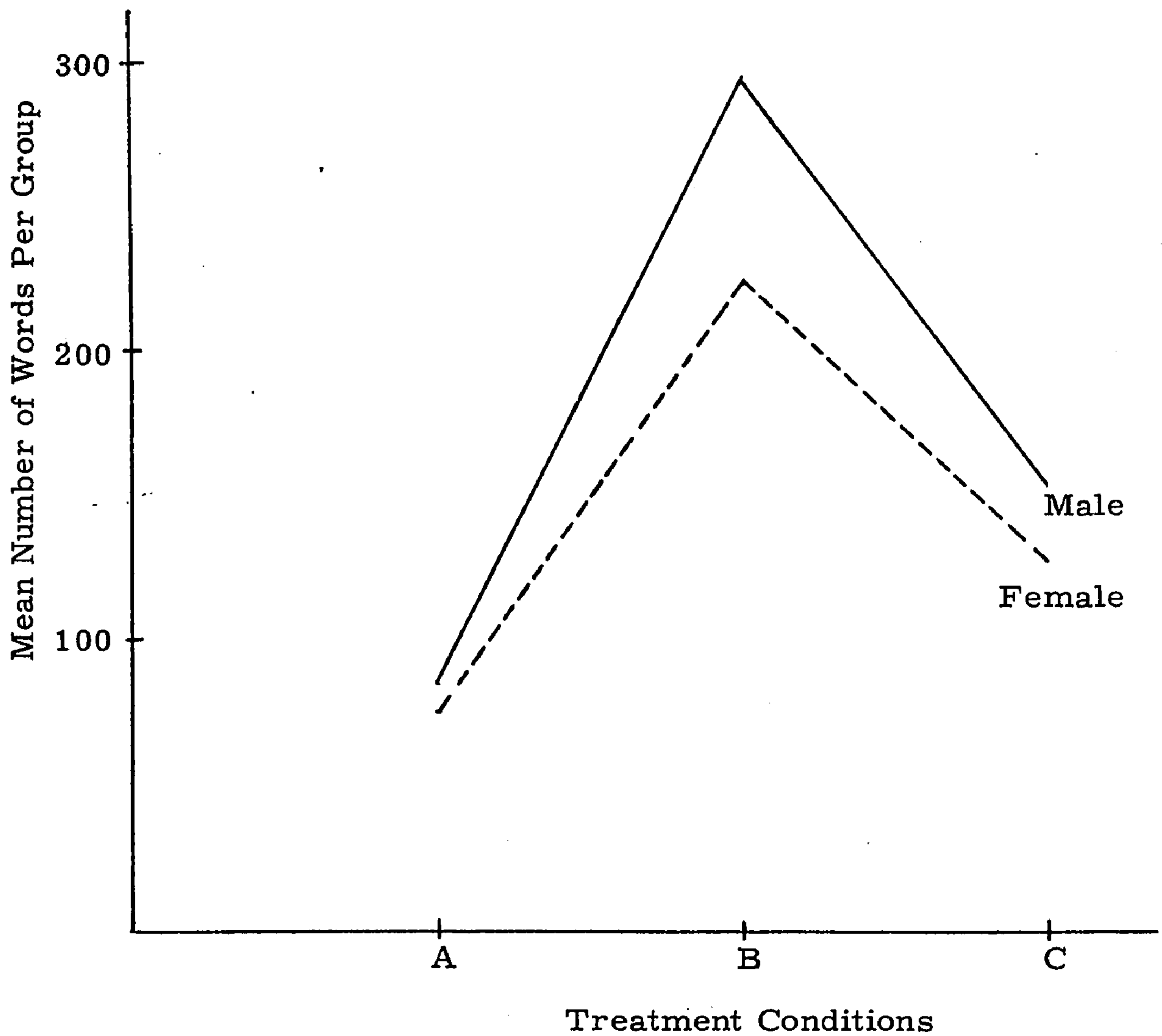

Fig. 12: --Plot of the Mean Number of Words for all Treatment Conditions.

trend similar to the plot of the number of rated relevant statements (Figure 11). Thus, subjects in the BM and BF treatment conditions used more words in their protocols than subjects in the CM and CF groups who in turn used more words than subjects in the AM and AF groups. The significance of the differences among pairs of treatment means was tested by means of Duncan's multiple range test. The summary of the results is shown in Table 11.

TABLE 11.--Summary of Duncan's Test Applied to the Differences Between Six Treatment Means of the Total Number of Words in Subjects' Protocols Within Each Group

Means		AF 77.1	AM 78.2	CF 129.8	CM 156	BF 221.3	BM 291.2	SSR*
AF	77.1		1.1	52.7	78.9	144.2	214.1	R_2=48.1
AM	78.2			51.6	77.9	143.1	213.0	R_3=50.7
CF	129.8				26.2	91.5	161.4	R_4=52.2
CM	156.0					65.3	135.2	R_5=53.4
BF	221.3						69.9	R_6=54.4
		<u>AF</u>	<u>AM</u>	<u>CF</u>	<u>CM</u>	BF	BM	

*Protection level of 77 per cent with α = .05

The differences between all pairs of treatment means of the total number of individual words in the protocols are significant at 77 per cent protection level except for the AM–AF and CM–CF pairs. This finding essentially fits in with the results of the Duncan's test for the number of relevant statements. The similarity of the significant indices and the statistical trends in these two sets of data suggested that perhaps this close relationship may have spuriously boosted the F ratios for the number of relevant statements. To investigate this possibility an analysis of covariance was performed to make allowance for the uncontrolled effect of the covariate X (i.e., the number of words used by subjects) on the number of relevant statements (Y). Table 12 presents the summary of the analysis of variance for the number of relevant statements (Y) by the adjustments of covariate X.

TABLE 12.--Analysis of Variance for the Number of Relevant Statements (Y) by Covariance Adjustments for the Number of Words Used by Subjects (X).

Source	Sum of Squares	d.f.	Mean Square	F
Sex	180.43	1	180.43	5.9*
Treatment	1270.54	2	635.27	20.8**
Interaction	98.98	2	49.49	N.S.
Adjusted Error	1612.4	53	30.42	
Total	3162.35	58		

*Significant at .05 level of confidence

**Significant at .001 level of confidence

Despite the logical expectation of a high correlation between the total number of words and the total number of relevant statements in subjects' protocols Table 12 clearly shows that the significance of the Sex and Treatment F ratios has not been altered significantly as a result of covariate adjustments. This simply means that the treatment groups differed significantly among themselves with respect to the total number of relevant statements, regardless of whether the subjects within any particular group varied their total word output. This statistical analysis still supports the inference that BM and BF groups had a significantly greater output of relevant statements than CM and CF groups which in turn produced significantly more relevant statements than the AM and AF groups which had the fewest number of relevant statements. The significant differences among these same treatment groups regarding the total number of individual words produced by the subjects sill obtains, however.

Analysis of the Number of Word-Association Responses

The total number of word-association responses given by the subjects in the six treatment conditions is shown in Table 13. The summary of the analysis of variance for the data in Table 13 is presented in Table 14.

TABLE 13.--Total Number of Word Association Responses for The Six Treatment Conditions

Subjects	AM	BM	CM
1	46	55	49
2	42	88	47
3	48	55	57
4	64	65	46
5	52	55	57
6	45	68	116
7	66	47	87
8	47	68	53
9	44	69	48
10	54	47	43
Subjects	**AF**	**BF**	**CF**
1	51	40	65
2	93	52	47
3	51	103	56
4	59	69	76
5	38	64	68
6	89	51	58
7	56	45	57
8	68	62	44
9	58	54	68
10	27	53	40

TABLE 14.--Summary of The Analysis of Variance of the Total Number of Word-Association Responses in the Six Treatment Conditions.

Source	Sum of Squares	d.f.	Mean Square	F
Sex	19.27	1	19.27	N.S.
Treatment	339.74	2	169.87	N.S.
Interaction	374.53	2	187.265	N.S.
Error	14630.8	54	270.94	
Total	15364.34	59		

Table 14 indicates that the word-association responses given by the subjects in the six treatment conditions were not systematically affected by the independent variable and their variance is purely due to chance. This inference can also be drawn from the plot of the word-association responses in Figure 13 which does not reveal any significant trends.

Analysis of Reaction Times

The reaction times of the subjects in the six treatment conditions are shown in Table 15. An analysis of variance performed on this set of data yielded results analogous to the results of the word-association responses. There were no significant differences between the groups which could be attributed to systematic effect of the independent variable. The plot for the reaction time data (Figure 14) tends to confirm this conclusion.

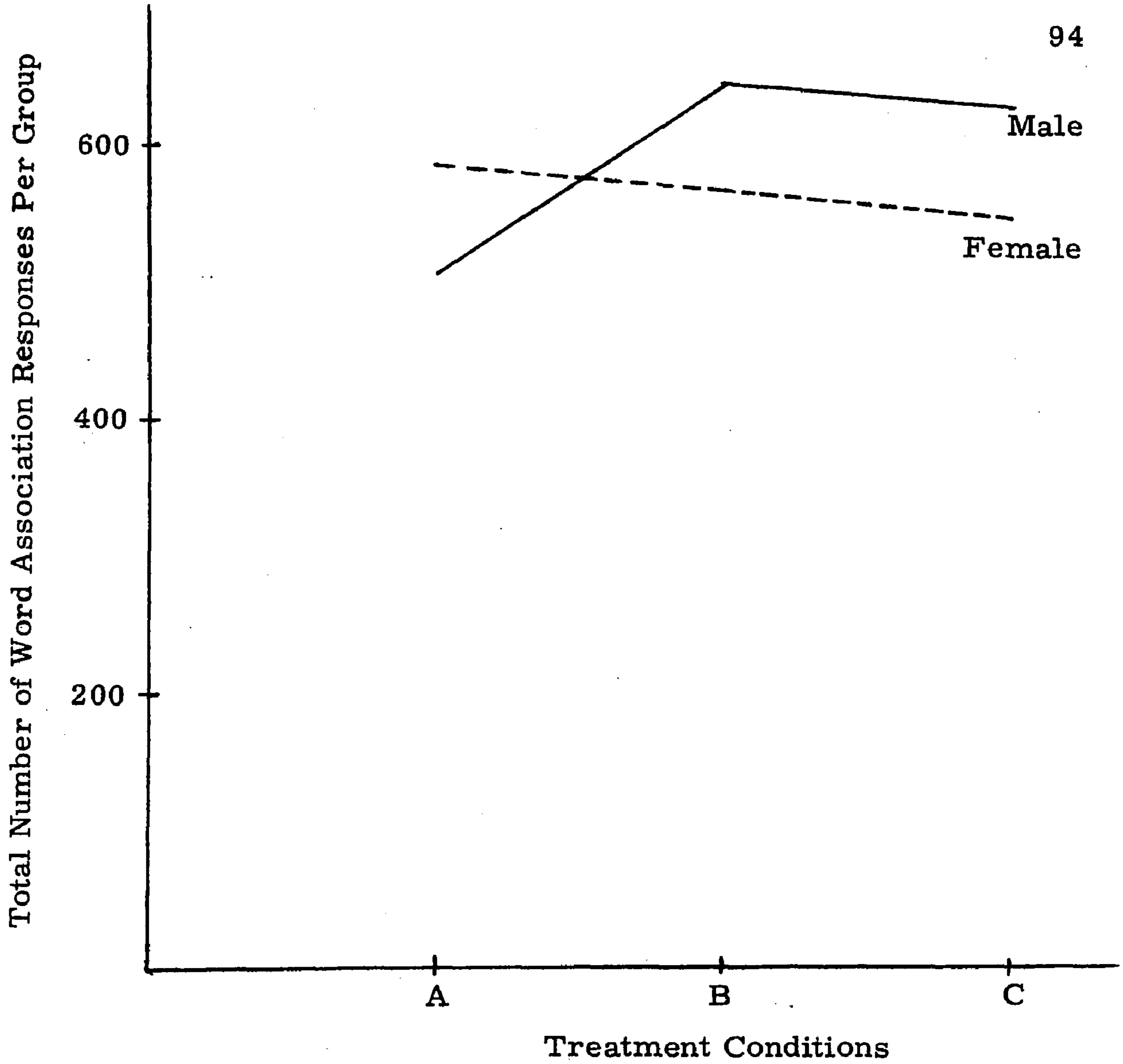

Fig. 13.--Plot of the Total Number of Word Association Responses for all Treatment Conditions.

TABLE 15.--Reaction Times of Subjects in Each of The Six Treatment Conditions

Subjects	AM	BM	CM
1	93	177	172
2	95	140	61
3	103	131	150
4	133	132	147
5	129	132	140
6	154	81	125
7	162	95	137
8	104	166	138
9	134	51	175
10	114	130	224
Subjects	**AF**	**BF**	**CF**
1	181	154	150
2	119	156	137
3	97	176	167
4	130	146	178
5	141	98	151
6	100	109	129
7	105	174	205
8	41	163	58
9	70	145	123
10	124	100	75

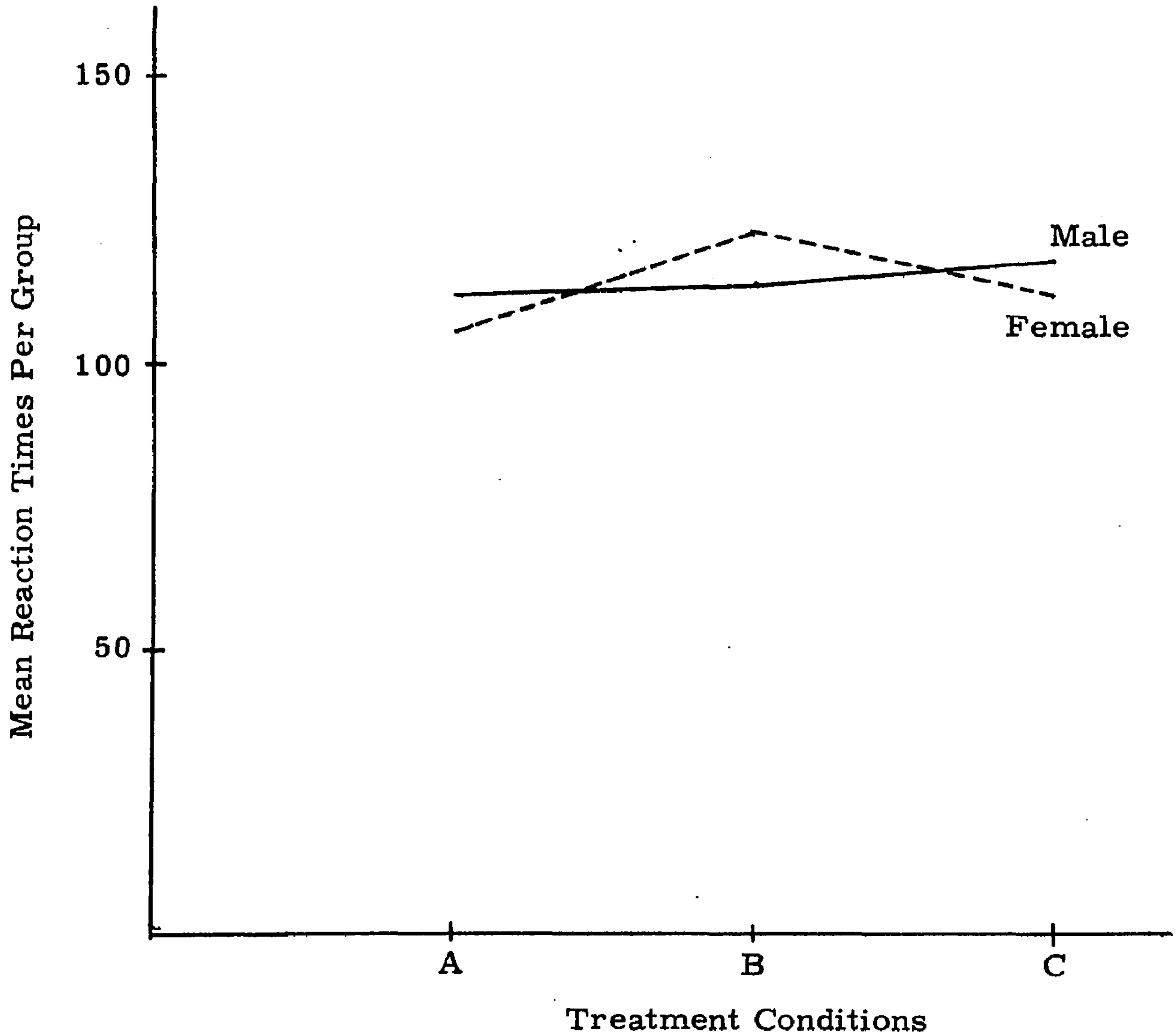

Fig. 14.--Plot of the Mean Reaction Times for all Treatment Conditions.

Analysis of Stimulus Counts

This last statistical analysis deals with the extent to which subjects in each of the six treatment conditions deviated in their stimulus counts from the correct counts for the same stimuli. The raw data for the deviations of subjects' counts from the correct counts, which are shown in Table 16, were obtained by this formula:

$$D = \text{Sum } |CC - SC|$$

where: D = sum of the absolute deviations from the correct count, CC = correct count of the stimulus, and SC = subject's count of the stimulus. The analysis of variance for this data is summarized in Table 17. The analysis of variance together with the plot of the data (Figure 15) show that the only significant effect with respect to the deviations of the subjects' counts from the correct counts is due to treatment. Duncan's multiple range test was used to analyze these data further. The results of this test are summarized in Table 18.

The multiple comparisons of pairs of means of the above data in Duncan's test suggest that both male and female subjects in the treatment condition A (consistent-information) tended to deviate, in terms of the difference between their counts and the correct counts of the stimulus patterns, significantly more than the subjects in the other four treatment conditions. There is also an absence of salient sex differences between these two groups although female subjects in the AF group tended to deviate slightly more than the male subjects

TABLE 16.--Sum of Deviations of Subjects' Counts from The Correct Counts of The Stimuli for The Six Treatment Conditions

Subjects	AM	BM	CM
1	25	21	23
2	19	31	19
3	67	6	9
4	46	21	12
5	13	23	22
6	80	7	18
7	34	11	23
8	50	8	22
9	29	15	19
10	34	27	18
Subjects	**AF**	**BF**	**CF**
1	13	15	4
2	37	2	1
3	28	28	3
4	124	20	2
5	64	37	7
6	12	15	34
7	50	8	9
8	115	27	24
9	59	5	39
10	24	1	21

in the AM group. It is also clear that there are no significant sex or treatment differences between the BM, BF, CM, and CF groups.

TABLE 17. --Summary of The Analysis of Variance of The Deviations of Subjects' Counts from the Correct Counts of The Stimuli.

Source	Sum of Squares	d.f.	Mean Square	F
Sex	96.27	1	96.27	N.S.
Treatment	11781.04	2	5890.52	14.38*
Interaction	827.03	2	413.515	N.S.
Error	22119.0	54	409.61	
Total	34823.34	59		

*Significant at .001 level of confidence

TABLE 18. --Summary of Duncan's Test Applied to The Deviations of the Subjects' Counts from The Correct Counts in the Six Treatment Conditions

Means	CF 14.4	BF 15.8	BM 17.0	CM 18.5	AM 39.7	AF 52.6	SSR*
CF 14.4		1.4	2.6	4.1	25.3	38.2	R_2=17.9
BF 15.8			1.2	2.7	23.9	36.8	R_3=18.8
BM 17.0				1.5	22.7	35.6	R_4=19.4
CM 18.5					21.2	34.1	R_5=19.8
AM 39.7						12.9	R_6=20.2

CF BF BM CM AM AF

*Protection level with α = .05 is 77 per cent.

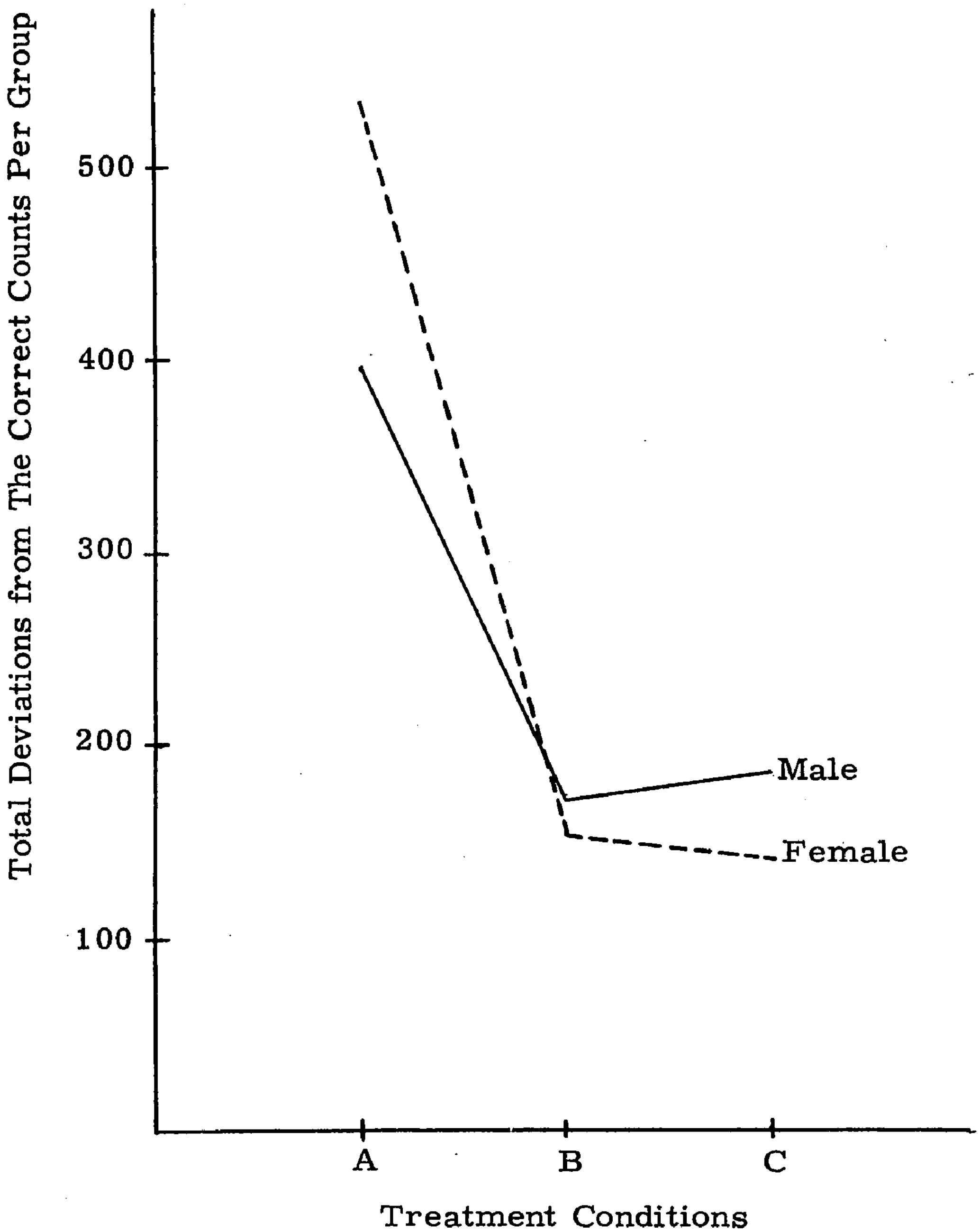

Fig. 15.--Plot of the Total Deviations from the Correct Counts for all Treatment Conditions.

DISCUSSION

The statistical treatment of the data produced the findings summarized below on the basis of the dependent variables:

(1) Relevant Statements. Both male and female subjects in the inconsistent-information groups produced significantly more relevant statements than male and female subjects in the control groups, who in turn gave significantly more relevant statements than male and female subjects in the consistent-information groups. Also, male subjects as a group produced significantly more relevant statements than female subjects. Within the individual pairs of matched groups only the male subjects in the inconsistent-information group produced substantially more relevant statements than female subjects in the corresponding group. Moreover, it was found that these significant inter-group and inter-sex differences still obtain when the effect of the total number of words on the total number of relevant statements is statistically controlled by covariance adjustments.

(2) Single Words. The group differences with respect to the total number of single words produced by the subjects closely parallel the group differences found for the total number of relevant statements. Again, both male and female subjects in the inconsistent-information groups produced significantly more words than the male and female

subjects in the control groups, who in turn produced substantially more words in their protocols than male and female subjects in the consistent-information groups. Also, male subjects as a group produced significantly more words than female subjects. Within the individual pairs of matched groups only the male subjects in the inconsistent-information group produced significantly more words than female subjects in the corresponding group.

(3) Deviations From the Correct Counts. Both male and female subjects in the consistent-information groups deviated, in terms of the difference between the number of their counts and the actual correct counts of the stimulus patterns, significantly more than the inconsistent-information and control groups. Male and female subjects in the inconsistent-information and control groups did not, however, differ substantially among themselves.

(4) Word Association Responses and Reaction Times. There were no significant differences between any of the treatment groups with respect to these two variables. It is clear that in the existing experimental design these two variables did not contribute significantly to the total treatment effects observed for the other three variables.

Prior to a detailed discussion of the overall evidence and its relationship to the hypothesis with which this study was concerned, an attempt will be made to interpret the sex differences obtained in this study since the predictions which bear on the results of the study do not explicitly involve sex differences.

Male subjects, as a group, tended to produce more relevant statements than female subjects. This sex difference was also observed with respect to the total number of words, i.e., male subjects used more words in their protocols than female subjects. There was no significant difference, however, with respect to the deviation of subjects' counts from the actual correct counts of the stimulus patterns, although female subjects in the consistent-information group showed a tendency to deviate from the correct counts slightly (not significantly) more than male subjects.

The sex difference observed in the subjects' performance somewhat corroborates the results of a study by Wallach and Kogan (1959) concerning the relation of sex differences and judgmental processes. These workers found that women tended to be more conservative than men when they were uncertain about their decisions and judgmentally more extreme than men when they were certain about their decisions. In terms of the present study, women in those groups with decisional uncertainty (i.e., inconsistent-information and control groups, the former being the least certain and the latter next to least certain due to the presence of inconsistent feedback in the former and no feedback in the latter group) tended to be consistently lower than men with respect to the amount of verbal productivity. Their judgmental extremeness, however, in situations involving decision certainty (i.e., the consistent-information group which received consistent feedback throughout) was not entirely supported in this study except in terms of their deviations from the correct counts which showed that they tended to be somewhat more, but not significantly so, extreme than men in their deviations. Thus, their low verbal productivity is indeed the significant sex differential.

Several explanations may be offered for this finding:

1) Men are threatened more readily in situations which directly or incidentally challenge their intellectual abilities. They are, thus, more prone to become cognitively involved and hence make more intense efforts to allay the threat than women, especially when they are confronted with another male figure, i. e., the experimenter. The verbal description of their thought content and their behavior in the experimental situation could be construed as efforts in the direction of defending against such threats.

2) It is equally plausible that women deal with cognitive uncertainty and discomfort by resorting to more affective and less conceptual mechanisms. Specifically, instead of intellectual justification of their failure or discomfort they may tend to utilize denial and devaluation, e. g., "I really didn't think <u>that</u> much about this experiment," or "You can't think of very much counting little silly dots." Or, considering their greater deviations from the correct counts which may signify something akin to "carelessness," they may be more apt to readily dispense with discomfort by immediate and overt action.

3) Or, as Wallach and Kogan have suggested, female conservatism in subjectively uncertain situations may be due to their learned fear of punishment.

The activation hypothesis with which this experiment was specifically concerned does not fare very well in the light of the obtained evidence. Not only was the prediction derived from this hypothesis not borne out; it is also evident that the results point in a direction opposite the one suggested by the hypothesis. Since the activation model cannot be readily dismissed on the basis of only one set of data, it is necessary to suggest some of the more plausible reasons for the lack of fit between the data and the theory.

The most obvious difficulties seem to lie in a possibly inadequate experimental design. It is quite likely that the assumption of the activation hypothesis concerning the equivalence of activation intensities at both ends of the continuum of cognitive activity was not met. This means that despite detailed planning of the experiment the question was never raised as to whether induced congruity and incongruity through information feedback do in fact possess comparable valences along an intensity dimension. Simply stated, an individual is apt to react more intensely if he is subjected to disagreement than when he receives agreement concerning the <u>same</u> issue. Equal amounts of agreement and disagreement in terms of plain information-feedback do not seem to result in equally intense cognitive activity. They have to, therefore, be equated in terms of their intensity by purely empirical means in view of the enormous variability of subjective cognitive content. This may be achieved, for instance, through equating an intermittent series of disagreements with a continuous series of monotonously persistent agreements. This clearly implies that in order for congruity to assume drive properties for cognitive activity it should result in a cognitive state comparable to the subjective experience of extensive boredom and monotony. This rather extreme kind of cognitive state cannot seemingly be induced by mere agreement feedback concerning information processes.

This observation ties in with another possible flaw in the design of the experiment. Assuming that subjects in all the treatment conditions began the experimental task with randomly comparable cognitive "sets," it is likely that immediately after the first trial their perception of the task was significantly altered as a function of the type and the presence or absence of feedback, so that subjects in the consistent-information groups did not have the same attitudinal sets after the first trial as the subjects in the inconsistent-information and control groups. More clear results might have been obtained, had the information feedback been administered after the last trial for all subjects.

Another important source of variation may have been the subjective experience of the subjects concerning "success" and "failure" in the task. It is possible that as a result of cultural conditioning subjects in the consistent-information groups were unable to talk extensively or "brag" about their success in the task, although they might have had the urge to do so, whereas the sense of failure in the other subjects compelled them to engage in verbal justifications. From a cultural view, one seems to need less justification for success than for failure.

Thus, the design problems with respect to an adequate test of the activation hypothesis seem to be reducible to a lack of matching between the congruent and incongruent conditions along several variable dimensions. Consequently, the hypothesis per se cannot be unequivocally dismissed. Its utility and relational validity have to rest on a more adequate experimental base than could have been foreseen on this first attempt.

The prediction derived from Festinger's theory seems, superficially, to have been borne out by the results of this study. The dissonance produced in the inconsistent-information groups as a result of dissonant feedback would seem to have given rise to more intense efforts in these subjects toward reduction of the dissonance through verbal behavior, in contrast to subjects in the consistent-information groups who, theoretically, did not experience dissonance, and consequently did not engage in dissonance-reducing behavior.

Closer analysis, however, reveals that although the present evidence can be grossly "fitted" with the prediction from Festinger's theory this would not provide unambiguous supportive evidence for the theory because the data for the control groups and also the way subjects resolved their hypothetical dissonance do not seem to be entirely consistent with the derivations from Festinger's model.

The data for the control groups can be aligned with the dissonance model only if it is assumed that, first, decision-making invariably leads to the creation of dissonance and, second, that the experimental task was in fact perceived by the subjects to involve decision-making.

It is difficult to determine the veracity of the latter assumption at this stage. The former assumption, however, is explicitly made by the dissonance model: All decisions or choices result in dissonance to the extent that the alternative not chosen contains positive features which make it attractive also, and the alternative chosen contains features which might have resulted in rejecting it. Hence, after making a choice people seek evidence to confirm their decision and so reduce dissonance. This prediction was confirmed by Ehrlich et al. (1957) who found that new car owners noticed and read ads about the cars they had recently purchased more than ads about other cars. Results of other studies by Brehm (1956) and Brehm and Cohen (1962) were also confirmatory.

Even if the experimental task in the present study can be conceived to have involved decisional processes, it cannot be assumed that the subjects knew exactly what the decision alternatives were supposed to be. The structure of the task and feedback situations was such that the subjects in the inconsistent-information and control groups could not know the exact number of possible alternatives (e. g., whether on a particular slide the correct count was 38, 39, 40, 41, 42, etc.).

Moreover, the subjects in these two groups could not determine what the correct alternative really was. Thus, it is less equivocal to assume that the situation, as perceived by the subjects, was mainly concerned with the exchange of, and exposure to, information. As such, the dissonance model predicts that in situations involving exposure to information individuals will seek out information which tends to reduce their dissonance and will avoid information which tends to increase it. The data obtained in this experiment appear to contradict this prediction because the subjects in the inconsistent-information and control groups (dissonance) produced more relevant statements than subjects in the consistent-information groups (consonance), i. e., subjects in the four dissonant groups actively engaged in information exchange which was directly and explicitly related to the dissonance-reducing task (Cf. the content of protocols for subjects in the BM, BF, CM, and CF groups in the Appendices).

It is doubtful whether an individual's detailed description of processes which are supposed to lead to dissonance in him (i.e., subject's account of, say, his method of counting the figures and his justifications for his poor performance) can be considered as avoidance of dissonant information. It is more likely that the subjects in the inconsistent-information and control groups actively sought out "dissonant" information. This interpretation is in line with the evidence reported by Feather (1963) in an investigation of dissonance and information receptivity in which he found that regular cigarette smokers expressed greater interest in an article concerning cigarette smoking and lung cancer than did nonsmokers, i.e., these individuals chose dissonance-producing information over communication which was supportive of their views. In an effort to reconcile this seemingly contradictory finding with the dissonance model Feather (1963) has suggested that states of cognitive dissonance are more likely to influence an individual's evaluation of information than his sensitivity to information. The experiments by Mills et al. (1959) and Rosen (1961) also demonstrated that individuals seek information about their choice, even if the information is adverse. Steiner (1962) has concluded that the question of whether people prefer to receive information supportive of their own views has not been unequivocally settled. Freedman (1965) has also indicated that the problem of choice between dissonant and consonant information is a good deal more complex than it has been assumed so far.

The implicit notion in the theoretical propositions concerning means of dissonance reduction is that the individual will select the least effortful alternative. But, if it is assumed that individuals choose that method of dissonance reduction which provides the most stable resolution of dissonance the fact that subjects do seek out supposedly adverse information is no longer paradoxical since this may afford considerable stability to the resolution despite the discomfort of the effort.

Even though the above considerations tend to narrow the gap between the dissonance model and the data obtained in this study, the fact still remains that almost any major prediction derivable from the dissonance model has to be amended in order to account for the new and discrepant data.

The present data do not lend consistent and unequivocal support to Festinger's position. There are simpler and more economical formulations that seem to account for the obtained evidence better than the dissonance model. One such formulation derives from the experimental work of Zeigarnik (1938). Bluma Zeigarnik, one of Lewin's pupils, conducted an experiment in 1927 to investigate the ability to recall finished versus unfinished tasks. According to field theory, interrupting a subject in the middle of a task should have the effect of leaving him in a state of tension and disequilibrium. Moreover, the interruption should serve as a "barrier" to the goal of completing the task, and this should increase the subject's desire to finish the task. If this hypothesis obtains, then the subjects should recall more unfinished than finished tasks under the conditions of the Zeigarnik experiment.

Zeigarnik assigned to her subjects eighteen to twenty-two simple problems such as completing jigsaw puzzles, working out arithmetic problems, making clay models, and the like. Each subject was allowed to finish half the tasks, the remainder being arbitrarily interrupted by the experimenter and the subject requested to go on to another task. The interrupted tasks were randomly scattered throughout the entire series.

When all tasks had been either completed or experimentally interrupted, Zeigarnik requested the subjects to recall <u>all</u> tasks. Approximately 80 per cent of the subjects recalled more uncompleted tasks than completed tasks. Moreover, tasks in which the subjects were strongly engrossed were more often recalled than those in which the subjects showed only a moderate degree of interest. The experiment demonstrates that tension, aroused by the task, remains undischarged until the task is completed. If uncompleted, the persistent tension is revealed by selective recall.

If it is assumed that the subjects in the present experiment experienced completion or incompletion of the task as a function of the presence or absence and the type of feedback, the so-called "Zeigarnik effect" seems to explain the results with simplicity and parsimony. The subjects in the inconsistent-information groups experienced incompletion of the task because their task-relevant efforts were consistently thwarted by disagreement feedback which may have led them to experience lack of "closure" in their cognitive field. Therefore, after the 10 series of trials they tended to achieve closure by more concerted, albeit verbal, efforts which indicated their "involvement" with the task as shown by the highest total number of relevant statements they produced. The subjects in the control groups seem to have experienced mild feelings of incompletion of the task since they did not receive any feedback confirming or denying the task completion. Hence, they too engaged in verbal behavior whose goal was presumably the achievement of cognitive closure and they produced considerably fewer relevant statements than the subjects in the inconsistent-information groups but significantly more relevant statements than the subjects in the consistent-information groups. The subjects in the consistent-information groups perceived the task as completed, because they were consistently informed by the experimenter that in effect the task they were set out to do <u>was</u> completed. It is, therefore, not surprising that subsequently they made significantly fewer verbal attempts to achieve cognitive closure.

The model based on information theory is another formulation which seems to provide an adequate explanation for the results obtained in the present study. Information has been operationally defined as "that which removes or reduces uncertainty" (Attneave, 1959, p. 1). Information theory then applies essentially to situations involving reduction of uncertainty. The uncertainty of a given question increases with the number of alternative answers that it potentially possesses; hence, uncertainty in any situation is operationally equated with the number of possible alternatives in that situation. In terms of a certainty-uncertainty dimension the consistent-information groups had the least amount of relative uncertainty as a result of the consistent feedback which reduced the intra-trial uncertainty to zero. The control groups had more uncertainty because in the absence of relevant feedback they did not have any external information concerning the possible range of alternatives. The subjects in the inconsistent-information groups, however, had the highest amount of relative uncertainty because the inconsistent feedbacks not only did not reduce their uncertainty; they in fact increased the subjects' uncertainty by re-introducing one more alternative in the situation through declaring the subject's chosen alternative as wrong and thereby adding to the cluster of alternatives in the situation which collectively tended to increase the subject's uncertainty more than in the consistent-information and control groups. If the subject's verbalizations are regarded as attempts at uncertainty-reduction proportional to the amount of uncertainty in their cognitive field, it is no wonder that, next to the inconsistent-information groups, the control groups produced the greatest and the consistent-information groups the smallest number of relevant statements.

Hence in view of theoretical parsimony, precision, and simplicity the model derived from information theory seems to provide the best fit for the data obtained in this study. Moreover, the information model possesses intrinsically more predictive and explanatory potency for a certain range of informational and decisional data (Cf. Feather, 1963; Freedman, 1965; Mills et al., 1959; Rosen, 1961; and Steiner, 1962) inexplicable by the dissonance model which cannot adequately account for the intensity of dissonance reduction tendencies.

Since in terms of the information model the dissonance-reducing tendency can be operationally defined as an inverse function of decision certainty (Cf. Rosen, 1961), it is quite likely that this precise and operational approach to the problems of measurement of dissonance intensity and dissonance reduction is potentially of more heuristic value than Festinger's model, which seems to have been divested of explanatory power by its equivocal and conflictual predictions.

As a final observation, it can be seen that the unwieldy results of the present research have indeed brought into sharper focus the need for further theoretical and methodological refinements of the activation hypothesis and its verification. Perhaps simplistic laboratory experiments are not adequate grounds to test the validity and applicability of the hypothesis. There is a growing body of anecdotal and descriptive data and discursive notions to suggest that "psychic constancy" is not the rule for all people and also, more importantly, that there are individuals who creatively violate the principle of cognitive consistency in the achievement of other, subjectively more meaningful, ends.

Consistent with this view, Barron has argued that there seems to be in creative individuals; ". . . an actual desire to break through the regularities of perception, to shatter what is stable or constant in consciousness, to go beyond the given world to find that something-more or that something-different that intuition says is there" (1963, p. 247). Barron (1963) boldly entertains the idea that ". . . at the very heart of the creative process is this ability to shatter the rule of law and regularity in the mind" (p. 249).

Perhaps one of the adaptive functions of the brain is to "average out" the sensory and perceptual data in many sensory modalities and experiential planes simultaneously (Barron, 1963). Yet, it is also likely that one of the special functions of "consciousness" in man is the ability to alter or slow down this averaging process in the brain in order to realize other potentially indefeasible experiences. The creative urge, the non-logical quests of the mystic, the use of alcohol and the so-called "consciousness-expanding" drugs (Watts, 1962) such as peyote, mescaline, psilocybin, and LSD, some of which have been in wide use in diverse cultures since antiquity, they all seem to be efforts away from cognitive balance and regularity and toward the achievement of initially incongruous, yet subjectively significant, cognitive experiences.

This study attempted to put some of the implications of the activation hypothesis, which sought to formalize the kind of observations that defy the universality of concepts of stability and balance into a theoretical model, to crude test; it did not succeed. This, however, suggests that, in view of the potential theoretical and practical significance of this area of study and its apparent relationship to the class of variables involved in research on creativity, more concerted and refined efforts should be directed toward research in this complex field.

SUMMARY

Several "consistency" constructs have been proposed in social psychology to account for the intensity, direction, and modification of attitudes. Heider's "balance theory," Newcomb's "strain-toward-symmetry" hypothesis, Cartwright and Harary's "structural balance" theory, and Osgood and Tannenbaum's "congruity" hypothesis all assume "consistency" to be a desirable attitudinal state. Festinger's "theory of cognitive dissonance" is logically in line with the preceding conceptions. It postulates that inconsistency between pairs of "cognitive elements" (i. e., attitudes, beliefs, and opinions) results in dissonance which is psychologically noxious. Hence there is a drive toward reduction of dissonance and resumption of consonance.

Recent empirical studies have produced data inconsistent with certain derivations from Festinger's theory. This study was undertaken to test the "activation hypothesis of cognitive behavior" which was proposed to provide an explanatory framework for the various cognitive data which cannot be unequivocally fitted into Festinger's model.

The "activation" hypothesis attempts to apply the concept of "optimal level of activation" to data in the cognitive realm. The fundamental proposition in this formulation is that cognitive activity is seldom in a quiescent state. There is a constant flow of activity in the

individual's cognitive system. When the flow of cognitive activity is experienced as either too congruent (homologous or monotonous) or too incongruent (heterologous or discrepant), the individual will tend to revert from both extreme conditions to an experientially optimal level of activity.

Two predictions were subjected to experimental verification in this study. The prediction derived from the activation hypothesis stated that induced states of cognitive congruence and incongruence will tend to result in comparable amounts of cognitive activity in the individual. The analogous prediction from Festinger's theory stated that induced states of cognitive congruence and incongruence will tend to result in more cognitive activity in the individual in the incongruent (dissonant) than in the congruent condition.

The design of the study involved the feedback of "consistent" and "inconsistent" information (independent variable) to the subjects in these respective groups in order to compare their performance with the subjects in the control groups. The number of relevant (i. e., to the experimental task and situation) statements of the subjects, the total number of words contained in statements produced by the subject, the reaction time of the subject in the experimental task, the total number of word association responses given by the subject, and the deviation of subject's counts from the correct counts of the stimulus patterns presented to him constituted the class of dependent variables which was used to gauge the intensity of subject's cognitive activity.

The following findings were obtained in the study:

1. Both male and female subjects in the inconsistent-information and control groups, respectively, produced significantly more relevant statements than male and female subjects in the consistent-information groups. Also, male subjects as a group produced significantly more relevant statements than female subjects. Within the individual pairs of matched groups only the male subjects in the inconsistent-information group produced substantially more relevant statements than female subjects in the corresponding group. Moreover, it was found that these significant inter-group and inter-sex differences still obtain when the effect of the total number of words on the total number of relevant statements is statistically controlled by covariance adjustments.

2. The group differences with respect to the total number of single words produced by the subjects closely parallel the group differences found for the total number of relevant statements. Again, both male and female subjects in the inconsistent-information and control groups, respectively, produced significantly more words than male and female subjects in the consistent-information groups. Also, male subjects as a group produced significantly more words than female subjects. Within the individual pairs of matched groups only the male subjects in the inconsistent-information group produced significantly more words than female subjects in the corresponding group.

3. Both male and female subjects in the consistent-information groups deviated, in terms of the difference between the number of their counts and the actual correct counts of the stimulus patterns, significantly more than the subjects in the inconsistent-information and control groups. Male and female subjects in the inconsistent-information and control groups did not, however, manifest any substantial group differences.

4. There were no significant differences between any of the treatment groups with respect to the number of word association responses and reaction times of the subjects. It is clear that these two variables were not significantly affected by the independent variable.

The obtained results do not support the prediction derived from the activation hypothesis. This was argued to be attributable to the possibly inadequate experimental design of the study in which the intensity of induced congruence and incongruence was not equivalent. There is, however, partial support for Festinger's prediction if it is assumed that "control" subjects experienced "dissonance" as a consequence of having to make a series of decisions.

Since the obtained evidence supports Festinger's theory only partially and rather equivocally it was suggested that the gestalt notion of "closure" and the principle of "certainty" derived from information theory can explain the results with more precision and economy and less equivocation.

The relation of the "activation hypothesis" to variables involved in the studies of creativity was discussed. It was suggested that because of the potential theoretical and practical utility of the notion of activation it may deserve further elaboration and research.

APPENDIX A

Interview protocols for the male subjects in the consistent-information group. The numbers heading each subject's protocol refer, respectively, to his number and age.

1-18: I'm just wondering what this is all about—that's all. . . . I expected something else—either questions or the experiment to include a test of some sort. My thought was about what it was all about—that's all. I'm quite blank as to what's it all about. . . . That's it.

2-19: Main thing I noticed is. . . after each time I gave you the number you told me it was correct even though I'm not sure if it was right or not. There were different shaped objects—some circles, and triangles. I think that's the only two shapes I remember. Colors were gray background and white forms. That's all I can think of. I'm kind of curious what's it all about?

3-19: While I was watching the slides the only thought that entered my mind was to do. . . look at the slides and count as many figures as I saw which I didn't do in any hurry trying to make sure how many figures there were there. . . . Now, I don't know exactly what this has to do with all the things—as a matter of fact during the experiment I was wondering what it was all about and actually I think that's the only thing that entered my mind was one thought of what it was—how many figures were there,—and to count them as accurately as possible. I think that's about it.

4-18: Well, it was interesting. It's the first time I've done anything in that nature. . . and. . . I guess you could say. . . it was sometimes easier to count the objects going vertically instead of horizontally which I feel most people do—just going horizontally. And. . . other than that I can't say too much more really other than it was interesting.

5-18: Well, in regard to what I just finished doing. . . let's see now—the purpose of it I don't know what it is. . . but I was sure. . . even though I didn't know what it is—that there is one. . . . I was also very surprised that the number that I got was the same as the number I was told was supposed to be there. . . and then about this experiment that your're trying to find out. . . I don't know anything about it. . . I don't know what you're trying to conclude from your experiments. . . I don't have any feelings really about it—I just thought it'd be interesting and so far it has been. . . . That's it.

6-19: First I'd like to ask a question—what is it you're aiming to do with the experiment? Well, as I was looking at the slides I found it easier to count the ones that were in straight lines first—I don't mean in straight lines across—but the ones that seemed to be in a line pattern in the main body either diagonally or some other pattern . . . it was easier to count them that way. I didn't seem to have any difficulty counting them—the size didn't make much difference to me. It didn't bother me having to count rapidly—I didn't doubt my count too much. . . . That's it.

7-19: Well, when I was counting these I felt that we're probably being compared with the rest of the students to see how fast and how accurately we can recognize these figures. We probably had to. . . we're broken down into different groups depending on how many got so many right in a certain amount of time—like if you got them all right every time and then according to your time that took them all right. I guess that's about. . . it. I'd like to know though what the purpose of counting all those was. Was there any specific reason for using that. . . the way they were set up? The way they were set up on the slide or the type of figure? I don't think I've any other questions or comments.

8-19: Well, I guess it's just. . . I don't know. . . I don't know. . . I can't think of anything. . . just counting little shapes. . . I don't know what. . . what you'd be trying to find out. . . . I guess that's it. Just what. . . what you're trying to find out? No other thoughts.

9-18: When I first started out I was counting the objects horizontally but I couldn't keep track of them and I got confused in my counting so I started counting them vertically and then I used rows and I could keep easier track of the numbers. . . . . I guess that's about it.

10-18: What do I think about your experiment? I was just mainly wondering why and what you're trying to do with it. Well, I've never been through anything like this before so it's pretty hard to say. . . you know. . . I don't understand what's going on—some kind of an experiment to test something—I'm not quite sure what. . . . That's about the size of it.

APPENDIX B

Interview protocols for the female subjects in the *consistent-information* group. The numbers heading each subject's protocol refer, respectively, to her number and age.

1-18: First thing I thought was about the figures on the slides that most of them seemed to be in a certain pattern so that you could tell just by counting one slide and then across the other way approximately how many they were—so that's how I did instead of counting them and I wonder why they were different shapes. And some of them seemed to be in straight lines and I wonder if it was me or they were pre-planned. . . . That's about it.

2-19: I think the experiment was interesting. However, I don't understand why you had various shapes placed in different rows of numerical order. Some of them were easily countable, easy to count-the other ones were spaced quite erratically. I've no idea what the purpose of this experiment is—however, I enjoyed participating in it, and I'm interested in knowing about the purpose of this experiment. Other than that I've no questions. I'd like to know my results, though. . . . No other ideas.

<u>3-17</u>: While I was taking the experiment I just wondered what I should be doing—looking for accuracy or speed rather than accuracy alone. But I was very surprised when you said each one I'd done was correct, because I decided to go for speed instead of for accuracy—but I was wondering what I was doing this for, and that's about all actually. I'm very blank because when I count I'm very blank. I was just trying to concentrate on what I was doing. I'm blank again. It had no significance. . . . I didn't know why I was doing it—it was just counting like something you do in grammar school. That's all.

<u>4-18</u>: Well, first I'd say I don't understand the experiment and that because I don't understand it I don't see how it's going to show you anything and. . . well, I don't know what else. . . I really don't know. . . . Well, I've never done anything like this before. . . it's an experience. . . I don't understand it. I'd like to know what the. . . what it means counting those little dots. . . I don't know. Just maybe what this is going to prove to you? Nothing else.

<u>5-20</u>: I don't have. . . much to say about it except that I'd be interested in knowing what it was for which is the reason I'm here. I don't. . . I don't think I was a very good subject for it right now. . . because I've trouble seeing at night. . . sometimes and my eyes were

drawn out of focus in counting which would probably may effect accuracy. . . . That's it.

6-19: I was just interested in taking psychology as an elective and I was interested in it and I signed up for this to see what type of an experiment it would be. . . . I had no idea. . . . A friend of mine once last year was called at random to do it and she really enjoyed it and I thought it would be a fun thing to do and I was interested in people and that type of thing and working with them and, you know, how they react. That's about it I guess. . . . I don't have too many thoughts on the subject—I was just, you know, curious what it involved . . . . Nothing more.

7-18: I don't have any real special thoughts. I don't exactly understand what it was for except maybe logical reasoning which after the first two figures a way how to do it and then all the others were like that. . . . After you get the idea of it. . . I mean you just have to have a system to work at and if you remember your multiplication tables you kind of do it. . . . That's it.

8-18: I don't know why I can't think of anything to say. . . . This really didn't make too much sense to me. I guess I don't know what you're doing. I don't know what to say. . . it's hard to count those-so many little things all going around in circles. I can't think of anything else.

9-18: The speed with which the person counts those things would vary according to learning and not just original abilities, if you know what I mean—wouldn't it? What's the purpose of it? Why do you have all of them in fairly large numbers or about the same number somewhere from 30 to 50? Nothing else.

10-18: Just what I think about perception? Why the experiment? A lot depends on your eye-sight, I think. My eye-sight isn't too good—it was kind of hard. . . but perception is relevant to thinking because if you can't see it you can't understand it and if you couldn't see it you couldn't think correctly about what you see. I'm interested in knowing what you're trying to prove. I'm not the one to talk much—no more comments.

APPENDIX C

Interview protocols for the male subjects in the <u>inconsistent-information</u> group. The numbers heading each subject's protocol refer, respectively, to his number and age.

<u>1-20</u>: Well, I believe the experiment is counting the columns each way and then going over the number that you count —this is the way I get it. Seems as though you had 7 by 4 and then 28 and there's some more that would give you 30 or 32. . . to do it rapidly, I think this is the only way that you could do it —it'd be almost impossible to count it. . . . I think the different arrangement of objects on the screen was something that made it a little difficult because one time you thought you knew and then the next time that you did it the same way it wouldn't come out the same. Also I noticed in one of the slides that it wasn't evenly distributed —on the end it was real heavy and then went sparsely. . . . I think the shapes of the objects were something else that makes it a little difficult —if they were round or square or were anything that has a regular shape it'd make it just a little easier. . . . I had one of the courses here on speed reading and it's something like this. . . that's what it relates to me anyway —it's just your idea of what you see on the screen —they flash a object

or a piece of reading for a 50th of a second at times and there'd be more than just what you think it is — you couldn't naturally tell what it was. . . it was just a glimpse. . . . What the experiment is trying to show? It's about all I've on it I believe.

2-21: First of course I'm curious what the experiment is about. . . . You gave me a task to perform — the way I tried to perform it was to just count figures across the top and take a quick glance and figure it and multiply them together to get the total. I got better — I think I got better — as I looked at the figures because my eyes were accustomed to what shapes and what size they were in even though they were different each time I think. . . . I think also while I was taking this test — because I know it was a test and you're working for some specific reason for this — I was just curious why I'm doing this and what results are going to be compiled from everyone doing this test. My task was to find out a certain number and I naturally thought about the easiest way to count up in the amount of time or as fast as I possibly could and I'd either. . . in my first ones I'd look at the group all at once and then I'd try to count. I think I tried to count by 2's or 3's — 1, 2, 3, 4, — I think I also got. . . I wasn't any closer in the actual counting and I got a little bit better looking at the groups at one time and I started counting 4, 8, 16 up across first and then down and I think after a while I started looking at the groups also. When I first looked at them. . . my first perception of the whole thing—I always looked at the thickness or the length of one side down and the length across because some of these pictures seemed to be to me about the same size and I could almost tell how many were probably in there—just a guess and I think that's the way I originally started out counting—as far as the numbers go. . . . I think that's about it.

3-21: It kind of makes me feel stupid because I didn't get any of them right—I couldn't figure any way to. . . as you counted to eliminate mistakes. . . . I could say that they're all in rows and columns—you could've sort of seen how you were going to count—I got mixed up a couple of times and every time I missed every count—that just makes you feel stupid. That's all I've. . . I don't know what you're trying to pull from it—it's kind of shows you how. . . it shows me that I'm not. . . I can't, you know, remember when I'm trying to count something fast, remember what I've counted and what I haven't counted if they aren't in a geometric pattern, and they're just splashed all over. It may be better if I went slower and tried for accuracy rather than speed. I just like to know what you're trying to figure out—what the purpose of your experiment is. Maybe it is some relationship between speed and accuracy—maybe a slower time would show—I mean up to a certain point—it'd show a higher percentage of accuracy—it'd sort of depend on the individual too —it'd be hard to tell, you have to know something about your individual's intelligence so you could base that on it. . . . It's just that I was mad that I didn't get any of them right. Well. . . that's all.

4-19: Well, the first thing is I couldn't understand why I was missing always two except that one time I got one and on the first one I started counting by regular ones and I should've counted by two—that's why I was so far off. But I didn't think, well, it was interesting, but I was surprised at the mistakes I made. I thought that they'll keep getting a little bit harder. Of course if they were in a straight line it would've been a lot easier and it wasn't what I was expecting, but I don't know, the way the things first went together kind of messed me up on the whole thing, there's no geometric form or anything to them, they're just little bitty spots and it makes it kind of rough. And the dark, I don't like that. At first when I was. . . I guess I wasn't ready for it or something, but I don't know. . . It's I don't know what to say. I'm surprised—I expected something, I always heard it was supposed to be round things and square holes and stuff like that—psychology. I don't know what to say—it's something I didn't expect. I'm surprised, very surprised. I wish I could count better. I'd like to know if I counted. . . if I was slower than most or faster or how much my degree of accuracy was. I hope it's about average. I somethimes wonder about that. But I guess that's about it. Like I said I was surprised, didn't know what. . . what to expect when you turned the chair over here, you know, when I walked in, I didn't know what to look at or what I was supposed to be doing and directions weren't within reason. I don't know, I'm not even thinking right now, that's the problem running through my mind. I've had. . . I used this classroom before for English, two semesters ago, I remember, that's why it was so easy to find. I was afraid that I was going to the wrong Aggie building. My room-mate had been wondering what's going on—he was surprised by the phone call because you sounded so serious he thought I was in trouble or something like that. When I called back, he felt much more at ease then when he found out what happened. I told my folks about it too—I wrote them a letter. He took—my room-mate took psych twice. . . he flunked it the first time but he's going through it all right now. . . he's taking 1b now. . . he's gotten interested in it and the other room-mate is taking it too. I suppose. . . that's it.

5-20: I missed everyone. Do most people miss everyone? I found myself counting, trying to keep straight lines, adding one here subtracting one here and trying to remember where that one was so when I got to it I would not count it over again. And I tried to systematize it in lines or basically just on lines. That's the only impression I had. . . I was kind of disappointed in missing them all; I got to a point where I was angry and I wanted to really work hard at it. And it seemed also that I was almost detecting like a flickering—I wasn't sure if I was concentrating on the blackness or the light spots. It seemed like it was going off and on. . . like neon signs, where they have like a flickering sensation. What are you trying to do actually? I was wondering what most people were doing on the average? If they were counting the exact number or not? I was going all for speed and I thought I was having a fairly good speed but not exactness. Still disappointed. That's it.

6-19: I don't know what it's really for but there must be some trick in that. . . . As you count along the lines aren't even—they'd be jogged up and down and you lose count, and I'm not. . . as I was counting I'd like to have gone back and started over because I wasn't too sure if I caught one in and didn't know which line it was in. . . . I'd like to know what it was because I must've been quite a ways off for each one on count—either missing them or counting too many. . . because you see them all at once and being not in a straight line you sort of lose count of which ones you counted and which ones you didn't. I'm wondering what it really is. . . there was some reason other than what I thought it was. . . because they weren't in an even line. . . . I don't have any other thoughts or questions.

7-18: Well, you threw the images up for me and I counted them as fast as I could which was what I thought you wanted me to do and even though I counted them I wasn't always sure because I rushed for time and I don't really understand why you wanted to. . . I just realize that I came to the experiment trying to help you and I guess I realize now from seeing that maybe when I do count something—when I do see something—maybe I'm not seeing all that I think I see and maybe I could've gone slower but I didn't know what the time element was so I tried to get done as much as I could and as accurately as I could and I would like to know why I was counting them and I don't know. . . that's about it. . . . I really don't know if I said that much to help you. I guess I shouldn't say this but it seems kind of silly to me to be sitting here counting things on the wall but I know it meant something so I think maybe it wasn't as goofy as it seems. I realize it was for some end means which I don't understand but I saw those things in front of me and my first reaction was to count them and get them done as fast as possible. . . . I don't know how accurate I was but it didn't sound I was too accurate. I understand you're doing it for perception but I don't know how my ideas came in contact with the perception. . . or how well I did. I would like to know what you're trying to have me. . . what in me you're getting out of me counting dots on the wall. That's about all I can think of really. . . . It was different and sort of stimulated me to see what's going on around here and I didn't realize psychology did that kind of experiment. I just thought it was so like an eye examination really—you know, they throw pictures in front of you and the thing is to see how many and not how accurate and all this seemed to me how accurately and how well I could see what was before me even though it was distorted. That's the best I can say on it. . . I don't know if it's helping you that much.

8-19: I didn't get any of the slides correct. . . . When I counted I tried to count in groups of 3 or 2 but sometimes I wasn't sure whether I counted the one or not—only once did I stop and then I didn't know where to start again. This is kind of an interesting experiment. The easiest one was the first one because it was in straight lines—I read from left to right then to the next row and then from right to left. . . . It's kind of like looking at an ant-hill hunting beads except that there aren't any. . . . I could've counted them correctly if I had counted them slower, I think—I was trying to give answers as fast as I could. . . . I think it's easier to count the circles instead of the triangles because it seems like the triangles are mixed up more. . . maybe because of the three sides—it's easier to count things in rows other than when they're all mixed up. If they're mixed up you tend to forget which ones you've counted and where to go. . . you can't break them into groups as easily—that's what I tried to do, break them into groups. Only the first one I could've counted how many in one row and then I judged from the other rows how many there were to multiply. The other ones I didn't have time to do because they were too broken up. . . I don't know what would've happened if they were all different colors— if that would've been easier or not. . . . I think that's all. What is this going to show? What did this show? Does it show any. . . what're the applications of this. . . of the findings? What do you hope to find? How did my counts compare or how did my answers compare to the whole group?. . . . That's it.

9-19: Well, I was pretty sure I had them right there several times. After a while I got the impression that the experiment was for. . . you gave a number and they gave you one that wasn't that number to see how fast. . . whether you'd counted them slower or whether you'd get them right or whether you kept on counting fast to get them to do as fast as they could or try to get the accuracy—that's all I thought about it. . . . I just thought I got them right a couple of times—couple of times I knew I had them wrong—but several times I tried diligently to pick out definitely the right number and it seemed I never got it right. . . . On one of those it showed squares and groups of 4 and groups of 5 and a couple of more groups of 4 and another 5—I just went down each group and counted them and put the groups together. . . . I'm sure my arithmetic wasn't that far off because I'm pretty good in math. I thought myself I was right on a couple of them and I can't say which ones right now. When there were a great majority of them I tried to pick them in groups of 2's and 3's and the ones I thought were wrong—some I missed by 4, 3 or 4, I just figured out I missed a group of them and that was all when I was counting in groups I thought I was. . . . I should've done it singly instead of going at it by groups because that way I don't think I'd missed it and I think I should've taken more time because I was trying to do it for speed and accuracy—and at first it seemed simple but when I started missing them it seemed there was something wrong. . . I guess I counted too many or too few it seemed like. Well. . . that's all.

<u>10-21</u>: I was just. . . I didn't quite understand, you know, exactly what you wanted but I knew you wanted me to count the slides and it was kind of hard—I kept wanting to go back and try to get what I missed and I thought maybe there'd be a pattern, you know, at first and I saw they were random I thought maybe you might've them 3 here and 3 there and I thought maybe I could count them faster if I counted, you know, 3, 6, 9, instead of 1, 2, right down the line, and I noticed I wanted to go back quite a bit and try to pick up what I'd missed. . . that's really. . . I don't know what else. . . it was quite a thing. . . I've never done anything like that before. I noticed I wasn't too far off from the correct answers and. . . I kind of guessed at some of them,

you know, after I counted and I thought maybe I missed one or two so I added it. . . . Well, I thought I'd just add two more to be sure. . . . As I was counting I kept thinking that I'd missed one or two or I hadn't counted as I was counting—or I'd counted it once already but I was afraid maybe I'd missed, you know, one here in the corner or something so I thought by maybe adding to that that the answer would be more correct. . . . Were you looking for specific answers? Could you tell me exactly what you wanted to know? And, well, what was the main purpose of the test? What did it have to do with the perception? Was it how fast the mind could count the spots? That's all I can think of.

APPENDIX D

Interview protocols for the female subjects in the *inconsistent-information* group. The numbers heading each subject's protocol refer, respectively, to her number and age.

1-18: Well, the experiment I don't quite understand why it's occurring and what you're trying to do with it. However, counting in some cases was fairly difficult, because you didn't have it in exact lines and in many cases you had odd types of grouping which I assumed that's just to make it more difficult rather than complete counting. I found that when I was doing the counting rather than shifting my eyes that shifted my whole head so it helped me keep count of what I was doing. And in many cases after I finished counting before the number that I had arrived at sort of registered I had to think a minute to remember what I had exactly counted previously. When you were switching the slides I found that for a brief moment I was in the dark. . . that my eyes were not quite accustomed to the slide. For a few seconds it was difficult to start and the slides that had the circles on them seemed easier to count rather than the ones that appeared. . . I don't know what they were, triangles or something. And I felt a little bad when the numbers that I had counted did not agree with the numbers that you had said was correct for that particular slide. In many cases when I was trying to count I sort of didn't stay with one particular pattern going straight across to the end of the slide. . . but I was pretty sure that I hadn't left out many of the objects on the slide. But they apparently were incorrect—these slides, I think probably for me would have been easier to see had. . . the background been white and the other part had been black. Because when I was looking at these slides, especially the ones that didn't have the circles on it in some cases they seemed blurred and I don't know if it was as it was supposed to be or whether it was just my eyes. But I found it in many cases difficult to see exactly what was going on. . . I didn't seem to go too slow. . . that's all.

2-19: I'm not exactly sure what kind of statements you really mean—you mean about those slides? The different shapes of the items made it hard to count—they seemed like they were moving every time a bit—or at least it's the way it appeared to my eyes. . . . Some of them were in patterns but others weren't—some of the same figures were turned the other way around and upside down. . . some figures seemed brighter than others. . . and the circles were easier. . . weren't as sharp on the eyes as the kind of like the diamond shapes—they're easier to skim over. I don't know of anything else to tell you about it. It seemed like I was working against time instead of working accurate

I was just trying to see how fast I can go through. That's all. . . that seems all that I can think of exactly to show you what I think of this experiment.

3-18: I tried to count in rows but sometimes I'd get confused and felt that if I stuck to the rows as much as possible I'd be more accurate than if I tried to do it in groups clumped together. Sometimes though I'd use columns and count them that way and other times when the pattern. . . was sort of confusing I'd try to use either one. And when I first heard that the experiment would be with numbers I was sort of disappointed because I've always disliked numbers and anything to do with math. And I was wondering what this was to prove and what it would show and how other people did on it. . . . I noticed also that I was usually more far off on objects that seemed to have less of a pattern to them. I don't think there was any relationship to the familiarity. . . between the object and how correctly I counted it. Although when I first saw the pennies (she means perhaps the geometric forms!— Experimenter's note). I thought that I might be more accurate because it's more familiar than triangles. . . . I think that's about all at the top of my head—but what is this experiment really about? Are we going to. . . sort of be compared to other students? Why did you use these tiny dots? Why did I have to count them so fast? I mean. . . I think it's more important to be accurate than just fast, isn't it?. . . . That's it.

4-18: Well, it was interesting but, I don't know, seemed like the ones I counted I thought they were right—they weren't—and it was. . . . I didn't see any relation with perception of counting but of course I didn't understand the experiment itself and I don't know—I didn't think it was really that important. . . and. . . I want to know mainly what is the main importance of this test. Why they're being given now—for what use they'll be used for—and the shapes that were used on the pictures if they had anything to do with it and how they were spread out and if there was any true formation to it like 4, 5, 4, 5, something like that? I still can't figure out why I missed so many. I think some of the patterns weren't in straight lines. . . and I had to count fast and I. . . guess I missed them. And the dots were very small and it was a strain on the eyes to sort of follow every one of them. If the shapes were, you know, kind of larger you might be able to count them more accurately and I suppose the background being black and all it was kind of hard to. . . . to keep track of all those little dots. What's the experiment for anyway? That's all, I guess.

5-19: Oh well, I thought it was very good, but I got confused a few times or rather mixed up in a number. That's about all I have to say. Will we find out the results? Will we be told about this? I don't really know what else to say. . . I can't think of anything. . . . I'm wondering though whether I was average or not, but I really can't think of much to say about that part especially when I seemed to miss all of them. I noticed about myself that I was pretty nervous each time and some of these slides were rather tricky—I'd think there was a definite pattern and there would be sometimes one more or less, so I just started counting each individual run. . . . . I'm sorry, but I can't think of anything more to say about it. But what is this experiment all about? I mean. . . what are you trying to get at? Can you tell me though what's this relation between perception and thinking or whatever? Besides, how can counting dots have something to do with perception? I guess that's all the questions I have.

6-19: I was trying to figure what you were figuring out in that test, and I thought it was. . . you know. . . had something to do like with grading system at school. . . . We were told that if a teacher commends a good student then the good student won't do good any more and if the teacher down-grades a bad student, the bad student won't do good any more. So I thought this probably was to see if a subject is repeatedly wrong if he would take more time to be more careful and do better and while I was doing it I thought of counting holes in the ceiling—that's what it reminded me of. You're going to answer my questions after I've spoken my thoughts and tell me what it was for? I'd like to have as many questions as you had up there to count. . . . But, seriously, what was this test about? What about the results? That's it.

7-19: This experiment is. . . I don't know. . . if you can group the figures together in several groups and then count how many rows, you know, and multiply and you get the approximate number of the shapes. Some will not have the same amount in each row, but you can get a pretty, fairly well-estimated judgment of how many there are. And the dots I think are the easiest to count. . . I mean by looking at them, because it's more in. . . they're more symmetrical than the triangles. I think it's an interesting experiment because this way you can judge how fast a person is able to distinguish what he sees and to associate it with what he thinks he sees. I think that's about all. . . . When you see something like this it tends to make you think that there're more than they really are because it's in one group and like I estimated most of the time I was off over how many there were by at least two or three but. . . . I can't understand why I seemed to miss all of them—I know that for one thing some of the patterns were sort of confusing. Also I was trying to count fast and maybe that way I lost some of the dots. Anyway it doesn't feel too good to miss so many—I feel like I can't count very good. . . . But does this experiment really have anything to do with perception? I. . . I can't see what counting dots and geometric shapes has got to do with perception? Are you going to tell us the results? That's it, I guess.

<u>8-18</u>: It is very interesting—I don't exactly know what I'm supposed to be thinking about. I've never been in one of these before. As far as my relationship between perception and thinking—I don't have. . . I'm not highly regarding myself as thinking deep or having very good perception. It's kind of hard to think for yourself since I've never been really expected to and in this case I'm kind of dubious as to what I'm supposed to be asked about or what I'm supposed to answer. . . . I think it's rather interesting and I feel honored that I should be asked to come for an interview such as this and I hope I can be of some help to this department. . . . I think psychology is very interesting and I like to analyze people—I hope I can be of some help. Exactly what is this experiment about or what am I helping to do? Are my answers extremely important in the dot test I just had or was it too wrong that I was off about 3 or 4 dots either way sometimes? Is this very important? What effect would it have? What do you get out of asking all of these? I guess that would be all. I don't know. . . like I said I've never been asked to do anythink like this before and if I can be of help at another time I really would enjoy doing something like this to help out. . . . I wonder

what my position would be as I am stating my thoughts? Will I be rated or figured up as a statistic or. . . . ? That's about it.

9-19: Well, I think it's very interesting. Is there supposed to be some correlation between your counting or something? I think it was hard to count all of them unless you had a method of maybe. . . what I started doing during counting was across and then back and forth like the way you read and then I started counting down, up and down, which seemed to be I came to the correct answer closer that way but. . . I think it's naturally to read across so you count across so you get lost in that little. . . because they aren't straight lines but you used to read them and what I tried to do when I counted was . . . first time I tried counting them separately each one and then I counted by two's and then in groups but I still never got exactly the same number. I can ask you a question, can't I? I just wondered why did you use geometric shapes instead of things—objects—like shoe or something that you can pick out? I think that's about all—I didn't think too much—I concentrated on counting mostly instead of thinking about what I was doing.

10-18: I was just wondering, do many people miss like I seem to have missed each time by two or four? I thought it was interesting—it was different. . . . And I'd like to know, you know, about it. It was easier to count the ones that weren't as mixed up—the triangles were easier to count but circles were more confusing. I guess that's all I can think of. I liked doing it. I like to do it, but I still feel like I'm a failure. I didn't get any of them. I have to say though that I tried to count every single one of them—but sometimes I think I rushed too much so I won't spend too much time on each slide. I guess that's everything. Just like to know, you know, why we did it? Do many people miss, you know, like 2 or 4 or many or am I the only one? . . . That's everything.

APPENDIX E

Interview protocols for the male subjects in the control (no-information) group. The numbers heading each subject's protocol refer, respectively, to his number and age.

1-21: When I first saw that I tried to take a short cut—multiply—top against the sides and then I figured that's not going to work so I started counting and that's why I was so slow on the first one and then I started counting and on the next slides I got confused by the formation of the dots. . . I lost count and maybe on two of them I actually felt weren't accurate and after a while the last two slides there I saw I started seeing formations where the dots were lined up in an order where I could follow easily. . . . I noticed on the last slide. . . some of the slides looked familiar. . . they looked like they had been repeated. . . and that last slide looked like I'd seen it before and I noticed it is easier to just count up and down instead of going across which I'd do in every other one. . . . I think that's it.

2-19: Well, you said I can ask questions? Well, I don't know what to say, to be honest with you! I think it's different—I've never been in an experiment where I. . . counted geometric forms and I was trying to think of what this could pertain to. Only thing I came up with is to study your reaction or something like that to see how fast you can count things and if you're under pressure or something like that if you're still quick and after that if you could possibly do it. Another thing is that most of those things were—geometric figures—were in a pattern in a way and—in other words you could count them rows and columns and you could count rows and columns and multiply them together. And. . . well. . . that's all I can think of about thinking about the experiment. . . . I was just wondering what those figures were supposed to represent? Was it supposed to be something about your reaction? How fast you can. . . how fast your sight is? In other words you see things and you're supposed to react to them? I was wondering if that had anything to do with it?. . . . That's about all I guess.

3-21: I have kind of a fear that I might've flown off. . . thrown the experiment off a little bit because I've a little bit of a headache I'm not over with yet. . . . The triangles in particular were pretty good and I tried to give as an accurate count as I possibly could. This is extemporaneous. I tried to line them up as much as I could by 2's basically—got a little difficult when there were a lot of them and they were close together—out of line in particular. I think that's where the error will come in. One of the thoughts I had during the experiment was that I thought there were 15 slides rather the 10 you said. . . . No more comments, I guess.

4-22: I guess we're not getting observed from what I see here and what I've seen in other experiments. We're going to see how fast I go through these in there and compare it to other people or other students. In doing this. . . I guess you try to figure out the. . . to which I would respond. I was curious though whether or not someone miscounted if this would mislead you in the experiment. Is it also the way they're arranged? Is there a certain way about reading these? I guess it's a kind of comparison between students. It was sort of an interesting experiment. . . . That's all.

5-19: First I was counting the slides and it seemed like there was the same slides over again at the end. The arrangement might've been a little bit different but the numbers seemed to be the same. I was wondering though about my accuracy—I know that I guessed a little bit on some of them trying to go as quickly as I could. . . . I was also wondering about how I would. . . sort of stack up against others. . . if I would come up as well or not. . . . I was hoping I'd do well. . . . I don't know whether this is relevant or not but one of my close friends is going to come at 7 o'clock and I was wondering whether he'd have to do the same thing. . . . I wanted to identify the little triangles with the pledge pin from a fraternity. . . I mean Tri Del sorority or some place like that. . . . No further thoughts.

6-19: What do I feel about this experiment? Well, I've done it before—not this exact experiment but I've looked at groups of objects and tried to. . . see how many there were at the time. My dad used to get me when I was about seven years old and point at the wall and ask how many flowers there were on it, this sort of thing. So I've had a little bit of training in that respect anyway. Actually when you first called me and made so much about being here at the right time, I thought you were going to keep me here out in the hall or something and observe me turn red in the face, but apparently not. There were various shapes of objects—I must've missed on a couple of them because my vision is not as good as it might be. . . I've a very poor vision. Educated guesses mostly, anyway. I came down not knowing what to expect and that's the way it's been. That's all.

7-18: Well, I thought that. . . I had trouble counting across. . . I tried to go across and I tried to take into consideration the ones in the bottom right below and I'd go to the next one and I couldn't remember whether I'd counted them and the circles were the easiest ones for me to count. I'm not. . . I thought little diamond-shaped ones with. . . were the hardest to count accurately. I was trying more for. . . I was trying to get speed and accuracy but I thought I didn't— I was trying to go more for speed I think but still I tried to count as best I could. I just like to know what. . . how. . . or exactly what it's supposed to prove. . . . I guess that's all.

8-24: What I thought when this experiment was going on was the idea stated in instructions was to count as rapidly as possible and I thought first of all the way to do it would be for me to try some sort of grouping arrangement and count by say two's, three's or by four's . . . whichever happened to be suggested by the pattern that was displayed. In some cases it was easier and in some cases it wasn't. I think on most of them I counted basically by groups of three's, except for the last two. The next to the last one I counted singles, and the last one I counted by groups of four. But in all cases it seemed like it was easier to count by grouping rather than to go through counting singly. Particularly in some of the earlier ones where there were a large number it would be easier to get confused, it seemed like. Also the difference in shapes—the triangles were easier to count than the circles were. For some reason as they were scattered across the ones with the circles on it tended to get black. Little bit of confusion in whether you'd been looking at that one before or not. But I thought it was pretty interesting, really. As I said, immediately the first thought I had was the way you would do it fastest would be by grouping but next to the last one I didn't think it was true. . . . That's it.

9-19: Well, I don't know, this experiment was between perception and accuracy or what? The idea of the experiment, was that it? Well, I don't know, it seemed like I couldn't count the figures on the slides trying to go fast and instead of being able to count, say taking groups at a time, I seemed to have to go through and count one at a time. And I don't know how accurate I was on them. I thought the slides that had them lined up sort of in rows were easier to count than the ones that were just scattered over the screen and seemed like the round figures were easier to count than with the little triangles. Seemed that the slides that had fewer figures on them were easier to count because then you didn't get them mixed up with the ones that you'd already counted. Gee, I don't know what else I could say.

10-18: Well, I'm interested in the experiment. It was a little bit confusing, I imagine. What I think it is, it is. . . well, it appeared to me you were to perceive the shapes and not just one at a time but in groups and count them accurately and rapidly and then shift from one size or geometric form to the next without. . . loss of ability or something? I'm not exactly sure what it was but. . . let me think. . . I had a feeling that. . . of. . . while I was doing it that I was making mistakes as I was going but I didn't want to turn around and come back. . . you know. . . I was counting and I left one out but I figured I'll do as accurate as possible. . . . No more comments, I'm afraid.

APPENDIX F

Interview protocols for the female subjects in the <u>control</u> (no-information) group. The numbers heading each subject's protocol refer, respectively, to her number and age.

<u>1-18</u>: First of all I had more trouble with the triangles—there were more of them and they were scattered and the angle. . . they were set at different angles and it's harder to perceive. The circles were quite easy. . . . The first slide, of course, was more or less squares—rows of 8 and 5 and I just counted one way and then down and then multiplied. I think I had less trouble with the squares than anything—the circles tended to make me a little dizzy, I think, and. . . I can't think of anything else except towards the last there. . . it got harder to count. The last. . . I think it was the last slide. . . there were more or less vertical columns and they were easier to count, but towards the last I was getting kind of blurry. Are you doing this for visual perception? Is it common for the triangles to be more confusing? I just wondered how my reactions compared with those of other students who have taken this. . . . That's about all.

2-20: Well. . .some of them were pretty hard to count—they're much easier when they're in a straight line, of course. I think the irregularly-shaped ones were harder to count. . . . And I wondered what you were measuring when you tended to do that. Reminded me a little bit of preparing beans, cleaning beans, preparing them to cook. I don't think I'm very fast at counting. . .I think I'm quite slow, but I got pretty mixed up on the lines that were wiggly. I can't think of anything else about the experiment. . .the relationship to perception.

3-18: It's been an interesting type of an experiment. . .you think you counted them all and you're sure that you haven't, because sometimes you count by 2's and they don't come out even and so you think you missed one or two. It seemed as if some of the lines were purposely made uneven so that you couldn't count by 2's because some of them were even. . . . I think that the size of the object made it easier to count. Of course the number of objects also made it easier to count them. And also the black background made it kind of hard to count—and the objects were too tiny. I wonder of what possible value this could be. . . . I don't think I have very much more to say about it.

4-19: I thought I miscounted many times. Also I thought I saw the same pictures many times. . . . The sizes didn't make too much difference to me. The relationship in the minds and things did. . . I miscounted when I got out of the line. I usually tried to count one, two, four, five, six, seven, nine, ten. . . not pairs or anything like that unless they were in fairly even lines. It. . . I was questioning in my mind how fast I was doing and whether I was compared to other people. I was doing it—whether I was doing it right or wrong. That was mostly all—whether my eyes were switching back and forth. . . see, I was wondering whether I was using one eye or both eyes. That's pretty much the major things. I don't have any questions more or less except why you're doing this.

5-19: Well, the first thing I thought when I saw the slides was the way the experiment was going to work. There were going to be intertwined lines that were geometric figures. When I saw the slides, however, I felt that perhaps the idea behind the experiment was to see if we could visualize groups like you see on a dice, you know, six immediately, because it's three and three. . . but the groups were mixed up and you had to figure out some way of getting groups rather than counting each one individually. In this. . . this is related to the deal we did in the 1a class the other day. I can't think of anything else. . . I tried to think how it's applicable to something—what your deep, hidden secret purpose is, but I can't see it right away. I'm starting the speedy reading course and I could see where visualization of a group could be applicable to a reading course such as that. That's all.

6-19: Well, the reason I volunteered for this experiment is because I thought it'd be interesting to see what kind of experiment they do in psych and psychology is interesting to me anyway. . . . I read several books on it—I haven't read the entire books but I've read several chapters in different books about it and it just interested me so I thought I'd volunteer just for fun to find out what it was like. . . . Oh, I was going to say on that first two slides that you showed me I didn't count—I guessed at them. . . I forgot about counting and I just guessed because I wasn't expecting that kind of a geometric figure. I was expecting something that was to be drawn or something. And another thing. . . . I wasn't expecting the black background—I was expecting black hand-writing on a white background and it kind of shocked me so I guessed. . . . I don't have any more thoughts.

7-19: I thought that the geometric figures. . . the difference in the circles and the squares and the diamonds made it easier to distinguish between the numbers. . . . It was easier to visualize the number and the circles than the diamond shapes that looked like they were more of them. I don't understand the purpose of this experiment. It was just how fast you can recall the numbers? Or you can perceive by just glancing at them? Were you supposed to take your time to count all of them? What's the point of it? No more thoughts.

8-19: One flaw I had was to wonder whether the emphasis would be on speed or accuracy, which I happened to find out before I started. . . . And another, I was interested in the apparent groupings of the figures on the field. In some cases I started reading from left to right and in some from top to bottom or bottom to top as the case might be. And of course, one wonders as one goes along how many they've gotten right. . . . I think that's about it.

9-19: The ones with straight lines were easier to count—the more mixed-up they are the harder they're to keep track of once you've counted. It's a good measure of perception—it seems to me. I can't. . . I missed quite a few. I just. . . I don't think there's much to. . . I don't. . . I don't have any thoughts on it. I don't know. . . I just. . . I don't have that many thoughts on it. It's different—it's a little hard to sit and read on it like some tests you take. . . maybe. . . like in sociology. Can't think of anything else.

10-18: You said speed and accuracy. . . was that the way you were supposed to count the figures on those ten slides? I can see that made counting very difficult. The dots were very confusing—I think I missed quite a few. It seemed that every time you showed a new slide I got more confused in counting them so that by the end I was just rushing through each one without paying too much attention to accuracy but. . . that's why I missed on some, I guess, every time though I tried to give an accurate count. But sometimes I seemed to miss one or two groups. I really don't know what else to say about the experiment except that it's fun. . . I've never done anything like this before. I hope it helps. What particular part of psych are you majoring in? I got other questions but I don't know how relevant they're to the experiment. . . . Why you're conducting this experiment? I think it's very interesting. Well, that's it.

APPENDIX G

The Ratings Made by the Four Judges for Subjects in the Six Treatment Conditions Together with their Respective Means.

	AM					BM					CM				
Ss	1	2	3	4	M	1	2	3	4	M	1	2	3	4	M
1	8	5	5	6	6	18	12	18	19	16.75	19	16	13	17	17.25
2	5	6	6	7	6	25	28	18	24	23.75	22	18	19	20	19.75
3	9	11	8	9	9.25	21	20	18	22	20.25	13	9	10	11	10.75
4	8	8	5	9	7.5	22	23	26	27	24.5	13	9	10	12	11
5	11	12	9	11	10.75	20	19	20	21	20	14	9	13	11	11.75
6	8	13	8	9	9.5	15	17	13	17	15.5	15	17	14	15	15.25
7	15	16	13	14	14.5	49	52	40	44	46.25	15	12	12	13	13
8	4	5	4	4	4.25	40	38	36	35	37.25	21	19	17	20	19.25
9	6	7	6	6	6.25	36	41	39	40	38	16	19	18	19	18
10	8	8	5	7	7	34	37	35	38	36	12	8	8	11	9.75
	AF					**BF**					**CF**				
1	8	6	6	7	6.75	24	25	24	27	25	18	15	15	17	16.25
2	10	9	8	8	8.75	17	14	13	16	15	9	8	8	11	9
3	11	9	8	8	9	16	17	18	18	17.25	13	11	11	14	12.25
4	9	9	7	8	8.25	26	28	28	29	27.75	19	14	14	18	16.25
5	7	7	6	7	6.75	18	17	16	15	16.5	11	10	10	12	10.75
6	4	4	5	5	4.5	13	14	14	15	14	16	14	12	15	14.25
7	7	9	6	8	7.50	28	29	24	26	26.75	11	11	8	12	10.5
8	4	5	6	5	5	27	24	23	24	24.5	9	9	6	8	7.75
9	6	7	4	4	5.25	26	34	27	25	28	12	13	7	12	11
10	9	9	8	7	8.25	21	21	18	20	20	19	15	14	17	16.25

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