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Placebos, instructions, and the development of expectancy.

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PLACEBOS, INSTRUCTIONS, AND THE DEVELOPMENT OF EXPECTANCY

A Dissertation Presented

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PART 1

INTRODUCTION

The placebo response may be generally defined as the appearance of changes in the protocol statements and/or the observable behavior of a person, consequent upon that person's having been subjected to a procedure not considered to be capable in itself of producing that change. Specifically, however, the use of the term has been more or less restricted to situations in which some pharmacologically inert substance, or placebo, is administered to a person who is told of some change in his subjective state and/or in his observable behavior that will therefore follow. Any change that follows is called a placebo response; but the placebo response par excellence is a change consistent with the effects foretold to the person.

There are numerous reports verifying that under the circumstances outlined, a variety of human subjective states and observable behaviors can be modified. Barber (1959), Kurland (1957), and Rosenthal and Frank (1956) summarize studies in which placebos reduced the number of yearly colds, inhibited the cough reflex, reduced complaints of pain, produced nausea, faintness, diarrhea, dermatitis medicamentosa, epigastric pain, urticaria, angioneurotic edema of the lips, decreased subjective reports and objective indices of anxiety, and resulted in actual end-organ changes. Hankoff, Engelhardt, and Freedman (1960) found that, after the administration of a placebo to 103 schizophrenic outpatients, 62 showed notable behavioral change, 42 in a positive direction, and 20 in a negative direction. Gliedman, Nash, Imber, Stone,

and Frank (1958) report on five studies they did in which it was shown that use of a placebo could markedly change somatic and psychic discomfort, as reflected by protocol statements, in psychoneurotic outpatients. Their report includes an interesting account of a woman in whom one aspect of the placebo response was increased sexual satisfaction. Baker and Thorpe (1957) found that a placebo significantly decreased the frequency of daytime urinary incontinence in a group of 18 deteriorated psychotic patients. Brodeur (1965) used placebos with, on the one hand, stimulant, and on the other, tranquilizer, instructions, to produce appropriate changes in the subjectively reported moods, as measured by an adjective checklist, and in objectively measured pulse-rates, of normal subjects. Interestingly enough, these changes were significant for the pulse-rate data, but not for the data from the adjective checklist.

In brief, placebos may be said to influence many kinds of behavior, in somatic as well as in psychological categories.

Inquiries into the mechanism whereby the placebo effect comes about have tended to be theoretical rather than empirical. The placebo response is considered to belong in the class, "effects of expectation." For the word "expectation," one may substitute the words, "conviction of the patient that this or that effect would occur" (Wolf, 1950, p. 106). Aside from looking at the very obvious effect of instructions, no investigations of the placebo effect have addressed themselves directly to the problem of how this conviction, set, expectation, or even, as Frank (1963) terms it, "faith," is aroused in the subject. It has

been taken for granted that the subject enters the placebo situation with certain convictions, sets, expectations, etc., that he has received from the culture at large, such as that medication, be it a pill, injection, ointment, or whatever, produces subjective and objective effects, that people called medical doctors or druggists are sources of medication, and that medical doctors and druggists are trustworthy, which probably means simply that what they say in the treatment situation has a high chance of being true.

Direct investigations of the placebo response reported in the literature may be divided into two types: 1) those designed to show the quality and magnitude of effects attributable to manipulations of set and expectation; and 2) those designed to elucidate factors influencing the quality and magnitude of those effects. Of the former type are those investigations cited above. Such constitute the bulk of the literature. The latter type of investigation has primarily concerned itself with the personality characteristics of people who show placebo responding.

The explicit medium of expectancy arousal in the placebo situation is a verbal instruction: the subject is told that if he takes such and such a medication, he will experience such and such subjective and/or objective effects. It is clear, however, that before such instructions can arouse expectancy, there must already be present in the subject the expectancy that what the presenter of the instructions is saying is true. The source of this primary expectancy, the expectancy that what the presenter of the instructions is saying is

true, remains unclear in any empirical sense, because, as stated above, most studies that have dealt in even a tangential way with this primary expectancy have approached it from the angle of the personality traits of the person receiving the instructions.

The findings from this sort of research have been characterized as being contradictory (Barber, 1959), inconclusive (Rosenthal and Frank, 1956; Guy, 1967), and inconsistent (Gelfand, Ullman, and Krasner, 1963).

Difficulty in evaluating the studies which have sought to demonstrate that placebo reactivity is a function of some personality characteristic of the reactor stems from the fact that, from study to study there is no consistency in the instruments used to measure the personality characteristics believed to be relevant. Thus Sharp (1965) used the General Attitude Variability Inventory to demonstrate low, positive correlations between placebo reactivity and the attributes of anxiety, self-sufficiency, and dominance; but Gelfand, Gelfand, and Rardin (1965) used the Religious Belief-Behavior Scale and the Marlowe-Crowne Social Desirability Scale to show that the characteristics of, respectively, religiosity, and striving toward socially desirable responses, were positively and significantly correlated with placebo responding. On the other hand, Gelfand, Ullmann, and Krasner (1963) used the California Psychological Inventory in evidencing that there was no significant relationship between placebo responding and any of the traits presumed to be tapped by that instrument; and Muller (1961) found no relationship between placebo responding and MMPI measures of

anxiety and depression, or between placebo responding and acquiescence as measured by acceptance or rejection of each of 50 Rorschach card interpretations. Lasagna, Mosteller, Von Felsinger, and Beecher (1954) used the Rorschach, TAT, Wechsler-Bellevue Vocabulary Subtest, and a questionnaire filled out by nurses on the ward, to determine that 11 out of 27 post-operative patients who received pain relief from a placebo differed from the 16 who did not receive such relief in being more anxious, more self-centered and preoccupied with internal bodily processes, more emotionally labile, and more dependent on outside stimulation than on their own mental processes. At the same time, the mental processes of the reactors were said to be less mature than those of the nonreactors, their instinctual needs were said to be greater, and their control over the social expression of those needs was said to be less strongly defined and developed. The lack of extra-test-data behavioral anchors for these personality characteristics, and the questionable state of the projective test arts at this time, tend to push the findings of Lasagna et. al. towards the meaningless end of the meaningful-meaningless continuum.

In fine, one is forced to assert that there is no unequivocal evidence concerning the relationship between personality variables and placebo responding. The situation is analogous to that currently obtaining with respect to the classification of psychotics into the various subtypes of psychosis. Just as the psychotic who shows behaviors said to be diagnostic of simple schizophrenia is likely also to show behaviors said to be diagnostic of paranoid, catatonic, or

hebephrenic schizophrenia, or perhaps of manic-depressive psychosis, so, if one were to take at face value the findings of some of the investigations just cited, one would have to conclude that the placebo responder may be characterized by traits which have also been found to be characteristic of the person who does not respond to placebo. Wolf, Doering, Clark, and Hagans (1957) suggest that, since they found intra-individual variation in placebo responding to be as great as inter-individual variation in that responding, the placebo reactor cannot be predicted from a knowledge of his other characteristics.

The inference may be drawn that there is some other factor at work in producing the placebo response, some factor that tends to override personality attributes of the subject. Learning might be such a factor.

The possibility that learning is such a factor has been put forth by Kurland (1957) and Shapiro (1960). The placebo response is easily conceptualized in learning terms. For example, one might say that every person, in the course of his lifetime, has been exposed to medications and to instructions from some source or another concerning the effects that the medications will cause him to perceive in himself. When the taking of the medication is consistently followed by the effects denoted by the instructions, one might say that the medication is the unconditioned stimulus for the unconditioned response represented by the effects thereof, and that the instructions, which are paired with the administration of the medication, become a conditioned stimulus for those effects, which, when produced by instruc-

tions, are of the nature of a conditioned response. Or, to phrase it more in the language of Tolman (1949, 1959) or of MacCorquodale and Meehl (1953), when the taking of the medication is consistently followed by the effects denoted by the instructions, the person forms the expectancy that if he takes something that falls into the general class of "medications," he will experience the effects denoted by the instructions. In reality, the situation is much more complex, since the source of the instructions may be another human being, so that the perceived truthfulness of other human beings in general, and of the specific other human being who gives the instructions, are involved.

A more formal analysis and description of the expectancy situation will be attempted. To avoid philosophical problems connected with such words as "perception," "cause," "thinking," and so on, we will view the organism as a computer, and will borrow somewhat from the terminology of logic and of general systems analysis.

In the expectancy situation, input into the system consists of some number n of occurrences of a temporal sequence of events. For simplicity's sake, let us say that there are two of these events, A, for antecedent, and C, for consequent. Event A has various attributes that define it as being Event A, and any event X which has the same attributes is an instance of Event A. Events A_2 through A_n are all events sharing with A_1 the attributes that define the class of events such that they are all called Event A. The same situation holds for Event C. C has attributes or properties which, shared by any other event X, place event X in the class of events such that they

are called C. C_1 through C_n form such a class.

Given the above, a simplistic conceptual model of the operations involved in the development of an expectancy would be one which merely computed the ratio of the number of times input C followed input A in time, to the number of times input A occurred. The model will require an entity that counts the number of times A occurs. Let us call this entity the experience counter. Let us call the entity that counts the number of times that C follows A the truth counter. The developing expectancy will be conceived of as having a value between 0.00 and 1.00, which value is stored, in our model, as expectancy. The expectancy formed may be said to have the form of a logical implication, "If A, then C." The value of expectancy after n experiences with A could be loosely said to be the strength of conviction or certainty that the next occurrence of A will be followed by C. Where perfect expectancy is 1.00, our conceptual model of expectancy is represented by the following operations, arranged as a computer program:

1. Set truth counter equal to 0.00.
2. Set experience counter equal to 0.00.
3. Set expectancy equal to 0.00.
4. Input Event A.
5. Increment experience counter by 1.
6. If input of Event C occurs, go to statement #7; if not, go to statement #8.
7. Increment truth counter by 1.
8. Set expectancy equal to value of truth counter divided by value of experience counter.

The resemblance of this schema to probability learning models and the conformity of the results that would be generated by this schema to those generated by the probability matching theorem of learning

theory fame, are clear: if there are 10 occurrences of Event A, and only 5 of them are followed by Event C, the value of expectancy will be .5. This is consistent with the probability matching theorem, where the value of the experience counter equals the number of trials on which it was possible for the subject to predict an event E_1 or E_2 on trial n+1, the value of the experience counter equals the number of times the experimenter reinforced a subject's prediction of event E_1 as being correct, and expectancy equals the number of times the subject predicts event E_1 on trial n+1. Needless to say, our model could be elaborated, but such elaboration would not be to the current purpose. Any expectancy situation can theoretically be broken down into an interlocking combination of programs like the one above presented, each program leading to the development of an expectancy of the form, "If A, then C."

The placebo situation may be analyzed into two sorts of expectancies formed within the placebo reactor:

1. "If person P communicates to me some assertion, that assertion is true."
2. "If I take a chemical agent, then I will experience some effect."

Now, it is clear that the experiences that go into setting these two expectancies may be diverse. With respect to the second expectancy, experiences with all sorts of administered chemical agents will no doubt have played a part, and experiences with the perceived events that followed the administration of these chemical agents. With respect

to the first expectancy, experiences with the class of people in general, with, to be exact, the times assertions made by people in general have been followed by the perception of the truth of those assertions, will have played a part; as well as experiences with the particular class of person involved as a source of instructions in the placebo situation, whether that person be a medical doctor or a psychological experimenter. All these experiences will be part of the past history of the placebo subject. There will also be experiences in the immediate history of the placebo subject, that is, experiences within a particular placebo situation with the one particular individual person who is the source of instructions in that situation.

It should be noted that the second expectancy is contained within the first expectancy, so to speak, for the second expectancy is in fact an assertion, whereas the first is an assertion about a class of assertions. The relationship between these two expectancies is that of object language to metalanguage, respectively, a distinction that will be made again in what follows. Meanwhile, we shall call the first expectancy the "primary expectancy," and the second expectancy the "secondary expectancy."

The acquisition of these primary and secondary expectancies will have conformed to the program developed herein. The secondary expectancy will have been formed by the sequence of the input of various chemical agents, followed by some effect. It is with the primary expectancy, however, that we shall be most intimately concerned, since it can subsume the secondary one.

The primary expectancy may be thought of as being formed by a sequence consisting of the input of an assertion made by person P, followed by the input of events indicating the truth of that assertion. Depending on the number of times Event C (perception of the truth of the assertion) has followed Event A (the making of the assertion by person P), the subject will come to expect that what person P asserts will be followed by some Event C consisting of evidence of the truth of the assertion.

It was the present investigator's hypothesis that if, following a number of presentations of this sequence of Event A followed by Event C, person P makes an assertion of the form of the secondary expectancy, the placebo effect would occur. The investigation to be presented dealt with a test of this hypothesis.

A conceptualization quite similar to the one for expectancy development herein set forth has been proposed by Welch (1957) to explain how the effects of suggestion and hypnosis come about. He conceives of the process as being fundamentally a classical conditioning paradigm, wherein the words of the suggester are associated with some unconditioned stimulus (UCS) and thereby become the conditioned stimulus (CS) for the relevant unconditioned response (UCR). For example, the hypnotist may have his subject gaze at a light, the UCS, the UCR to which is ocular fatigue. At the same time, the hypnotist says to the subject, in essence, "You are experiencing ocular fatigue." The suggester or hypnotist makes more pairings of this sort, that is, pairings of assertions that the subject will experience such and such an effect,

with the UCS's sufficient to produce the effects predicted. Welch writes, "If the subject analyzed himself in some naive fashion, he might say, 'When the hypnotist said I felt A, I felt A; when he said I felt B, I felt B; and now he says I feel X, and I feel X'" (Welch, 1947, p. 361). The difference between the process as conceived by Welch and a classical conditioning process is the role played by language: not only are the CS's themselves units of linguistic communication, the semantics of which are independent of their sequelae, but also the process involves metalanguage, units of linguistic communication the subject of which is other units of linguistic communication. If the former units of linguistic communication, the CS's, are seen to be in object language, then the latter are in metalanguage and are at the next higher level of abstraction. Hence Welch and others (Corn-Becker et al., 1949; Waters and Kodman, 1962) have called this process "abstract conditioning," or "abstracted conditioning."

The model of expectancy formation described in the present paper may be considered a restatement of the abstract conditioning hypothesis, with specific application to the placebo effect.

To the present writer's knowledge, there are only three experiments in the literature that are directly relevant to the abstract conditioning hypothesis. All of the studies used the galvanic skin response (GSR) as the response variable, instead of a more overtly motor behavior.

Corn-Becker, Welch, and Fischelli (1949) presented subjects with pairings of words like "breeze," "red," "green," "music," "flicker,"

"dark," and "nothing," with the referents denoted by these words. For example, the word "breeze" would be flashed on a lantern-slide screen for 14 seconds, and during the last 4 seconds of the 14, the subject would experience a breeze from an electric fan. Both the order of presentation of words and the lengths of the time intervals between presentations were randomized. After 12 or 16 pairings of words with their real-world referents, the words "electric shock" would appear on the lantern-slide screen. This term was never paired with its referent. The GSR to each presented word was measured. Under these experimental circumstances, groups of subjects who underwent sequences of pairings of words with referents exhibited GSR's to the words "electric shock" greater both in amplitude and duration, than those of subjects who had not undergone such sequences of pairings. Abstract conditioning was therefore considered to have occurred.

Waters and Kodman (1962) essentially replicated the Corn-Becker et al. study, adding the variable of suggestibility as defined by performance on the Release Test. They found that their suggestible subjects did not show abstract conditioning, whereas their nonsuggestible subjects did. The reason the suggestible subjects were said to have failed to show abstract conditioning was that their GSR responses to the words "electric shock," which, as in the Corn-Becker et al. study, were never paired with their referent, were as great without exposure to the conditioning procedure as with it. One could speculate, of course, that the so-called suggestible subjects came into the experiment already conditioned to expect that what the experimenter told

them was true, a fact which might have accounted for their response to the Release Test; whereas the nonsuggestible subjects did not have the necessary learning history, when they entered the experiment, to expect that what the experimenter told them was true, and had therefore to form this expectancy during the conditioning procedure itself.

Grings, Carlin, and Appley (1962) conducted a study similar to the above two investigations. One of their major variables was the number of pairings of words with referents, or in other words, a trials variable. The response measure used was, once again, the GSR. They demonstrated abstract conditioning for the subjects who were exposed to only 14 pairings of words with referents, but not for those who were exposed to 49 such pairings. The latter result was presumed to be due to the fact that GSR habituation occurred.

Data directly relevant to the statement that the placebo response is the result of some type of learning come from four animal studies.

Gliedman, Teitelbaum, and Gantt (1956) report an experiment in which a dog was given apomorphine by injection. This operation induced vomiting. Eventually, saline injections produced the same effect, and finally the experimenter's mere entrance into the room produced the same effect.

Herrnstein (1962) found that, after rats had been injected with scopolamine hydrobromide, a drug which suppressed conditioned barpressing for a food reward, injections of saline would produce the same suppression. If the rats had not experienced scopolamine hydrobromide injection, the saline injections did not produce suppression.

Furthermore, there was a hint of a trials effect, in that rats that had experienced more pairings of scopolamine injection and the unconditioned response to that drug of bar-press suppression, showed more bar-press suppression following saline injections that rats that had experienced fewer such pairings.

Balagura and Hoebel (1967) implanted electrodes in the lateral hypothalamus of the rat, in those neural centers thought to be intimately involved in the regulation of feeding behavior. By pressing a bar, the rat could stimulate these centers. Injections of glucagon, a hormone with raises the blood sugar level, were followed by suppression of the bar-pressing rate. After, but not before, the series of glucagon injections, mock injections of normal saline also resulted in suppression of the bar-pressing rate, suggesting, therefore, a learning effect.

Balagura (1968) conducted a study specifically designed to investigate the learning effect obtained by Balagura and Hoebel (1967). The response used was the increase in blood sugar level resulting from glucagon injections. Rats' blood was quantitatively analyzed for sugar content following a series of glucagon injections. The expected blood sugar increase was observed. A blood sugar increase equal in magnitude to that obtained following glucagon injection, was obtained following saline injection in those animals that had first undergone a series of glucagon injections.

We may conclude that placebo or placebo-like responses have been observed in animals, and that when they have been observed, the data

have suggested strongly that the response is the result of learning.

The investigation of placebo responding to be presented herein was intended to show that, in humans, the placebo response might be the result of the form of learning that has been called "abstract conditioning" or "expectancy development."

Two circumstances dictated against a direct investigation of conditioned drug effects in humans. The first circumstance was the administrative impracticality of administering active drugs to student subjects. The second circumstance was the fact that, in humans, it seems that expectancy can smother or even reverse the unconditioned responses to drugs. For example, Lyerly et al. (1964) were able to wipe out the normally energizing effects of amphetamine sulphate and the normally tranquilizing effects of chloral hydrate by giving tranquilizing instructions with the former and energizing instructions with the latter; and Wolf (1950) was able to completely reverse the normally emetic effects of ipecac by means of instructions that the drug would relieve vomiting. Thus the major variable in human placebo responding was suspected by the present investigator to be, not the experience a person has had with actual drugs, though some attempt was made to examine that factor, but the development of what we have called primary expectancy.

Unlike the studies of expectancy development cited above, the present investigation dealt with the body-sway response rather than the GSR, since it seemed intuitively apparent that the sway response had more in common with the sort of effects reported in the placebo litera-

ture, than did the GSR. Besides, the GSR tends to be an erratic measure, to such an extent that J. I. Lacey, delivering a colloquium at the University of Massachusetts in the spring of 1967, characterized all the GSR data he had ever seen as being of questionable worth. A third, but ancillary, reason for choosing a sway measure was that E. Dzendolet of the University of Massachusetts had devised an ingenious piece of equipment, a sway transducer, that would facilitate data collection (Bensel, Dzendolet, & Meiselman, 1968).

The present investigation also incorporated attempts to separate effects of what we have called primary expectancy, the expectancy of the truth of whatever the experimenter said, from the effects of secondary expectancy, the expectancy, presumably developed by past experience with medication administered, that a medication would have some effect.

Finally, the present investigation examined the effect of two different settings, as it were, of expectancy, as that construct was defined in the systems analysis model of expectancy development set forth above. That is, the investigation looked at the effect of different ratios of the number of times Event C, the consequent, followed Event A, the antecedent, to the total number of experiences with Event A.

PART 2

METHOD

Subjects

The subjects were 112 undergraduate female students, with a mean age of 18.04 (S.D. = .71), at the University of Massachusetts, who were required to participate in a psychological experiment as part of their work in an introductory psychology course.

Females were preferred as subjects for this study for two reasons. First, perception of color was involved in the experiment, and whereas color-blindness occurs in about 7% of males, it occurs in less than 1% of females. Second, the principal dependent measure was body-sway, which Bensel, Dzendolet, and Meiselman (1968) have shown to be less pronounced in females than in males; therefore, the use of female subjects was expected to reduce data variability.

Subjects were self-selected. They signed up for the experiment in a folder that was available to them 24 hours a day in a room in a classroom building.

To a certain extent, subjects were also self-screened for freedom from medication for the 24-hour period preceding participation in the experiment. The message on the experimental folder that was intended to produce self-screening is reproduced in Appendix A. When a subject showed up to participate in the experiment, the experimenter asked her

whether she had had any medication of any kind except vitamins in the preceding 24 hours. Any subject admitting that she had was replaced in the design. Furthermore, subjects were replaced who were suffering from physical injury to the legs or feet.

Subjects were randomly assigned to the subgroups of the experimental design. The randomization procedure involved first scrambling the 16 smallest subgroups of the experimental design by assigning each one, as it was originally ordered in the design, a random number from 1 to 16, and then reordering the groups on the basis of the random numbers. The first subject who signed up in the experimental folder was assigned to the experimental subgroup that was first in the random order, the second to the second randomly ordered experimental subgroup, and so on down to the 16th subject, who was assigned to the 16th randomly ordered experimental subgroup. At that point, the process started over again, with the 17th subject who signed up being assigned to the first randomly ordered experimental subgroup, etc.

Subjects were run individually, over a 3-week period from October 23, 1969, to November 18, 1969.

Apparatus

1. A stimulus box, which was a wooden box with a stereopticon-type eyepiece on the front, granting visual access to the inside of the box. A piece of translucent white plastic was mounted inside the box between the eyepiece and 6 10-watt colored bulbs; white, red, green, blue, orange, and yellow. A single-pole single-throw toggle switch with

a spring return was located on the right side of the box and was connected in series with a 6-way switch in the experimenter's room, so that, depending on the setting of the 6-way switch made by the experimenter, a different bulb in the stimulus box was activated when the toggle switch on the side of the box was depressed.

2. Sway transducer, consisting of a square platform of 3/4 inch plywood supported at the center of each side of the platform by the end of each of four short, horizontally positioned steel bars. The ends of the bars extended under the platform and made contact with it by means of machine screws which firmly attached the platform to the bars. The other ends of the bars were rigidly fastened to a steel framework below the platform. Strain gauges were applied to two of the bars. The two gauges on opposing bars were made part of a Wheatstone bridge circuit so that a force applied at any point of the platform, except at the center, along a line joining the opposing bars, would create an imbalance in the bridge circuit. No sensation of movement or rocking of the platform occurred if a subject shifted her weight.

The outputs of the bridge circuit were led into a preamplifier (Grass Instrument, Model 5P1), and displayed on one channel of a polygraph (Grass Instrument, Model 5).

The subject was so positioned on the platform that what was measured was body-sway in the forward-backward dimension.

3. Eli Lilly placebo tablets (#21), consisting of lactose U.S.P.

4. Grape-flavored fruit drink.

5. Tape recorder.

6. Loudspeaker.

7. Spring scale; tape measure, ruler, carpenter's level. These items were used, respectively, for taking subject weights and for taking subject heights. A balance scale with a height rod was not available.

8. Stop-watch.

9. Blindfold consisting of safety goggles stuffed with facial tissue.

Procedure

The experimental operations will be easier to grasp if they are organized into 9 phases.

Except when weight and height were being taken or when a subject had to be positioned on the sway platform or when materials had to be given to a subject, the subject and the experimenter were in separate but adjacent rooms. There was no doorway between the two chambers. The subject's chamber had a one-way mirror through which the subject could be observed without herself observing. To make it more difficult for the subject to see into the experimenter's chamber, even when the lights were on in the experimenter's chamber, the side of the one-way mirror that faced into the experimenter's chamber was backed with a piece of plyboard. A small, hinged door at the center of the bottom edge of the piece of plyboard could be opened to grant a view into the subject's chamber.

All instructions except those involved in taking weight and height and in requesting the completion of questionnaires, were tape-recorded. Any subject receiving any set of instructions was exposed to the exact same tape-recording as all other subjects receiving that set of instructions.

It took approximately one hour to run each subject.

Phase 1. The shoeless weight and height of each subject was taken. Bensel, Dzendolet, and Meiselman (1968) have demonstrated that the product of 55% of a subject's height times the subject's weight is correlated with body-sway; hence, obtaining weight and height measures made it possible to use analysis of covariance, should that have become necessary.

After the weight and height were taken, the sway platform was pointed out to the subject. The experimenter then left the subject's chamber and the following taped instructions, slightly altered from Bensel et al. (1968) were transmitted over the loudspeaker:

> Your task during this part of the experiment is to stand on the platform which I just pointed out to you. I would like you to stand on the platform without moving your feet or legs once their position has been set, without moving your hands or arms -- please clasp your hands and let them hang limply in front of you -- and without moving your head unnecessarily. I do not want you to stand rigidly as if at attention. It is important that you relax. But try to relax without moving your feet and legs, your arms or your head. Are there any questions?

If there were any questions, the instructions were either reread or otherwise made clear.

Then the experimenter re-entered the subject's chamber. The shoeless subject was helped to assume the proper stance in the center of the platform with her heels together and her feet at about 45 degrees to each other. Friction tape guides on the sway platform aided in the uniform placement of subjects thereon. The subject was told to stand so that body weight was evenly distributed on both feet. After the subject was so positioned, she was blindfolded, the experimenter returned to his own chamber, and the subject was given a 5-minute trial on the sway transducer.

At the end of the trial, the experimenter re-entered the subject's room, assisted her off the platform, and removed the blindfold.

Phase 2. Phase 2 began the experimental manipulations proper. The 112 subjects were randomly assigned to the cells of a 3-between, 2-within analysis of variance, 7 subjects to a cell. The between variables were Placebo, Abstract Conditioning, and Instructions. The within variables were Trials and Points of Measurement.

The Placebo variable had two levels, Pill (P), and No Pill (-P).

The Abstract Conditioning variable had four levels: Positive Abstract Conditioning (+AC), Negative Abstract Conditioning (-AC), Sensory Control (SC), and Temporal Control (TC).

The Instructions variable had two levels, Instructions (I), and No Instructions (-I).

The Trials variable had two levels, experimental Phase 1 body-sway measures (Trial 1), and experimental Phase 4 body-sway measures (Trial

2).

The Points of Measurement variable (P) had fifteen levels, corresponding to the 15 20-second intervals in a 5-minute trial on the sway platform.

The design is schematized in Diagram 1.

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Insert Diagram 1 about here
****

Subjects assigned to the Positive Abstract Conditioning cells were exposed to a situation within the confines of which every assertion made by the experimenter concerning a certain type of experience they would have, was true, or within which, to put it another way, the subjects' expectancies were set at 100%. The expectancies of subjects assigned to the Negative Abstract Conditioning cells were set at 25%. What this meant was that the Positive Abstract Conditioning level of expectancy and the Negative Abstract Conditioning level of expectancy represented different ratios times 100 of the number of instances a subject experienced an Event C following an Event A, to the total number of times the subject experienced Event A.

Event A in this case was the experimenter's voice telling the subject that when she looked into the stimulus box and depressed the toggle switch, she would see a certain color of glow in the box. Event C was the occurrence or not of the color predicted, which indicated, respectively, that the experimenter spoke the truth or that he did not.

Exposition follows of the differing Phase 2 experimental treatments for subjects at different levels of the Abstract Conditioning

Diagram 1: Layout of experimental design

				Trial 1	Trial 2
				$P_1 \cdots\cdots P_{15}$	$P_1 \cdots\cdots P_{15}$
P	+AC	I	$S_1 \cdots S_7$		
		-I	$S_1 \cdots S_7$		
	-AC	I	$S_1 \cdots S_7$		
		-I	$S_1 \cdots S_7$		
	SC	I	$S_1 \cdots S_7$		
		-I	$S_1 \cdots S_7$		
	TC	I	$S_1 \cdots S_7$		
		-I	$S_1 \cdots S_7$		
-P	+AC	I	$S_1 \cdots S_7$		
		-I	$S_1 \cdots S_7$		
	-AC	I	$S_1 \cdots S_7$		
		-I	$S_1 \cdots S_7$		
	SC	I	$S_1 \cdots S_7$		
		-I	$S_1 \cdots S_7$		
	TC	I	$S_1 \cdots S_7$		
		-I	$S_1 \cdots S_7$		

variable.

Positive Abstract Conditioning: The subject was seated in front of the stimulus box and shown how to operate the toggle switch. With the experimenter out of the room and the lights out, she was given the following tape-recorded instructions over the loudspeaker:

> This is the experimenter. Depress the toggle switch on the side of the box each time the sound of a buzzer, like this (2" buzzer) comes over the loudspeaker. This is not a speed test, but whenever you hear the sound of a buzzer, you should depress the toggle switch as quickly as is comfortable for you, and you should keep the switch depressed until you hear me say "Stop." You are to depress the switch only when you hear the buzzer. In the event that verbal material precedes the sound of the buzzer, you are to pay attention to the verbal material, but wait for the sound of the buzzer before you depress the toggle switch. Presently I will give you six practice trials. Remember that you do not depress the toggle switch until you hear the sound of the buzzer, and you do not release the toggle switch until you hear me say "Stop." Here are the six practice trials.

Each subject was then given 6 practice trials. Each color of light, in randomized order, was presented once. On 3 of the trials, the sound of the buzzer was preceded by the verbal statement, "If X then Y." A statement with no truth value was chosen so that the number of times subjects were exposed to true statements in the stimulus box situation would not deviate from 100% for the Positive Abstract Conditioning subgroup or from 25% for the Negative Abstract Conditioning subgroup.

The order of practice trials on which this verbal statement preceded the sound of the buzzer was randomized.

Following the practice trials, further instructions were transmitted over the loudspeaker:

> Now the practice trials are over. The next trials will be part of the actual experiment. Continue to look into the box and listen to what comes over the loudspeaker. Remember to depress the toggle switch when you hear the sound of the buzzer, and to release it when you hear me say "Stop."

The subject was then exposed to 20 soundings of the buzzer. Before each sounding, the subject heard the examiner's tape-recorded voice say, over the loudspeaker, "If you depress the toggle switch this next time, you will see a _____ glow in the box." The word in the blank space denoted the color of one of the bulbs in the stimulus box, "white," "red," "green," "blue," "yellow," or "orange." Over the 20 soundings of the buzzer, or trials, the sequence of colors presented was randomized. The experimenter, using the 6-way switch in his chamber, was able to control the truth value of his predictions.

The temporal sequence of events for the practice trials and the actual conditioning trials was (statement)-6"-(2" buzzer)-5"-("Stop."). During the practice trials, if there was to be no verbal statement preceding the sound of the buzzer, the statement was replaced by a 2-second period of silence, which was about as long as it took to say, "If X then Y." During the actual conditioning trials, the statement "If you depress the toggle switch this next time, etc..." filled about 7 seconds.

Subjects in the Positive Abstract Conditioning subgroup found that every time the experimenter told them that if they depressed the toggle switch they would see a certain color of glow in the box, they did in fact see that color of glow in the box when they depressed the toggle switch.

Negative Abstract Conditioning: Subjects in this subgroup were treated like those in the Positive Abstract Conditioning subgroup, except that subjects in this subgroup found that on only 25%, or 5, of the 20 trials, did the experimenter's assertion about the color of glow to be seen when the toggle switch was depressed turn out to be true. The location within the 20-trial sequence of trials on which the experimenter's assertion turned out to be true was randomized.

Sensory Control: Subjects in this subgroup were treated like subjects in the Positive and Negative Abstract Conditioning groups with respect to the stimulus box situation, with the one exception that members of this subgroup were not told what colors of glow would follow their depressing the toggle switch. In other words, subjects in this subgroup received the same stimulus box instructions as subjects in the Positive or Negative Abstract Conditioning subgroups, and were exposed to 20 stimulus box trials after the 6 practice trials, but during the 20 trials, the sounding of the buzzer was never preceded by an assertion about the color of the glow to be seen in the box when the switch was depressed. Such an assertion was replaced by 7 seconds of silence.

This subgroup controlled for the possible effects of visual stimu-

lation alone.

Temporal Control: Subjects in this subgroup were not exposed to the stimulus box at all, but were given the task of circling the N's on a page Xeroxed from the Amherst, Mass. telephone directory, for as long as it would have taken them to go through 20 trials with the stimulus box, about 10 minutes. This subgroup controlled for the effects of time. The instructions given this subgroup follow:

> I have given you a pencil and a copy of a printed page. Your task is to take the pencil and circle as many of the letter N's -- that is N as in Ned -- both capitals and smalls, as you can between now and when I ask you to stop. This is more a test of accuracy than of speed. Work steadily, at a speed that is most comfortable for you. You may begin now.

Phase 3. Phase 3 continued the experimental manipulations. It was relevant for subjects assigned to levels of the Placebo variable, and for subjects assigned to levels of the Instructions variable. That is, it was relevant for subjects assigned to the following subgroups: (Pill).(Instructions), (No Pill).(Instructions), (Pill).(No Instructions), and (No Pill).(No Instructions).

Pill, Instructions: Immediately after the subject's experience with the stimulus box or with the letter circling task, as the case was, the experimenter re-entered the subject's chamber with two 5-ounce paper cups, one filled about $\frac{1}{4}$ with fruit juice, and the other containing the pill. These cups were placed on a table in the subject's chamber, the experimenter left the room, and the following instructions were transmitted over the loudspeaker:

> On the table is a little cup of juice and a small white pill. The pill is a minute dose of a drug called vestibuline. It will take effect in about 10 minutes. It acts upon the mechano-receptors of the inner ear, thereby disrupting the sense of balance. Its only effect will be to make you unsteady on your feet for from 5 to 10 minutes. There are no side effects with this drug. If you take the drug, after about 10 minutes have passed you will find that, when you are standing, you will be unsteady on your feet. I will then ask you to stand once again on the platform upon which you stood at the beginning of the experiment. You will be blindfolded and shoeless, as before. When you step on the platform and are in position, the disturbance you will feel in your sense of balance, the unsteadiness you will feel in your posture, will make it impossible for you to keep from wobbling or swaying for as long as you are standing on the platform. Please take the pill now.

The subject was observed through the one-way mirror to make sure the pill was taken. In addition, after the subject had been observed to take the pill, the experimenter stuck his head in through the doorway of the subject's room and asked, "Did you take it?"

No Pill, Instructions: The subject in this subgroup was not given a pill. Instead, after exposure to the stimulus box or to the letter circling task, as the case was, she received the following instructions over the loudspeaker:

> In about 10 minutes I will ask you to stand once again on the platform upon which you stood at the beginning of the experiment. You will be blindfolded and shoeless, as before. This time, however, you will find that when you step on the platform and are in position, you will

> feel somewhat unsteady on your feet. This will be nothing to be alarmed at. We have simply found it to be a fact that the second time people stand on this platform, they are unsteady on their feet. Why this should be is not clear, but people standing on the platform a second time report that they feel as if their inner ear has been affected, thereby disrupting their sense of balance. Thus, although you will be trying to stand on the platform in as steady a manner as possible, you will be unsteady on your feet while you are standing on the platform. When you step on the platform, as I will ask you to do in about 10 minutes, and you are in position, the disturbance you will feel in your sense of balance, the unsteadiness you will feel in your posture, will make it impossible for you to keep from wobbling or swaying for as long as you are standing on the platform.

Pill, No Instructions: Subjects in this subgroup received the placebo, but were not given instructions concerning what the effects of the placebo would be. Following their performance of whatever Phase 2 tasks had been their lot, the experimenter re-entered the subject chamber with the two 5-ounce paper cups, prepared as set forth in the description of the Pill, Instructions subgroup procedure, and set the cups on a table. Then the experimenter left the subject chamber, and the following instructions were transmitted over the loudspeaker:

> On the table is a little cup of juice and a small white pill. I can tell you nothing about the pill at this time, except that it is perfectly harmless. There is nothing in the pill that will make you sick or ill in any way. About 10 minutes after you take the pill, I will ask you once again to stand on the platform that you stood on at the beginning of the ex-

periment. Please take the pill now.

If there were any questions about the effects of the pill, they were put off with reassurances that the pill was perfectly harmless, and the assertion that to reveal the specific effects of the pill at that time would spoil the study.

In fact, out of 56 subjects who were given the pill, only one had any questions about it, and initially refused to take it. That one refusal, produced by this subgroup, was eventually overcome by the experimenter's persuasion, but the data of the subject who initially refused were replaced in the design, since it was felt that she had been exposed to a different procedure from any of the others.

No Pill, No Instructions: Subjects in this subgroup received neither placebo nor instructions for increased body-sway. After performance of whatever Phase 2 tasks had been their lot, they were given the following message over the loudspeaker:

> In about 10 minutes, I shall ask you once again to stand on the platform that you stood on at the beginning of the experiment. Between now and then, please sit quietly. Please do not smoke.

All subjects were given the above message at the end of the Phase 3 manipulations.

Furthermore, no smoking was allowed at any time during Phases 1 through 6 of the experiment.

Phase 4. All members of all subgroups were given a second 5-minute trial on the sway transducer 10 minutes after the completion of the Phase 2 tasks. This second sway transducer trial was preceded by the

same instructions, modified from Bonsel et al. (1968), as the first, and was in all other particulars conducted like the first.

Phase 5. Each subject received a final, single trial on the stimulus box. The subject was shown how to use a stopwatch and, if she had not used the stimulus box before, how to look into the stimulus box and how to depress the toggle switch. Then the experimenter turned out the lights in the subject's chamber and left the room. The following instructions were transmitted over the loudspeaker:

> Please look into the stimulus box. Do not depress the toggle switch till you hear the buzzer. If you depress the toggle switch when you hear the buzzer, a white glow will appear in the box, and within a few seconds the glow will begin to pulse or flicker faintly. You have a stopwatch to hold in your left hand. When you depress the toggle switch on the side of the box with your right hand, also push the button on the stopwatch to start the watch. As soon as you observe the faint pulsing or flickering of the white light in the box, push the button on the stopwatch again, to stop the watch.

Since the glow that appeared in the stimulus box when the toggle switch on the side of the box was depressed in actuality did not flicker, Phase 5 of the experiment was intended to answer the question of whether or not subjects who found that everything the experimenter said in the experimental situation up to that point was true, from assertions about what would be seen in the stimulus box during Phase 2 down to those about what would be experienced on the sway transducer during Phase 4, would show a tendency to hallucinate, that is, to see a flicker where there was none, in contradistinction to subjects who found that

not everything the experimenter said in the experimental situation up to that point was true.

If the subject did not indicate within a minute's time that she saw the flicker, the trial was terminated and the subject was said to have failed to hallucinate.

Phase 6. In order to provide a rough assessment of a subject's pre-experimental experience with medication, a questionnaire designed to tap that experience was administered. This questionnaire, the Drug Effects Questionnaire (DEQ) and its scoring key may be seen in in Appendix B.

Phase 7. Phase 7 consisted of the administration, to all members of all subgroups, of a 5-point rating scale, the Subjective Effects Questionnaire (SEQ), intended to assess the subjective effects of the Phase 3 manipulations. This rating scale may be seen in Appendix C.

Phase 8. To allow the possibility of inferences concerning the effects of experimental manipulations on subject attitudes towards the experimenter, and the effect of subject attitudes on subject behavior in the experiment, each subject completed a Semantic Differential rating (Osgood et. al., 1957) on 8 concepts: "Doctor," "Father," "Room," "Experimenter in this Experiment," "Druggist," "Kitten," "Car," and "Pill." The bipolar scales used are in Appendix D.

Phase 9. The purpose of Phase 9 was to assess the extent to which any subject was aware of the purposes of the experiment. The instrument designed to do this, the Awareness Questionnaire, may be seen in Appendix E.

Data analysis. The basic data treatment for all dependent measures was analysis of variance conforming to the 3-between, 2-within experimental design explicated earlier, where the between variables were Placebo (F), Abstract Conditioning (A), and Instructions (G). The within variables were Trials (T), and Points of Measurement (P). Only the analyses of variance of body-sway data, however, made use of the full design. The dependent measures from the Hallucination Test (experimental Phase 5), the Drug Effects Questionnaire (experimental Phase 6), the Subjective Effects Questionnaire (experimental Phase 7), the Semantic Differential ratings (experimental Phase 8), and the Awareness Questionnaire (experimental Phase 9), made use of only the between-subjects portion of the design, except that the analyses of variance of the Semantic Differential ratings added the within-subjects variables of Semantic Differential Factors (C) and Scales Nested within Semantic Differential Factors (D(C)).

The analyses of variance might have proceeded under either of two assumptions, either that the Instructions variable was nested within the Placebo variable, or that the Instructions variable was not so nested. Which assumption was to be followed would depend on whether one focused on the word-for-word content of the Instructions for increased body-sway for members of the Pill subgroup as contrasted with those for members of the No Pill subgroup, or whether one focused on the meaning of the Instructions for increased body-sway for members of the Pill subgroup as compared with those for members of the No Pill subgroup. In the former case, Instructions are different from one level of the

Placebo variable to the other, and must therefore be said to be nested within the Placebo variable (Design 1). In the latter case, Instructions are essentially the same from one level of the Placebo variable to the other, and must therefore be said to cross with the Placebo variable (Design 2).

Since the experimental focus was on the meaning of the Instructions rather than on the exact words used, Design 2 was the design of choice. Furthermore, Design 2 was the more complete design, allowing as it did the investigation of a possible interaction between Placebo and Instructions.

All the analyses of variance were performed through use of one of the previously prepared Biomedical statistical computer programs, in this case BMD08V, Sept. 1, 1965 version, prepared by the Health Sciences Computing Facility of UCLA. The computer used was a CDC 3600, housed at the Research Computing Center of the University of Massachusetts.

Where appropriate, further qualitative analyses elucidated the contributions to results of the 4 levels of the Abstract Conditioning variable and of the interactions of those levels with the levels of the Placebo and Instructions variables. Tests for relevant simple effects were also carried out.

Relevant subsidiary analyses, not necessarily analyses of variance, were conducted to seek relationships between sway platform performance under various experimental conditions, and data from experimental Phases 5, 6, 7, 8, and 9.

Predictions

The principal prediction to be made about the way the present investigation would turn out if the model of expectancy development elucidated above was correct, was:

1. There will be a significant Abstract Conditioning X Instructions (AG) interaction. Specifically, the effects of Instructions will vary as a function of the level of Abstract Conditioning, so that, where A_1 designates Positive Abstract Conditioning, A_2 Negative Abstract Conditioning, A_3 Sensory Control, and A_4 Temporal Control; and where G_1 designates Instructions for increased body-sway and G_2 No Instructions for increased body-sway, the following relationships will hold for mean body-sway increases produced by the various subgroups: $A_1G_1 > A_3G_1 = A_4G_1 > A_1G_2 = A_2G_2 = A_3G_2 = A_4G_2 > A_2G_1$. In other words, the Abstract Conditioning procedure will make a difference only when there are some instructions for increased body-sway given to the subject. When such instructions are given, the highest mean increase from pre- to post-test body-sway measures will be shown by subjects exposed to the Positive Abstract Conditioning procedure (A_1), and the lowest by subjects exposed to the Negative Abstract Conditioning procedure (A_2). Those subjects who receive instructions for increased body-sway and who were previously exposed to either of the Control procedures, are expected to produce mean pre-test post-test sway measure increases falling between those of the A_1 and A_2 Instruction subgroups.

Secondary predictions are:

2. Instructions alone will be a potent variable -- subjects who

receive instructions for increased body-sway will show a greater mean body-sway increase from pre- to post-trial than will subjects receiving no instructions for increased body-sway.

3. In Phase 5 of the experiment (Hallucination Test), subjects will show an increased tendency to hallucinate, as a function of increasing levels of percentages of true experimenter assertions. This prediction says that a) subjects who have been exposed to the 100% truth condition (Positive Abstract Conditioning) during experimental Phase 2 will be more likely to hallucinate than those exposed to the 25% truth condition (Negative Abstract Conditioning); and b) the subjects in whom the tendency to hallucinate will be most pronounced will be those who have not only found that the experimenter's assertions were true 100% of the time during experimental Phase 2, but who found also that during experimental Phase 4 they were unsteady on their feet just as the experimenter had told them they would be.

4. Subjects in the Pill subgroup will show a greater mean difference between their pre- and post-trial body-sway measures than will subjects in the No Pill subgroup. This outcome will be conceivable as arising from the expectancy brought into the experimental situation by the subjects, that pills are potent.

PART 3

RESULTS

Analysis of body-sway data

Scoring of body-sway data. The original measure of body-sway was the difference in millimeters between maximum and minimum polygraph pen deflections within each of 15 20-second intervals in each of 2 5-minute trials on the sway platform.

Body-sway data was scored with the "Oscar K" (Benson-Lehner Corp.), a procedure which had the effect of multiplying a vertical millimeter on the polygraph records by a factor of 4.36 (S.E. = .0067). Analyses of body-sway data dealt directly with these Oscar K units, and the values presented for body-sway are in Oscar K units unless otherwise indicated.

Descriptive statistics. Since pre-test body-sway was measured before subjects were exposed to any of the experimental manipulations, the pre-test sway for the entire group of 112 subjects may be considered to be representative of this dependent measure in a general population. Figure 1 is a histogrammatic depiction of the frequency

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Insert Figure 1 about here
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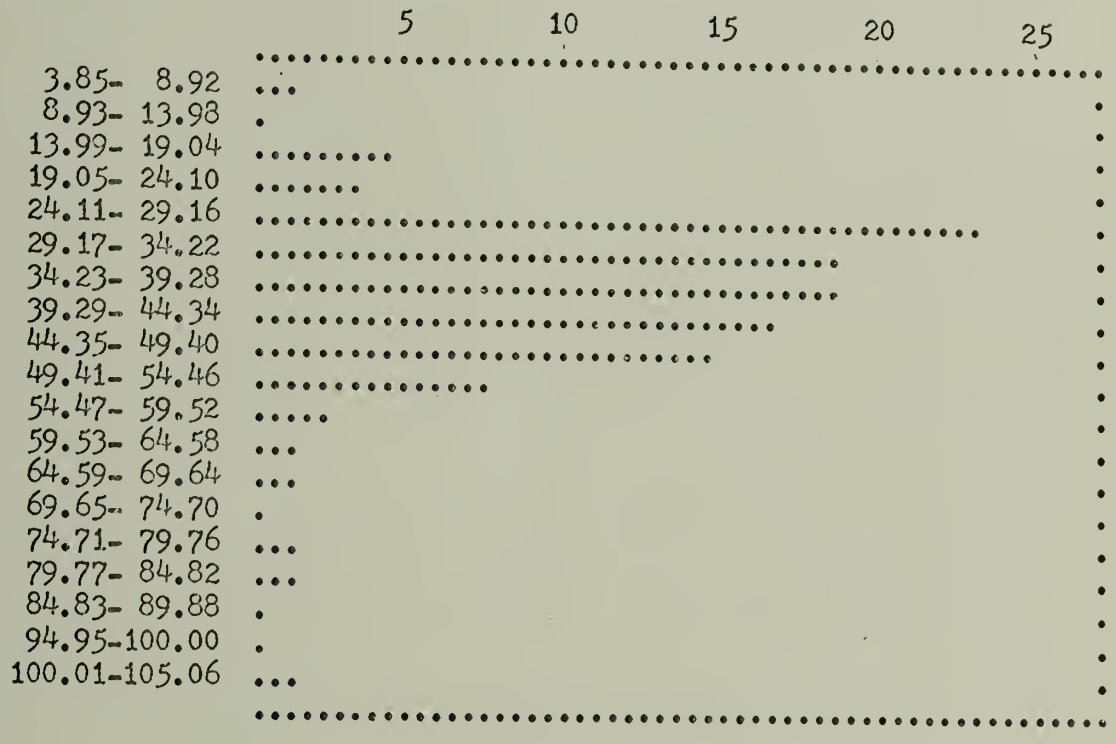
distribution for this data.

It can be seen that the distribution was skewed in the direction of the larger sways. The pre-test mean was 38.01 Oscar K units (8.72 mm.), the S.D. 14.05 Oscar K units (3.22 mm.). Although visual in-

Figure 1: Distribution of pre-experimental body-sway



Horizontal Dimension -- Frequency

Vertical Dimension -- Body-sway in Oscar K units

spection of the distribution suggests a general normality, an 8-binned chi-square goodness of fit test indicated that the distribution did in fact depart significantly from normality.

Cochran's test for heterogeneity of variance, applied to the variances of the 16 smallest subgroups of the experimental design, necessitated rejection of the hypothesis that the assumption of homogeneity of variance had been met.

However, in view of the robustness of the analysis of variance (Myers, 1966; Pp. 61-63), these departures from the assumptions of normality and homogeneity were not deemed to prohibit the use of the analysis of variance as a tool of statistical inference.

Tests of random assignment of subjects to experimental subgroups. To verify that random assignment of subjects to experimental subgroups had in fact been achieved, an analysis of variance was performed on the pre-trial body-sway data, utilizing the design described in the Method section, without the Trial 2 data.

The Instructions main effect ($F(1,96) = 2.91$; p between .05 and .10), the Placebo X Abstract Conditioning interaction effect ($F(3,96) = 2.49$; p between .05 and .10), and the Abstract Conditioning X Instructions interaction effect ($F(3,96) = 2.24$; p between .05 and .10) approached statistical significance. The means for the Instructions main effect and for the Placebo X Abstract Conditioning and Abstract Conditioning X Instructions interactions are shown in Table 1. Since

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Insert Table 1 about here
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Table 1: Means relevant to significant sources of variance in the analysis of pre-experimental body-sway

1a.

Instructions	No Instructions
35.87 (8.23 mm.)	40.15 (9.21 mm.)

1b.

	+AC	-AC	SC	TC
Pill	39.14 (8.98 mm.)	35.44 (8.13 mm.)	48.01 (11.01 mm.)	35.78 (8.21 mm.)
No Pill	40.62 (9.32 mm.)	39.02 (8.95 mm.)	33.86 (&.77 mm.)	32.20 (7.39 mm.)

1c.

	Instructions	No Instructions
+AC	37.15 (8.52 mm.)	42.60 (9.77 mm.)
-AC	30.07 (6.90 mm.)	44.39 (10.18 mm.)
SC	42.24 (9.69 mm.)	39.62 (9.09 mm.)
TC	34.01 (7.80 mm.)	33.97 (7.79 mm.)

the F-ratios for these sources of variance are so large that the probability of their occurring by chance lies between .05 and .10, it seems that the procedure for randomly assigning subjects to experimental subgroups approached failure -- there are pre-trial body-sway differences, nearing statistical significance, between various of the experimental subgroups.

To clarify the source of the near failure of the randomization procedure, four analyses were undertaken.

First, Pearson product-moment correlations were calculated between the pre-trial body-sways and a measure with which body-sway has been found to covary (Bensel et al., 1968). For each subject, this covariate was 55% of the subject's height multiplied times her weight.

Second, an analysis of variance was applied to a matrix consisting of covariate scores for each subject.

Third, analyses of variance were applied to matrices consisting of the components of the covariate, that is, to subject heights and to subject weights.

Fourth, for those experimental subgroups implicated in the nearly significant Placebo X Abstract Conditioning and Abstract Conditioning X Instructions interactions obtained with the analysis of variance on pre-trial body-sway, Spearman rank-difference correlation coefficients were calculated for the rankings of the relevant subgroup means for body-sway, vs. the rankings of the relevant subgroup means for the covariate.

The product-moment correlation of pre-trial body-sway with the

covariate was .745, significant beyond the .01 level.

Analysis of variance of the covariate matrix showed a main effect for Instructions significant between the .05 and .10 levels ($F(1,96) = 2.83$), and an Abstract Conditioning X Instructions interaction effect significant between the .05 and .01 levels ($F(3,96) = 3.07$). The means for the Instructions vs. the No Instructions subgroup fell in the same order as the means for those subgroups when pre-trial body-sway was the measure. The Placebo X Abstract Conditioning interaction did not approach significance.

Analysis of variance of subject heights indicated that the Abstract Conditioning X Instructions interaction was significant between the .10 and .05 levels ($F(3,96) = 2.32$). Although there was no Instructions main effect, the mean of the subgroup that received instructions for increased body-sway and the subgroup that did not receive such instructions fell in the same order as the means for those subgroups when pre-trial body-sway was the measure -- the No Instructions subgroup averaged slightly taller (64.68 inches) than the Instructions subgroup (64.48 inches). The Placebo X Abstract Conditioning interaction did not approach significance.

Analysis of variance of subject weights revealed an Instructions main effect significant between the .10 and .05 levels ($F(1,96) = 3.13$), and an Abstract Conditioning X Instructions interaction significant beyond the .05 level ($F(3,96) = 2.81$). Once again, the means on weight of the Instructions (129.91 lbs.) and No Instructions (136.77 lbs.) subgroups fell in the same order as the means for those subgroups when

pre-trial body-sway was the measure. The Placebo X Abstract Conditioning interaction did not approach significance.

Finally, the Spearman rank-difference correlation coefficients between mean pre-trial body-sway and mean covariate of the subgroups relevant to the Placebo X Abstract Conditioning interaction and to the Abstract Conditioning X Instructions interaction were found to be .91 (p less than .01) and .75 (p less than .02), respectively.

It is therefore reasonable to conclude that, despite use of a random assignment technique, heavier, taller girls and lighter, shorter girls were systematically assigned to such of the experimental subgroups as to produce the near significant differences between the mean pre-trial body-sway of various of the subgroups, and as to result in a strong tendency for the analysis of variance of pre-trial body-sway to produce significant F-ratios for the Instructions, Placebo X Abstract Conditioning, and Abstract Conditioning X Instructions sources of variance.

Inferential statistics. An analysis of variance was performed on the body-sway data, according to the design set forth in the Method section.

Two main effects, that for Trials and that for Points of Measurement, were statistically significant, the former beyond the .01 level ($F(1,96) = 8.40$), and the latter beyond the .001 level ($F(3,96) = 2.52$).

Two other sources of variance approached statistical significance, the source of variance for the Instructions main effect ($F(1,96) = 2.53$; p between .20 and .10), and the source of variance for the Ab-

stract Conditioning X Instructions X Trials interaction effect ($F(3, 96) = 2.52$; p between .20 and .10).

No other sources of variance even approached statistical significance. Indeed, there were several F-ratios that were significantly insignificant, which is to say that when the reciprocals of those ratios were looked up on the same degrees of freedom as the appropriate F's, the reciprocals were statistically significant.

Such significantly insignificant F-ratios were produced by the Instructions X Trials interaction ($1/F(1,96) = 8.35$; p less than .005), the Placebo X Points of Measurement interaction ($1/F(14,1344) = 3.87$; p less than .001), the Placebo X Instructions X Trials interaction ($1/F(1,96) = 83333.33$; p less than .0001), the Instructions X Trials X Points of Measurement interaction ($1/F(14,1344) = 1.87$; p less than .05), and the Placebo X Abstract Conditioning X Instructions X Trials interaction ($1/F(3,96) = 3.04$; p less than .05).

For ease in conceptualization of the effects, such as they were, of the between-subjects variables of Placebo, Abstract Conditioning, and Instructions, on body-sway change from pre-trial to post-trial, the within-subjects variables of Trials and Points of Measurement were eliminated from the design by subtracting mean post-trial body-sway from mean pre-trial body-sway and performing an analysis of variance on the resulting difference scores according to the between-subjects portion of the analysis proposed in the Method section. With respect to the between-subject variables and their interactions, the results of an analysis of such difference scores were of course identical to

those obtained in the originally proposed analysis, and in addition there was a considerable gain in simplicity, since changes from pre-trial to post-trial that were represented in the originally proposed analysis by the interaction of either individual between-subjects variables or of the various interactions of between-subjects variables, with the Trials variable, were represented in the new analysis by between-subjects main effects and interactions.

The Abstract Conditioning X Instructions interaction effect (formerly Abstract Conditioning X Instructions X Trials), which approached statistical significance, is graphically depicted in Figure 2. The means relevant to this interaction effect are presented in Table 2.

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Insert Figure 2 and Table 2 about here
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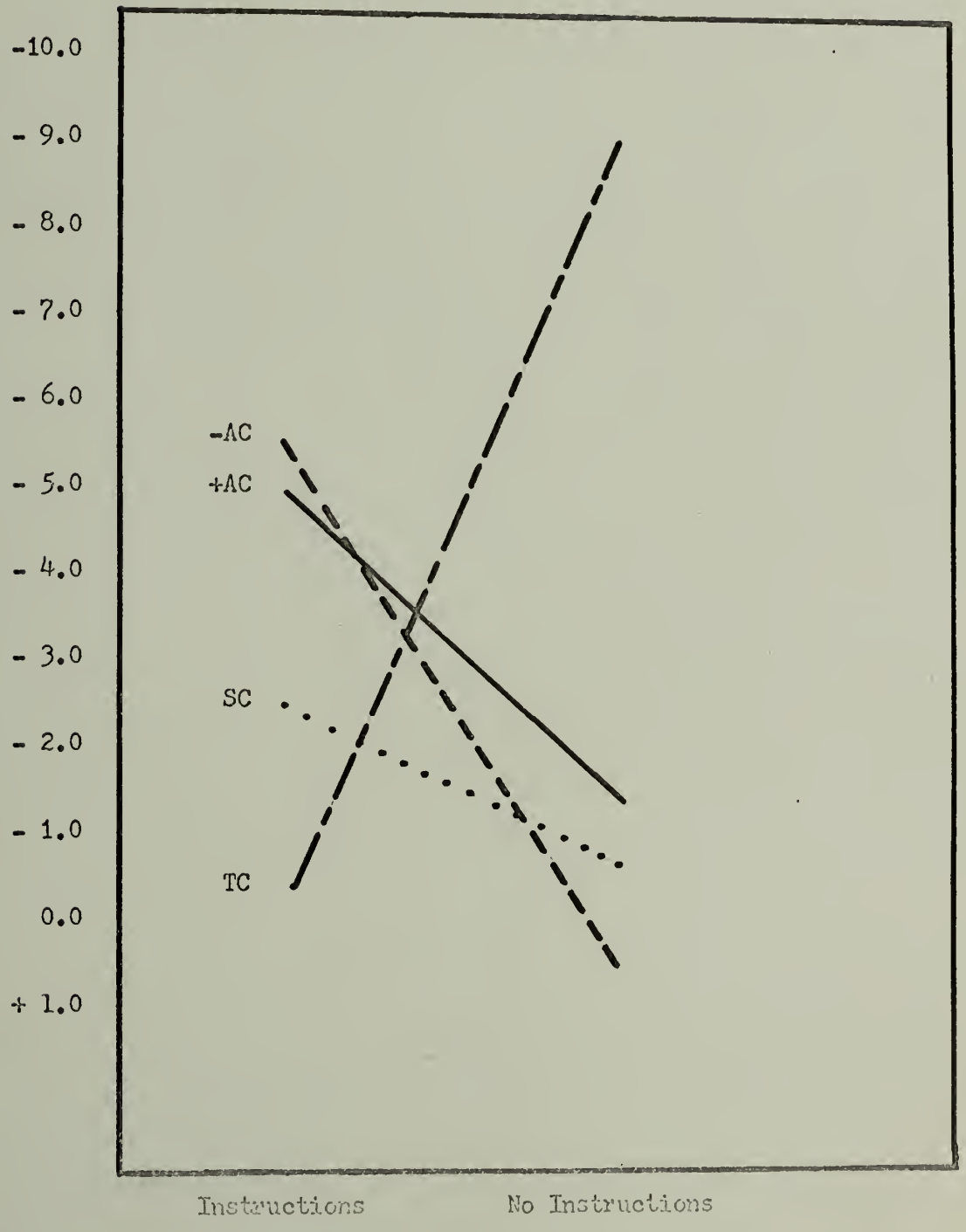
Figure 2 shows clearly that subjects who received instructions for increased body-sway from pre-trial to post-trial and who were exposed to the Positive Abstract Conditioning procedure (A_1), the Negative Abstract Conditioning procedure (A_2), or the Sensory Control procedure (A_3), increased their body-sway more from pre-trial to post-trial than did subjects exposed to those three Abstract Conditioning subgroups who did not receive instructions for increased body-sway from pre-trial to post-trial. The case was quite the reverse for subjects exposed to the Temporal Control procedure (A_4) -- these subjects increased their body-sway more from pre-trial to post-trial when they did not receive instructions for increased body-sway than when they did.

Table 2: Means relevant to the Abstract Conditioning X Instructions interaction in the analysis of body-sway difference scores

	Instructions	No Instructions
+AC	-4.90 (-1.12 mm.)	-1.38 (-0.32 mm.)
-AC	-5.50 (-1.26 mm.)	0.51 (0.12 mm.)
SC	-2.45 (-0.56 mm.)	-0.51 (-0.12 mm.)
TC	-0.36 (-0.08 mm.)	-9.01 (-2.07 mm.)

Figure 2: Graphic representation of the Abstract Conditioning X Instructions interaction in the analysis of body-sway difference scores



Another way of putting this result is to say that subjects who received instructions for increased body-sway increased their body-sway more from pre-trial to post-trial than those who did not receive such instructions, for every Abstract Conditioning subgroup except the Temporal Control subgroup. Testing for the change in the Instructions effect as a function of the $(A_1 + A_2 + A_3)$ vs. A_4 contrast, resulted in an F of 9.87, on 1 and 24 d.f., significant beyond the .005 level. The effect of Instructions vs. No Instructions on subjects in the Temporal Control subgroup was significantly different from the effect of Instructions vs. No Instructions on the other three Abstract Conditioning subgroups.

The outcome of the foregoing contrast analysis prompted analysis of the contrast simple effects, that is, $(A_1 + A_2 + A_3)$ vs. A_4, at the Instructions level and at the No Instructions level. The contrast approached statistical significance at the Instructions level ($F(1,12) = 3.12$; p between .20 and .10), but not at the No Instructions level ($F(1,12) = 1.62$).

Inspection of Figure 2 suggested an analysis of the $(A_1 + A_2)$ vs. $(A_3 + A_4)$ contrast at the Instructions level. The resulting F-value of 1.13, on 1 and 12 d.f., was clearly nonsignificant.

Considered all together, these analyses of body-sway data showed that, overall, subjects swayed more during the second trial (40.96, or 9.39 mm.) than they did during the first trial (38.01, or 8.72 mm.), that summing across trials there were significant differences between body-sway measures taken at some or other of the 15 points of measure-

ment, and that subjects in all the Abstract Conditioning subgroups except the Temporal Control subgroup increased their body-sway from pre-trial to post-trial more when under instructions to do so than when under no such instructions. On the other hand, subjects in the Temporal Control subgroup, who were exposed to the condition wherein for 10 minutes instead of having anything to do with the stimulus box they circled N's on a page Xeroxed from the Amherst, Mass. telephone directory, increased their body-sway more from pre-trial to post-trial when under no instructions to do so than when under instructions to do so. There was a tendency approaching statistical significance for the subjects in the Temporal Control subgroup to increase their body-sway less under the Instructions condition than did subjects in the other three levels of Abstract Conditioning. Finally, summing across trials, there was an overall tendency for subjects who received instructions for increased body-sway to sway less (37.52 units, or 8.61 mm.) than subjects who did not receive such instructions (41.44 units, or 9.50 mm.).

With respect to the originally proposed analysis no interaction other than the Abstract Conditioning X Instructions X Trials interaction, and with respect to the analysis of difference scores no interaction other than the Abstract Conditioning X Instructions interaction, even approached statistical significance.

In view of the meager results of the analyses of variance performed upon body-sway scores, and in view of the earlier discovery that these data did not fulfill the homogeneity of variance assumption,

square-root, natural logarithm, and common logarithm transforms were carried out, and analyses of variance were performed upon the transformed scores.

The analyses of variance done on the square-root transforms and on the natural logarithm transforms produced essentially the same outcomes as the analyses done on nontransformed body-sway data, except that the Abstract Conditioning X Instructions X Trials effect reached statistical significance. The F-ratio for that source of variance for the square-root transform analysis was 3.11, significant beyond the .05 level on 3 and 96 d.f.. The F-ratio for the same source of variance for the natural logarithm transform analysis was 3.83, significant beyond the .025 level on 3 and 96 d.f.. In both of these analyses of transformed data, as in the analyses of nontransformed data, this interaction represented the fact that subjects exposed to the Temporal Control situation and not given instructions for increased body-sway increased their body-sway from pre-trial to post-trial more than subjects exposed to the Temporal Control situation and also given instructions for increased body-sway, while for all other Abstract Conditioning subgroups the reverse was the case.

Trials and Points of Measurement main effects remained significant in the square-root and natural logarithm transform analyses, as they were in the analyses of the nontransformed data. Furthermore, those interactions that were significantly insignificant in the analyses of the nontransformed data were, for the most part, significantly insignificant in these analyses of the transformed data. The exception was

the Placebo X Abstract Conditioning X Instructions X Trials interaction, which remained nonsignificant, but not significantly so.

The analysis of variance performed on the common logarithm transforms of body-sway turned up nothing new. Indeed, the Trials main effect disappeared, as did the approach to significance of the Abstract Conditioning X Instructions X Trials interaction.

Analysis of Subjective Effects Questionnaire data

Results of originally proposed analysis of variance. Subjects' scores from the Subjective Effects Questionnaire (SEQ) were subjected to analysis of variance. These scores, it will be recalled, represented ratings on a 5-point scale of subjective perception of body-sway change from pre-trial to post-trial, whereon subjects were required to indicate whether they had swayed much more (5), a little more (4), about the same (3), a little less (2), or much less (1), the second time they stood on the sway platform than the first time.

The main effect for Instructions was significant beyond the .001 level ($F(1,96) = 12.19$), indicating that the mean score of 3.30 for the Instructions subgroup was significantly lower than the mean score of 3.87 for the No Instructions subgroup.

Thus it can be stated that while both the Instructions and No Instructions subgroup subjects reported swaying more during the second trial on the sway platform than during the first trial, those in the No Instructions subgroup reported a greater degree of increase than those in the Instructions subgroup.

No other main effects or interaction effects even approached statistical significance.

Results of post-hoc analyses: SEQ distortion effects. Overall distortion effects: To get at the degree of association between objective change in body-sway from pre-trial to post-trial, and subjective reporting of that change, subjects were dichotomized on the one hand into those who objectively increased their sway vs. those who objectively decreased their sway, and on the other, into those who rated themselves above 3.0 on the SEQ vs. those who rated themselves at 3.0 or below. The resulting 2 X 2 contingency table is set forth as Table 3. A contingency coefficient computed for the data so cast had

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Insert Table 3 about here
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the value of .74, significant beyond the .02 level. This coefficient reflects the facts that 1) of the subjects who objectively increased their sway from pre-trial to post-trial, more rated themselves subjectively as increasing their sway, and fewer as remaining the same or as decreasing their sway, than would have been expected by chance, and that 2) of the subjects who objectively decreased their sway from pre-trial to post-trial, fewer rated themselves subjectively as increasing their sway, and more as remaining the same or decreasing their sway, than would have been expected by chance.

Because use of the contingency coefficient entailed considerable loss of information, refinement of the determination of the nature of the association between objective change in body-sway from pre-trial

Table 3: Subjective experience of body-sway change as a function of objective body-sway change

	Subjective Effects Questionnaire Ratings		
	Rated Self above 3.00	Rated Self at or below 3.00	
Increased Objective Body-Sway	46 (E = 39.48)	20 (E = 26.52)	66
Decreased Objective Body-Sway	21 (E = 27.52)	25 (E = 18.48)	46
	67	45	112

to post-trial, and subjective reporting of that change, was attempted by use of the Spearman rho. The 16 smallest subgroups of the experimental design were ranked according to mean difference between pre-trial and post-trial sway, and also according to mean SEQ score. With regard to objective body-sway, a negative mean difference between pre-trial sway and post-trial sway indicated that mean body-sway increased from pre-trial to post-trial. A positive mean difference indicated that mean body-sway decreased from pre-trial to post-trial. Decreased sway from pre-trial to post-trial was treated as a kind of "negative increase," which is to say that for the purposes of ranking, a negative constant was added to all the difference scores, so that the subgroup that had the greatest mean decrease from pre-trial to post-trial was ranked as having the lowest mean increase.

Table 4 shows the 16 smallest experimental subgroups, the mean difference scores on objective body-sway, the mean scores on the SEQ, and the rankings attained by each subgroup on objective body-sway and

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Insert Table 4 about here
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SEQ scores. With more of the information utilized, the Spearman rho, in this case a measure of association between objective increase or decrease in body-sway and subjective reporting of change in body-sway, was -.009, indicating that in fact when the subgroups of the experimental design were taken into account, subjects did very poorly in judging the direction and degree of their changes in body-sway from pre-trial to post-trial.

Table 4: The means of the smallest subgroups of the experimental matrix ranked on objective and subjective body-sway change

Subgroup	Objective Sway Mean	Subjective Sway Mean	Objective Sway Rank	Subjective Sway Rank
$F_1A_1G_1$	-4.36 (-1.00 mm.)	3.57	9.0	8.5
$F_1A_1G_2$	-4.19 (-0.96 mm.)	4.14	8.0	15.5
$F_1A_2G_1$	-6.57 (-1.51 mm.)	3.57	15.0	8.5
$F_1A_2G_2$	0.30 (0.07 mm.)	4.00	5.0	13.0
$F_1A_3G_1$	1.04 (0.24 mm.)	3.29	3.0	5.0
$F_1A_3G_2$	3.47 (0.80 mm.)	3.43	1.0	6.5
$F_1A_4G_1$	0.27 (0.07 mm.)	3.00	6.0	1.5
$F_1A_4G_2$	-6.35 (-1.47 mm.)	3.43	14.0	6.5
$F_2A_1G_1$	-5.44 (-1.25 mm.)	3.14	12.0	3.5
$F_2A_1G_2$	1.43 (0.33 mm.)	3.86	2.0	11.0
$F_2A_2G_1$	-4.44 (-1.02 mm.)	3.71	10.0	10.0
$F_2A_2G_2$	0.72 (0.17 mm.)	4.00	4.0	13.0
$F_2A_3G_1$	-5.94 (-1.36 mm.)	3.00	13.0	1.5
$F_2A_3G_2$	-4.48 (-1.03 mm.)	4.14	11.0	15.5
$F_2A_4G_1$	-0.99 (-0.23 mm.)	3.14	7.0	3.5
$F_2A_4G_2$	-11.68	4.00	16.0	13.0

Key: F_1 -- Pill; F_2 -- No Pill

A_1 -- Positive Abstract Conditioning; A_2 -- Negative Abstract Conditioning; A_3 -- Sensory Control; A_4 -- Temporal Control

G_1 -- Instructions; G_2 -- No Instructions

A look at Table 5a reveals that, of the subjects who decreased

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Insert Table 5 about here
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their objective sway from pre-trial to post-trial, 21 rated themselves as having increased their sway, 18 rated themselves as remaining the same, and only 7 rated themselves as having decreased their sway. Subjects who increased their sway did somewhat better, with 46 of them rating themselves as having increased, 17 rating themselves as remaining the same, and only 3 rating themselves as decreasing.

The difference between subjects who increased and subjects who decreased their sway, in the accuracy with which they assessed their own changes in sway, is more clearly seen in Table 5b, wherein increasers are compared with decreasers in terms of whether their SEQ ratings were congruent with or discongruent with the objective direction of sway change. The chi-square for this 2 X 2 table is seen to be 39.38, which, on 1 d.f., is significant beyond the .001 level, with the major contribution to chi-square coming from the Decreaser-Discongruent cell. Subjects who decreased their objective sway reported that they had increased it, and only infrequently said that they had decreased it, whereas on the other hand, subjects who increased their objective sway reported that they had done so, and only rarely reported that they had decreased it.

In terms of Oscar K units, subjects who increased their sway increased it more than subjects who decreased their sway decreased it. The average increase for those who increased was 8.99 units (2.06 mm.),

Table 5

Table 5a: Frequencies in three categories of Subjective Effects Questionnaire ratings as a function of objective body-sway change direction

	Subjective Effects Questionnaire Ratings			
	Increased	No Change	Decreased	
Increased Objective Sway	46 (E = 39.48)	17 (E = 20.63)	3 (E = 5.89)	66
Decreased Objective Sway	21 (E = 27.52)	18 (E = 14.37)	7 (E = 4.11)	46
	67	35	10	112

Chi-square = 7.63

p less than .001

Table 5b: Frequencies of subjects whose Subjective Effects Questionnaire ratings were congruent with the direction of their objective body-sway change, as a function of direction of objective body-sway change

	Subjective Effects Questionnaire Ratings		
	Congruent with Direction of Objective Sway Change	Discongruent with Direction of Objective Sway Change	
Increased Objective Sway	46 (E = 33.73)	3 (E = 15.27)	49
Decreased Objective Sway	7 (E = 19.27)	21 (E = 8.73)	28
	53	24	77

Chi-square = 39.38

p less than .001

while the average decrease for those who decreased was 5.71 units (1.31 mm.). A t-test on the difference between these means was non-significant, but this fact does not eliminate the possibility that the difference in accuracy of subjective judgement of sway performance between those who increased their objective sway and those who decreased their objective sway is due to the fact that the increasers were judging larger changes than were the decreasers.

SEQ distortion of absolute sway change: An investigation was made of the distribution in the experimental design matrix, of subjects who, irrespective of the direction in which objective body-sway had changed, judged their sway performance accurately, vs. subjects who did not judge their sway performance accurately.

By those who judged their sway performance accurately is meant those who rated themselves on the SEQ as increasing their sway when they in fact had increased it, and those who rated themselves on the SEQ as decreasing their sway when they had in fact decreased it.

Two sets of criteria were used for identifying inaccurate judges. Under the first set of criteria, inaccurate judges were 1) subjects who said they had remained the same when in fact they had either increased or decreased their sway, 2) subjects who said they had increased their sway when in fact they had decreased it, and 3) subjects who said they had decreased their sway when in fact they had increased it.

Under the second set of criteria, inaccurate judges were only those subjects who said they had remained the same when in fact they

had either increased or decreased their sway.

Lest it be objected that those inaccurate judges who said they had remained the same when in fact they had either increased or decreased their sway, were subjects who changed their objective sway relatively little and thus had a more difficult time discerning that any change had taken place, while those who increased their sway and said they increased it, and those who decreased their sway and said they decreased it, were judging larger changes, it must be reported that, overall, subjects who rated themselves as not changing their sway changed an average of 7.15 units (1.64 mm.), while those who rated themselves as changing in one direction or another when in reality they had, changed an average of 7.41 units (1.70 mm.). The difference between these means is minuscule and clearly nonsignificant, and while statistical nonsignificance does not completely rule out the possibility that the .26 unit (.06 mm.) difference observed between the aforementioned means indexes the difference between discernible sway changes and indiscernible sway changes, the hypothesis becomes somewhat weak.

SEQ distortion associated with receipt of the pill: When the first set of criteria for identifying inaccurate judges is used, the distribution of accurate vs. inaccurate judges of body-sway change with respect to the Pill and No Pill experimental subgroups may be

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Insert Table 6 about here
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seen in Table 6a. It can be observed that, of subjects who received

Table 6

Table 6a: Criterion 1 distortion on the Subjective Effects Questionnaire as a function of the Placebo variable

	Subjective Effects Questionnaire Ratings		
	Congruent	No Change or Discongruent	
Pill	21 (E = 26.50)	35 (E = 29.50)	56
No Pill	32 (E = 26.50)	24 (E = 29.50)	56
	53	59	112

Chi-square = 4.33

p less than .05

Table 6b: Criterion 2 distortion on the Subjective Effects Questionnaire as a function of the Placebo variable

	Subjective Effects Questionnaire Ratings		
	Congruent	No Change	
Pill	21 (E = 27.10)	24 (E = 17.90)	45
No Pill	32 (E = 25.90)	11 (E = 17.10)	43
	53	35	88

Chi-square = 7.07

p less than .01

the pill, 21 rated the direction of body-sway change from pre-trial to post-trial accurately, while 35 said either that their sway remained the same, or that it changed in a direction opposite to that of the actual change. Of subjects who did not receive the pill, 32 rated the direction of body-sway change from pre-trial to post-trial accurately, while 24 said either that their sway remained the same, or that it changed in a direction opposite to that of the actual change. The chi-square for this table may be seen to be 4.33, on 1 d.f., significant beyond the .05 level.

It cannot be asserted that the difference between the Pill subgroup and the No Pill subgroup is due to the easier discriminability of the objective body-sway changes with which the No Pill subgroup was dealing. Easier discriminability would have to mean greater body-sway changes in the No Pill subgroup, when in fact the greater body-sway changes were observed in the Pill subgroup. Mean absolute (i.e. regardless of direction of change) body-sway change for the Pill subgroup was 8.60 units (1.97 mm.), while that for the No Pill subgroup was 6.68 units (1.53 mm.). The difference between these means, tested by an analysis of variance, is nonsignificant, but the force of the argument is not thereby diminished.

When the second set of criteria for identifying inaccurate judges is applied, the distribution of accurate vs. inaccurate judges of body-sway change with respect to membership in the Pill and No Pill experimental subgroups appeared as in Table 6b. Table 6b shows that of subjects who received the pill, 21 rated the direction of body-sway

change from pre-trial to post-trial accurately, while 24 said that their sway remained the same. Of subjects who did not receive the pill, 32 rated the direction of body-sway change from pre-trial to post-trial accurately, while only 11 said that their sway remained the same. The chi-square for this table may be seen to be 7.07, on 1 d.f., significant beyond the .01 level.

Differing ease of discriminability of body-sway change between the Pill and No Pill subgroups can be eliminated as a cause of this outcome for the reasons outlined above, namely, that on the average those in the Pill subgroup changed their body-sway from pre-trial to post-trial more than did those in the No Pill subgroup, and hence were working with larger, and presumably more discriminable changes, than were those in the No Pill subgroup.

It can therefore be concluded that subjects who got the pill showed a significant tendency to report subjectively that their body-sway had not changed from pre-trial to post-trial, when in fact it had, while subjects who did not get the pill showed a significant tendency to report accurately that their sway had changed.

SEQ distortion associated with receipt of instructions: Tables 7a and 7b show what happened when the distribution in the experimental

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Insert Table 7 about here
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design matrix of accurate vs. inaccurate judges with respect to membership in the Instructions and No Instructions subgroups was examined. Table 7a was cast using the first set of criteria for identifying in-

Table 7

Table 7a: Criterion 1 distortion on the Subjective Effects Questionnaire as a function of the Instructions variable

	Subjective Effects Questionnaire Ratings		
	Congruent	No Change or Discongruent	
Instructions	21 (E = 26.50)	35 (E = 29.50)	56
No Instructions	32 (E = 26.50)	24 (E = 29.50)	56
	53	59	112

Chi-square = 4.33

p less than .05

Table 7b: Criterion 2 distortion on the Subjective Effects Questionnaire as a function of the Instructions variable

	Subjective Effects Questionnaire Ratings		
	Congruent	No Change	
Instructions	21 (E = 27.70)	25 (E = 18.30)	46
No Instructions	32 (E = 25.30)	10 (E = 16.70)	42
	53	35	88

Chi-square = 8.54

p less than .01

accurate judges. Table 7b was cast using the second set of criteria. The results parallel those obtained when the crucial experimental subgroups were the Pill and No Pill subgroups.

Table 7a shows that of subjects who received instructions for increased body-sway from pre-trial to post-trial, 21 reported accurately on the SEQ the direction of change, while 35 reported inaccurately on the SEQ either that their sway remained the same, or that it changed in a direction opposite to that of the actual change. Of subjects who did not receive instructions for increased body-sway from pre-trial to post-trial, 32 reported accurately on the SEQ the direction of change, while 24 reported either that their sway remained the same, or that it changed in a direction opposite to that of the actual change. The chi-square for Table 7a is seen to be 4.33, on 1 d.f., significant beyond the .05 level.

Table 7b shows that of subjects who received instructions for increased body-sway from pre-trial to post-trial, 21 reported accurately on the SEQ the direction of change, while 25 reported on the SEQ, inaccurately, that their sway remained the same. Of subjects who received no instructions for increased body-sway, 32 reported accurately on the SEQ the direction of change, while only 10 reported on the SEQ, inaccurately, that their sway remained the same. The chi-square for Table 7b is seen to be 8.54, on 1 d.f., significant beyond the .01 level.

Once again, if differing levels of discriminability of objective body-sway change account for the differences between the Instructions

and No Instructions subgroups with respect to accuracy of subjective judgements of body-sway change, then the subgroup the subjects of which dealt with the larger and presumably more discriminable objective body-sway changes should have been the subgroup manifesting more accurate judges than inaccurate ones, and the subgroup the subjects of which dealt with the smaller and presumably less discriminable objective body-sway changes should have been the subgroup manifesting more inaccurate judges than accurate ones. It can be seen, however, that the Instructions subgroup, with the larger objective body-sway changes (8.66 units, or 1.99 mm.), manifested fewer accurate judges than inaccurate ones, while the No Instructions subgroup, with the smaller objective body-sway changes (6.63 units, or 1.52 mm.), produced more accurate judges than inaccurate ones.

It can be concluded that subjects given instructions for increased body-sway showed a significant tendency to report that they had not changed their sway from pre-trial to post-trial when in fact they had, while subjects not given instructions for increased body-sway showed a significant tendency to report change and direction of change accurately.

SEQ distortion associated with Abstract Conditioning: Determination of the distribution of accurate vs. inaccurate judges with respect to the Abstract Conditioning subgroups indicated that the differing Abstract Conditioning subgroups were not associated with significantly different frequencies of subjects falling into the accurate judge category as opposed to the inaccurate judge category. Nor did any of the

6 possible comparisons between various dyads of the Abstract Conditioning subgroups result in significantly different frequencies of subjects falling into the accurate judge category as opposed to the inaccurate judge category. One can only note in passing that the Temporal Control subgroup was the only Abstract Conditioning subgroup in which, under either set of criteria, more subjects who changed their sway rated the change inaccurately on the SEQ, than rated it accurately.

The conclusion to be drawn is that the various Abstract Conditioning manipulations did not in any notable way or with any degree of reliability differentially influence the accuracy of SEQ ratings.

SEQ distortion associated with membership in subgroups relevant to the Placebo X Instructions interaction: Tables 8a and 8b show the numbers of subjects falling into the accurate judge category and the

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Insert Table 8 about here
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inaccurate judge category under, respectively, the first and second sets of criteria for inaccuracy, as a function of membership in the subgroups of the experimental matrix relevant to the Placebo X Instructions interaction.

No matter which criterion for accuracy is used, there are significant differences in the distributions of subjects into the accurate judge vs. the inaccurate judge categories, depending upon which combinations of the two experimental variables under consideration the subjects were exposed to.

Table 8

Table 8a: Criterion 1 distortion on the Subjective Effects Questionnaire as a function of membership in subgroups relevant to the Placebo X Instructions interaction

Subjective Effects Questionnaire Ratings

	Congruent	No Change or Discongruent	
Pill and Instructions	8 (E = 13.25)	20 (E = 14.75)	28
Pill and No Instructions	13 (E = 13.25)	15 (E = 14.75)	28
No Pill and Instructions	13 (E = 13.25)	15 (E = 14.75)	28
No Pill and No Instructions	19 (E = 13.25)	9 (E = 14.75)	28
	53	59	112

Chi-square = 8.70

p less than .05

Table 8, continued

Table 8b: Criterion 2 distortion on the Subjective Effects Questionnaire as a function of membership in subgroups relevant to the Placebo X Instructions interaction

	Subjective Effects Questionnaire Ratings		
	Congruent	No Change	
Pill and Instructions	8 (E = 13.85)	15 (E = 9.15)	23
Pill and No Instructions	13 (E = 13.25)	9 (E = 8.75)	22
No Pill and Instructions	13 (E = 13.85)	10 (E = 9.15)	23
No Pill and No Instructions	19 (E = 12.05)	1 (E = 7.95)	20
	53	35	88

Chi-square = 16.44

p less than .001

When the first criterion for inaccuracy is used, the chi-square generated by those differences is 8.70, on 3 d.f., significant beyond the .05 level (Table 8a). When the second criterion for inaccuracy is used, the chi-square generated by the differences is 16.44, on 3 d.f., significant beyond the .001 level.

For both Table 8a and Table 8b, the largest contributions to the chi-squares come from, on the one hand, the row containing subjects who received both a pill and instructions for increased body-sway, and on the other hand, from the row containing subjects who neither received a pill nor instructions for increased body-sway. It is therefore manifest that the significance of the Placebo X Instructions interaction with respect to accuracy of SEQ ratings is due to the difference between these two experimental subgroups. Tables 9a and 9b

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Insert Table 9 about here
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represent direct comparisons of the two experimental subgroups in question, Table 9a when the first set of criteria for judgemental inaccuracy is used, and Table 9b when the second set of criteria for judgemental inaccuracy is used. The chi-square generated by Table 9a may be seen to be 8.66, on 1 d.f., significant beyond the .01 level. That generated by Table 9b may be seen to be 16.44, on 1 d.f., significant beyond the .001 level.

The mean absolute sway changes from pre-trial to post-trial for the subgroups relevant to the Placebo X Instructions interaction are presented in Table 10, inspection of which indicates that the subjects

Table 9

Table 9a: Criterion 1 comparison of the (Pill).(Instructions) and (No Pill).(No Instructions) subgroups

Subjective Effects Questionnaire Ratings

	Congruent	No Change or Discongruent	
Pill and Instructions	8 (E = 13.50)	20 (E = 14.50)	28
No Pill and No Instructions	19 (E = 13.50)	9 (E = 14.50)	28
	27	29	56

Chi-square = 8.66

p less than .01

Table 9b: Criterion 2 comparison of the (Pill).(Instructions) and (No Pill).(No Instructions) subgroups

Subjective Effects Questionnaire Ratings

	Congruent	No Change	
Pill and Instructions	8 (E = 14.44)	15 (E = 8.56)	23
No Pill and	19 (E = 12.56)	1 (E = 7.44)	20
	27	16	43

Chi-square = 16.59

p less than .001

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Insert Table 10 about here
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in the (Pill).(Instructions) subgroup changed their body-sway more on the average from pre-trial to post-trial, than did subjects in the (No Pill).(No Instructions) subgroup. Therefore the differences in SEQ accuracy between the two subgroups are not likely to be due to differences in discriminability of sway changes.

The reasonable conclusion to be drawn is that when subjects were exposed to both the pill and to instructions for increased body-sway from pre-trial to post-trial, if they changed their sway at all, they were significantly less likely than subjects exposed to neither the pill nor to instructions for increased body-sway, to report correctly the direction of sway change, and much more likely to claim either that their sway had not changed, or that it had changed in a direction opposite to that of its actual change. These inaccuracies occurred despite the fact that subjects in the (Pill).(Instructions) subgroup were dealing with larger average sway changes and, one might logically infer, more discriminable sway changes, than were subjects in the (No Pill).(No Instructions) subgroup.

Subjects who received a pill but no instructions for increased body-sway, and subjects who received instructions for increased body-sway but no pill, were equally inaccurate on the SEQ, under the first criterion for SEQ inaccuracy, but the degree of their inaccuracy is statistically nonsignificant. Each of these subgroups contributed 13 out of 28 subjects to the accurate judge category, and 15 out of

Table 10: Mean objective absolute body-sway changes of subgroups relevant to the Placebo X Instructions interaction

	Instructions	No Instructions
Pill	10.24 (2.35 mm.)	6.97 (1.60 mm.)
No Pill	7.08 (1.62 mm.)	6.28 (1.17 mm.)

Table 11: Fundamental differences between Subjective Effects Questionnaire ratings of those whose objective body-sway increased from pre-trial to post-trial and those whose objective body-sway decreased from pre-trial to post-trial

	Subjective Effects Questionnaire Ratings: Congruent	No Change or Discongruent	
Increased Objective Sway	46 (E = 31.23)	20 (E = 34.77)	66
Decreased Objective Sway	7 (E = 21.77)	39 (E = 24.33)	46
	53	59	112

Chi-square = 32.39

p less than .001

28 subjects to the inaccurate judge category.

On the face of it, neither of these two subgroups was as inaccurate on the SEQ as the (Pill).(Instructions) subgroup, and in fact there is a tendency for the difference in SEQ accuracy between these two subgroups and the (Pill).(Instructions) subgroup to approach statistical significance -- the chi-square yielded by a comparison of either of these two subgroups with the (Pill).(Instructions) subgroup is 1.90, on 1 d.f., significant between the .20 and .10 levels.

At the same time these two subgroups, clearly not as accurate as the (No Pill).(No Instructions) subgroup, are less accurate than that subgroup to a degree approaching statistical significance -- the chi-square associated with a direct comparison of either the (Pill).(No Instructions) or the (No Pill).(Instructions) subgroup with the (No Pill).(No Instructions) subgroup is 2.63, on 1 d.f., significant between the .20 and .10 levels.

Under the second set of criteria for SEQ inaccuracy, the (Pill).(No Instructions) subgroup contributed 13 subjects to the accurate judge category and 9 subjects to the inaccurate judge category, while the (No Pill).(Instructions) subgroup contributed 13 subjects to the accurate judge category and 10 subjects to the inaccurate judge category. Both of these subgroups remain, under the second set of criteria for judgemental inaccuracy, more accurate than the (Pill).(Instructions) subgroup, but still not significantly so -- the chi-square generated by a direct comparison of the combined (Pill).(No Instructions) and (No Pill).(Instructions) subgroups with the (Pill).(Instructions)

subgroup is 3.22, on 1 d.f., significant between the .10 and .05 levels.

At the same time, under the second set of criteria for judgemental inaccuracy, the chi-square yielded by a direct comparison of the combined (Pill).(No Instructions) and (No Pill).(Instructions) subgroups with the (No Pill).(No Instructions) subgroup, is 8.99, on 1 d.f., significant beyond the .01 level, indicating that the two experimental subgroups under consideration were significantly less accurate on the SEQ than the (No Pill).(No Instructions) subgroup.

Reference may be made to Table 10 to verify that, although under both SEQ inaccuracy criteria the (Pill).(No Instructions) subgroup and the (No Pill).(Instructions) subgroup showed a tendency to be more accurate than the (Pill).(Instructions) subgroup and less accurate than the (No Pill).(No Instructions) subgroup, the mean absolute body-sway changes for the (Pill).(No Instructions) subgroup and for the (No Pill).(Instructions) subgroup were, on the one hand, less than those for the (Pill).(Instructions) subgroup, and on the other, greater than those for the (No Pill).(No Instructions) subgroup. Differences in discriminability of body-sway change as the crucial factor in the observed differences in SEQ accuracy are therefore ruled out.

The results of the post-hoc analysis of subgroups relevant to the Placebo X Instructions interaction with respect to accuracy of subjective judgement of body-sway change from pre-trial to post-trial may be summarized as follows: 1) subjects in the (Pill).(Instructions)

subgroup showed a significant tendency to report that they had not changed their sway when in fact they had, or that they had changed their sway in a direction opposite to that of the actual objective change; 2) subjects in both the (Pill).(No Instructions) and (No Pill).(Instructions) subgroups tended to report correctly changes in body-sway and direction of changes in body-sway more frequently than those in the (Pill).(Instructions) subgroup and less frequently than those in the (No Pill).(No Instructions) subgroup; and 3) subjects in the (No Pill).(No Instructions) subgroup showed a significant tendency to report that they had changed their body-sway when in fact they had, and to report correctly the direction of the change.

Omission of detailed post-hoc investigation of SEQ distortion associated with other main and interaction effects: No SEQ distortion effects were associated with the levels of the Abstract Conditioning variable.

The post-hoc analytical procedure, cumbersome to begin with, increased in cumbersomeness as a direct function of increasing subdivision of the experimental matrix into subgroups relevant to the Placebo X Abstract Conditioning, Abstract Conditioning X Instructions, and Placebo X Abstract Conditioning X Instructions interactions, the relevant subgroups dichotomized further into accurate judge vs. inaccurate judge categories. In addition, more extensive subdivision of the experimental matrix began to generate frequency counts too small for the meaningful application of statistical techniques. Finally, and most importantly, the apparent yield of interpretable outcomes, determined

by a rough scanning of the data, was far too trivial to warrant the extended effort of formal analysis. For these reasons it was deemed necessary and desirable to omit from consideration detailed post-hoc analysis of the Placebo X Abstract Conditioning, Abstract Conditioning X Instructions, and Placebo X Abstract Conditioning X Instructions interactions.

SEQ distortion as a function of direction of body-sway change and of various experimental conditions: Introduction: Reconsideration of Table 5 and consideration of Table 11 brings to light two facts, first

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Insert Table 11 about here
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that subjects who increased their body-sway were more accurate on the SEQ than subjects who decreased their body-sway, and second, that when subjects who increased their body-sway were inaccurate on the SEQ, they were inaccurate in a manner different from the inaccuracy on the SEQ of subjects who decreased their body-sway. By way of amplifying upon the second fact just given, let it be pointed out that sway increasers, when they were inaccurate, erred by claiming to have remained the same from pre-trial to post-trial when in fact they had increased, rather than by claiming they had decreased, whereas sway decreasers, when they were inaccurate, erred by claiming they had increased when in fact they had decreased, rather than by claiming they had remained the same.

In view of these differences, it seemed to make sense, in the post-hoc investigation of the influence of the principal experimental variables on frequencies of sway increasers and sway decreasers falling

into accurate judge vs. inaccurate judge categories, to use one set of standards of SEQ accuracy for the sway increasers, and another set of standards of SEQ accuracy for the sway decreasers.

Because most of the sway increasers rated themselves on the SEQ as either increasing their sway or as remaining the same (46 and 17, respectively), while only a very few (3) rated themselves as decreasing their sway, inaccuracy for sway increasers was held to consist of rating oneself as not changing body-sway from pre-trial to post-trial, or of rating oneself as decreasing, when one had in fact increased. Accuracy, of course, was held to consist of rating oneself as increasing when one had in fact increased.

Because most of the sway decreasers rated themselves on the SEQ as either increasing their sway (21 subjects) or as remaining the same (18 subjects), while only 7 subjects rated themselves as decreasing their sway, inaccuracy for sway decreasers was held to consist of rating oneself as increasing one's sway from pre-trial to post-trial when in fact one had decreased. Accuracy, for sway decreasers, was held to consist of rating oneself as decreasing when in fact one had decreased, or of rating oneself as remaining the same when in fact one had decreased.

Although these standards for SEQ accuracy for the sway decreasers may sound peculiar, there is common-sense justification for them, in that it is certainly true that those who said they remained the same when in fact they decreased, are more accurate than those who said they increased when in fact they decreased.

SEQ distortion by sway increasers: 66 subjects increased their body-sway from pre-trial to post-trial. An investigation was made of the distribution of these sway increasers into accurate judge vs. inaccurate judge categories as a function, first, of the Placebo variable, second, of the Instructions variable, and third, of the interaction of the Placebo and Instructions variables.

Formal examination of the Abstract Conditioning main effect, and of the interactions of Abstract Conditioning with the Placebo variable, the Instructions variable, and the Placebo X Instructions interaction, were omitted, since, on the one hand, an informal survey of the relevant data revealed a dearth of noteworthy outcomes, and since, on the other, the continued subdivision of the experimental matrix that would have been necessary for formal analysis would have resulted in frequency counts too small to permit the use of any statistical tests other than Fisher's exact probability test. Aside from the fact that computation of exact probabilities tends to be burdensome, the Fisher exact probabilities test can be used only when the data are cast in 2 X 2 tables, whereas the Abstract Conditioning X SEQ Accuracy layout would generate a 2 X 4 table, the Abstract Conditioning X Placebo X SEQ Accuracy and the Abstract Conditioning X Instructions X SEQ Accuracy layouts would each generate a 2 X 8 table, and the Abstract Conditioning X Placebo X Instructions X SEQ Accuracy layout would generate a 2 X 16 table.

SEQ distortion associated with receipt of the pill: Table 12a sets forth the distribution of sway increasers into accurate judge vs.

inaccurate judge categories as a function of membership in the experimental design matrix subgroups relevant to the Placebo main effect.

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Insert Table 12 about here
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Of the 31 sway increasers who received the pill, 19 (62%) rated the direction of change accurately, while 12 (38%) reported on the SEQ either that they had not changed their sway from pre-trial to post-trial, or that they had decreased their sway. It will be recalled that only 3 subjects who increased their sway rated themselves on the SEQ as decreasing it; of these 3, 1 fell into the Pill subgroup.

The 35 sway increasers in the No Pill subgroup were so distributed that 27 (77%) of them fell into the accurate judge category, while only 8 (23%) fell into the inaccurate judge category. Two of the 3 sway increasers who rated themselves as decreasing their sway fell into the No Pill subgroup.

The chi-square associated with Table 12a may be seen to be 1.96, on 1 d.f., significant between the .20 and .10 levels. Thus, while there was no acceptably significant difference between the distribution of subjects falling into the accurate judge vs. inaccurate judge categories as a function of membership in the experimental design matrix subgroups relevant to the Placebo main effect, there was an apparent tendency for sway increasers who received the pill to claim on the SEQ more frequently than would have been expected by chance that they had either not changed their sway or had decreased it, and less

Table 12

Table 12a: Effect of the Placebo variable on the Subjective Effects Questionnaire accuracy of those whose body-sway increased from pre-trial to post-trial

	Subjective Effects Questionnaire Ratings		
	Accurate	Inaccurate	
Pill	19 (E = 21.61)	12 (E = 9.39)	31
No Pill	27 (E = 24.39)	8 (E = 10.61)	35
	46	20	66

Chi-square = 1.96

p between .20 and .10

Table 12b: Effect of the Instructions variable on the Subjective Effects Questionnaire accuracy of those whose body-sway increased from pre-trial to post-trial

	Subjective Effects Questionnaire Ratings		
	Accurate	Inaccurate	
Instructions	17 (E = 20.91)	13 (E = 9.09)	30
No Instructions	29 (E = 25.09)	7 (E = 10.91)	36
	46	20	66

Chi-square = 4.42

p less than .05

Table 12, continued

Table 12c: Effect of membership in subgroups relevant to the Placebo X Instructions interaction, on Subjective Effects Questionnaire accuracy of those whose body-sway increased from pre-trial to post-trial

	Subjective Effects Questionnaire Ratings		
	Accurate	Inaccurate	
Pill and Instructions	7 (E = 9.76)	7 (E = 4.24)	14
Pill and No Instructions	12 (E = 11.85)	5 (E = 5.15)	17
No Pill and Instructions	10 (E = 11.15)	6 (E = 4.85)	16
No Pill and No Instructions	17 (E = 13.24)	2 (E = 5.76)	19
	46	20	66

Chi-square = 6.50

p between .10 and .05

Table 12d: Comparison of the Subjective Effects Questionnaire accuracy of sway increasers in the (Pill).(Instructions) and (No Pill).(No Instructions) subgroups

	Subjective Effects Questionnaire Ratings		
	Accurate	Inaccurate	
Pill and Instructions	7 (E = 10.18)	7 (E = 3.82)	14
No Pill and	17 (E = 13.82)	2 (E = 5.18)	19
	24	9	33

Chi-square = 6.32

p less than .02

frequently than would have been expected by chance that they had increased it. For sway increasers who did not receive the pill, the effect was reversed: more of these subjects than would have been expected by chance to do so reported correctly on the SEQ that they had increased their sway, while fewer than would have been expected by chance to do so reported on the SEQ that they had remained the same or decreased their sway.

Sway increasers in the Pill subgroup increased their sway an average of 9.62 units (2.21 mm.); those in the No Pill subgroup increased their sway an average of 8.41 units (1.93 mm.). This circumstance mitigates against the possibility that the differences in accuracy between the two subgroups were due to the greater discriminability of the larger sway increases. Were that the case, the Pill subgroup should have been more accurate than the No Pill subgroup, whereas in actuality the reverse obtains.

SEQ distortion associated with receipt of instructions: Table 12b presents distributions of sway increasers into accurate vs. inaccurate judge categories as a function of membership in the experimental design matrix subgroups relevant to the Instructions variable.

It can be seen that of the 30 sway increasers in the subgroup that received instructions for increased body-sway, 17 (56%) rated themselves on the SEQ as increasing their sway, and 13 (44%) as either remaining the same or as decreasing. Of the 36 sway increasers in the subgroup that did not receive instructions for increased body-sway, 29 (81%) rated themselves on the SEQ as increasing their sway, and only 7 (19%) as remaining the same or as decreasing. Actually, very few subjects

who increased their sway reported that they had decreased it -- there were only 3 such subjects in the entire experiment, 2 of them falling into the Instructions subgroup, and one of them falling into the No Instructions subgroup. The chi-square for Table 12b may be seen to be 4.42, on 1 d.f., significant beyond the .05 level.

The mean sway increase for subjects in the Instructions subgroup who in fact increased their objective body-sway was 11.17 units (2.56 mm.); that for those in the No Instructions subgroup was 7.17 units (1.64 mm.) Overall, then, the Instructions subgroup sway increasers increased their sway to a greater extent than did the No Instructions subgroup sway increasers. Therefore the greater accuracy of the SEQ ratings of the No Instructions subgroup sway increasers as compared to those of the Instructions subgroup sway increasers, cannot be due to the greater discriminability of the sway increases with which sway increasers in the No Instructions subgroup were dealing.

SEQ distortion associated with membership in subgroups relevant to the Placebo X Instructions interaction: Table 12c sets forth the dichotomization of sway increasers into accurate judges vs. inaccurate judges as a function of membership in those subgroups of the experimental design matrix that were relevant to the Placebo X Instructions interaction.

Of the 14 sway increasers in the (Pill).(Instructions) subgroup, 7 (50%) were accurate judges on the SEQ, and 7 (50%) were inaccurate judges on the SEQ. The (Pill).(No Instructions) subgroup yielded a distribution of 12 (71%) accurate judges as opposed to 5 (29%) inac-

inaccurate judges, the (No Pill).(Instructions) subgroup yielded a distribution of 10 (62%) accurate judges as opposed to 6 (38%) inaccurate judges, and the (No Pill).(No Instructions) subgroup yielded a distribution of 17 (90%) accurate judges as opposed to only 2 (10%) inaccurate judges. Table 12c may be seen to generate a chi-square of 6.50, on 3 d.f., significant between the .10 and .05 levels.

This chi-square represents the facts that although the numbers of subjects falling into the accurate judge vs. inaccurate judge categories for the (Pill).(No Instructions) subgroup and for the (No Pill).(Instructions) subgroup did not depart from chance expectation, there were in the (Pill).(Instructions) subgroup fewer accurate judges on the SEQ and more inaccurate judges on the SEQ than would have been expected by chance, and there were in the (No Pill).(No Instructions) subgroup more accurate judges on the SEQ and fewer inaccurate judges on the SEQ than would have been expected by chance.

A direct comparison of the (Pill).(Instructions) subgroup and the (No Pill).(No Instructions) subgroup is represented in Table 12d. Because the d.f. is less than 2 and the expected frequency in one cell is below 5, the statistical analysis applied to this comparison was the Fisher exact probability test, which indicated that the two subgroups differ beyond the .02 level in frequencies of sway increasers falling into the accurate judge as opposed to the inaccurate judge category. In other words, sway increasers in the (Pill).(Instructions) subgroup were significantly less accurate in subjective judgements of their sway change and its direction, than were sway increasers in the (No

Pill).(No Instructions) subgroup. That is to say, sway increasers in the former subgroup showed a significant tendency to report either that their sway had remained the same or that they had decreased it, when in fact they had increased it, and sway increasers in the latter subgroup showed a significant tendency to report correctly their sway change and its direction.

The contribution of differences in ease of discriminability of the sway increases dealt with by subjects in the (Pill).(Instructions) subgroup and in the (No Pill).(No Instructions) subgroup, to the observed differences in SEQ accuracy between the two subgroups, is eliminated from consideration by the circumstance that sway increasers in the former, less accurate subgroup produced an average sway increase of 12.64 units (2.90 mm.), while sway increasers in the latter, more accurate subgroup produced an average sway increase of 7.18 units (1.65 mm.).

The (Pill).(No Instructions) subgroup and the (No Pill).(Instructions) subgroup do not differ significantly from one another. A comparison of the pooled frequency counts for these two subgroups with, on the one hand, the frequency counts for the (Pill).(Instructions) subgroup and, on the other, for the (No Pill).(No Instructions) subgroup, indicated that the (Pill).(No Instructions) subgroup subjects and the (No Pill).(Instructions) subgroup subjects, although more accurate on the SEQ than the (Pill).(Instructions) subgroup subjects, were not significantly more accurate, the chi-square generated by a direct comparison attaining a value of 1.16, on 1 d.f., with the as-

sociated probability lying between .30 and .20; and although less accurate on the SEQ than the (No Pill).(No Instructions) subgroup subjects, were not significantly less accurate, the chi-square generated by a direct comparison attaining a value of 2.24, on 1 d.f., with the associated probability lying between .20 and .10. In neither case can the observed differences be attributed to differences in discriminability of body-sway increases, since the mean sway increase for the two combined subgroups was 8.46 units (1.94 mm.), which was less than the mean sway increase of 12.64 units (2.90 mm.) of the less accurate (Pill).(Instructions) subgroup, and more than the mean sway increase of 7.18 units (1.65 mm.) of the (No Pill).(No Instructions) subgroup.

It should be pointed out that while there is no significant difference between the (Pill).(No Instructions) subgroup and the (No Pill).(Instructions) subgroup with respect to frequencies of sway increasers falling into the accurate judge as opposed to the inaccurate judge categories, 71% of the sway increasers in the former subgroup were accurate judges, while only 62% of the sway increasers in the latter subgroup were accurate judges. It is highly likely that chance accounts for this differences, but let it be noted that, once again, differences in ease of discriminability must be ruled out as an explanation of the differences, since the sway increasers in the subgroup that produced the greater SEQ accuracy, the (Pill).(No Instructions) subgroup, were dealing with smaller mean sway increases (7.13 units, or 1.64 mm.), than were sway increasers in the subgroup that produced the lesser SEQ accuracy, the (No Pill).(Instructions) sub-

group (9.87 units, or 2.26 mm.).

The results of these analyses of the effects on the SEQ accuracy of sway increasers as a function of membership in the subgroups relevant to the Placebo and Instructions main effects and to the Placebo X Instructions interaction effect, may be summarized as follows: 1) sway increasers in the Pill subgroup tended to be less accurate on the SEQ than sway increasers in the No Pill subgroup; 2) sway increasers in the Instructions subgroup were significantly less accurate on the SEQ than sway increasers in the No Instructions subgroup; 3) sway increasers in the (Pill).(Instructions) subgroup were significantly less accurate on the SEQ than sway increasers in the (No Pill).(No Instructions) subgroup; 4) sway increasers in the (Pill).(No Instructions) subgroup and the (No Pill).(Instructions) subgroup tended to be slightly more accurate on the SEQ than sway increasers in the (Pill).(Instructions) subgroup, and slightly less accurate on the SEQ than sway increasers in the (No Pill).(No Instructions) subgroup.

SEQ distortion by sway decreasers: It will be recalled that for subjects who decreased their sway from pre-trial to post-trial, the criterion for SEQ accuracy was different from the criterion for SEQ accuracy for subjects who increased their sway. Sway decreasers were said to be accurate on the SEQ if they reported thereon that they decreased their sway from pre-trial to post-trial or if they reported that their sway remained the same. Only if a sway decreaser reported on the SEQ that her sway increased, was she classified as being inaccurate. Classifying as accurate on the SEQ those sway decreasers who

reported that their sway remained the same from pre-trial to post-trial, was felt to be justified on the grounds that such subjects, although apparently mistaken about whether they had changed their sway at all from pre-trial to post-trial, were not so mistaken as also to misjudge the direction of the change.

46 subjects decreased their body-sway from pre-trial to post-trial. An investigation was made of the distribution of these sway decreasers into accurate on the SEQ vs. inaccurate on the SEQ categories, as a function of membership in experimental design matrix subgroups relevant to the Placebo and Instructions main effects, and to the Placebo X Instructions interaction effect. Formal investigation of other main and interaction effects was omitted, for reasons presented earlier in connection with the sway increasers -- the cumbersomeness of the post-hoc analytical procedure, the decreasing frequencies within cells as a function of further partitioning of the experimental design matrix, and the failure of informal examination of the data to reveal any hint that other main and/or interaction effects were present.

SEQ distortion associated with receipt of the pill: Table 13a

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Insert Table 13 about here
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shows the distribution of sway decreasers into SEQ accurates and SEQ inaccurates as a function of membership in the experimental design matrix subgroups relevant to the Placebo main effect. Of the 25 sway decreasers in the Pill subgroup, 15 (60%) were accurate on the SEQ, and

Table 13

Table 13a: Effect of the Placebo variable on the Subjective Effects Questionnaire accuracy of those whose body-sway decreased from pre-trial to post-trial

	Subjective Effects Questionnaire Ratings		
	Accurate	Inaccurate	
Pill	15 (E = 13.59)	10 (E = 11.41)	25
No Pill	10 (E = 11.41)	11 (E = 9.59)	21
	25	21	46

Chi-square = .702

p between .50 and .30

Table 13b: Effect of the Instructions variable on the Subjective Effects Questionnaire accuracy of those whose body-sway decreased from pre-trial to post-trial

	Subjective Effects Questionnaire Ratings		
	Accurate	Inaccurate	
Instructions	18 (E = 14.13)	8 (E = 11.87)	26
No Instructions	7 (E = 10.87)	13 (E = 9.13)	20
	25	21	46

Chi-square = 5.33

p less than .05

Table 13, continued

Table 13c: Effect of membership in subgroups relevant to the Placebo X Instructions interaction, on Subjective Effects Questionnaire accuracy of those whose body-sway decreased from pre-trial to post-trial

	Subjective Effects Questionnaire Ratings		
	Accurate	Inaccurate	
Pill and Instructions	10 (E = 7.61)	4 (E = 6.39)	14
Pill and No Instructions	5 (E = 5.98)	6 (E = 5.02)	11
No Pill and Instructions	8 (E = 6.52)	4 (E = 5.48)	12
No Pill and No Instructions	2 (E = 4.89)	7 (E = 4.11)	9
	25	21	46

Chi-square = 6.47

p between .10 and .05

10 (40%) were inaccurate. The 21 sway decreasers in the No Pill subgroup were divided into 10 (48%) accurate judges and 11 (52%) inaccurate judges. The apparently superior accuracy of the Pill subgroup is not statistically significant (chi-square on 1 d.f. = .702; p between .50 and .30). This nonsignificant difference is in the direction that would have been expected had it been due to differences in discriminability of the sway decreases with which those who decreased their sway in each of the two subgroups were dealing. The mean sway decrease for sway decreasers in the Pill subgroup was 7.34 units (1.68 mm.); that for sway decreasers in the No Pill subgroup was 3.77 units (.86 mm.). A t-test performed on the difference between these means attained a value of 2.75, on 44 d.f., significant beyond the .01 level. Hence it certainly cannot be denied that the Pill subgroup sway decreasers were judging larger and therefore presumably more discriminable sway decreases than were the No Pill subgroup sway decreasers. At the same time, however, it must be noted that sway decreasers in the Pill subgroup who rated themselves on the SEQ as increasing their sway, that is, who were inaccurate on the SEQ, were dealing with greater mean sway decreases (5.98 units, or 1.37 mm.) than were sway decreasers in the No Pill subgroup who rated themselves on the SEQ as decreasing their sway or as remaining the same -- that is, who were accurate on the SEQ (3.48 units, or .80 mm.).

In any event, one reasonable conclusion to be drawn is that there was a minuscule trend in the direction of greater accuracy on the SEQ for sway decreasers in the Pill subgroup as opposed to those in the No

Pill subgroup. Furthermore, sway decreasers in the Pill subgroup decreased their sway significantly more than did sway decreasers in the No Pill subgroup. And finally, the sway decreasers in the Pill subgroup who were inaccurate on the SEQ, were dealing with larger, and presumably more discriminable, sway decreases, than were the sway decreasers in the No Pill subgroup who were accurate on the SEQ.

SEQ distortion associated with the receipt of instructions: Table 13b shows the distribution of sway decreasers into accurate judges on the SEQ vs. inaccurate judges on the SEQ as a function of membership in the experimental design matrix subgroups relevant to the Instructions main effect. It can be seen that of the 26 sway decreasers in the Instructions subgroup, 18 (69%) fell into the accurate judge category, and 8 (31%) fell into the inaccurate judge category. Of the 20 sway decreasers in the No Instructions subgroup, only 7 (35%) were accurate on the SEQ, while 13 (65%) were inaccurate. The chi-square generated by Table 13b attained a value of 5.33, on 1 d.f., significant beyond the .05 level. Sway decreasers who had received instructions for increased body-sway were accurate on the SEQ significantly more frequently than sway decreasers who had not received instructions for increased body-sway, and were inaccurate on the SEQ significantly less frequently than sway decreasers who had not received instructions for increased body-sway.

The mean decrease for sway decreasers in the Instructions subgroup was 5.77 units (1.32 mm.); that for sway decreasers in the No

Instructions subgroup was 5.64 units (1.29 mm.). This fact, however, although in line with what might be expected if differences in discriminability of sway decreases accounted for the differences in accuracy observed, does not by itself make the discriminability difference hypothesis sound. In order for the discriminability difference hypothesis to be sound, it is necessary that no SEQ inaccurate cell in the matrix under consideration have a higher mean sway decrease than any SEQ accurate cell. As Table 14 shows, this condition is met by the

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Insert Table 14 about here
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mean sway decreases produced by sway decreasers dichotomized first into members of the Instructions subgroup and members of the No Instructions subgroup, and then further dichotomized into SEQ accurates and SEQ inaccurates.

SEQ distortion associated with membership in subgroups relevant to the Placebo X Instructions interaction: Table 13c sets forth the distribution of subjects into SEQ accurates vs. SEQ inaccurates as a function of membership in the experimental design matrix subgroups relevant to the Placebo X Instructions interaction. Examination of the table reveals the following: 1) of the 14 sway decreasers in the (Pill).(Instructions) subgroup, 10 (71%) were accurate on the SEQ and 4 (29%) were inaccurate; 2) of the 11 sway decreasers in the (Pill).(No Instructions) subgroup, 5 (45%) were accurate and 6 (55%) were inaccurate; 3) of the 12 sway decreasers in the (No Pill).(Instructions) subgroup, 8 (67%) were accurate and 4 (33%) were inaccurate; 4) of the 9 sway decreasers in the (No Pill).(No Instructions) subgroup, 2 (22%)

Table 14: Mean sway decreases for SEQ accurates and SEQ inaccurates in the Instructions and No Instructions subgroups

	Subjective Effects Questionnaire Ratings	
	Accurate	Inaccurate
Instructions	6.24 (1.43 mm.)	4.71 (1.08 mm.)
No Instructions	6.60 (1.51 mm.)	5.12 (1.17 mm.)

were accurate and 7 (78%) were inaccurate.

Because Walker and Lev (1953, p. 107) recommend that, should only approximate probabilities be required, the use of chi-square is appropriate even when expected frequencies fall as low as 2 in a cell, a chi-square was computed for Table 13c. The chi-square attained a value of 6.47, on 3 d.f., significant between the .10 and .05 levels. The major contributions to this chi-square came from the (Pill).(Instructions) row and the (No Pill).(No Instructions) row. A direct comparison of these two rows by means of the Fisher exact probability test resulted in a p-value of less than .05.

The role of differences in discriminability of sway decreases in producing this difference in accuracy is cancelled out by the circumstance that the sway decreasers in the (No Pill).(No Instructions) subgroup who were accurate on the SEQ were, on the average, dealing with lower sway decreases (5.17 units, or 1.19 mm.) than sway decreasers in the (Pill).(Instructions) subgroup who were inaccurate on the SEQ (5.45 units, or 1.25 mm.).

Frequencies of sway decreasing SEQ accurates and inaccurates in the (Pill).(Instructions) subgroup were compared with frequencies of sway decreasing SEQ accurates and inaccurates in the combined (Pill).(No Instructions) and (No Pill).(Instructions) subgroups. The slight advantage in accuracy observed for the (Pill).(Instructions) subgroup (71% of the subjects accurate for that subgroup vs. 57% of the subjects accurate for the 2 combined subgroups) was not significant statistically (chi-square on 1 d.f. = .825; p between .50 and .30).

Frequencies of sway decreasing SEQ accurates and inaccurates in the (No Pill).(No Instructions) subgroup were also compared with frequencies of sway decreasing SEQ accurates and inaccurates in the combined (Pill).(No Instructions) and (No Pill).(Instructions) subgroups. There was a tendency for sway decreasers in the (No Pill).(No Instructions) subgroup to be more inaccurate -- 78% of these sway decreasers were inaccurate -- than sway decreasers in the combined (Pill).(No Instructions) and (No Pill).(Instructions) subgroups, in which 43% of the sway decreasers were inaccurate. The differences between the frequencies accounting for the cited percentages were tested by means of Fisher's exact probability test, which generated a p-value lying between .10 and .05.

Differences in discriminability of sway decreases cannot account for this observed difference in accuracy, since sway decreasers in the (No Pill).(No Instructions) subgroup who were accurate on the SEQ were working with smaller sway decreases (5.17 units, or 1.19 mm.) than the sway decreasers in the combined subgroups who were inaccurate on the SEQ (5.39 units, or 1.25 mm.). Furthermore, within the combined subgroups, SEQ inaccurates were working with larger mean sway decreases (5.39 units, or 1.25 mm.) than were SEQ accurates (4.65 units, or 1.07 mm.).

In summary, results of post-hoc analyses of the distribution of sway decreasers into SEQ accurates and SEQ inaccurates as a function of membership in the experimental design matrix subgroups relevant to the Placebo and Instructions main effects and to the Placebo X Instructions interaction, were: 1) sway decreasers who had received a

pill manifested a very slight tendency to be more accurate on the SEQ than sway decreasers who had not received a pill; 2) sway decreasers who had received a pill decreased their sway significantly more than did sway decreasers who had not received a pill; 3) sway decreasers who had received instructions for increased body-sway were significantly more accurate on the SEQ than sway decreasers who had not received instructions for increased body-sway; 4) sway decreasers in the (Pill).(Instructions) subgroup were significantly more accurate on the SEQ than sway decreasers in the (No Pill).(No Instructions) subgroup; 5) sway decreasers in the (Pill).(Instructions) subgroup were more accurate on the SEQ than sway decreasers in the combined (Pill).(No Instructions) and (No Pill).(Instructions) subgroups, but only to a degree best accounted for by chance; 6) sway decreasers in the (No Pill).(No Instructions) subgroup showed a strong tendency, approaching statistical significance, to be more inaccurate on the SEQ than sway decreasers in the combined (Pill).(No Instructions) and (No Pill).(Instructions) subgroups.

Analysis of Hallucination Test data

Hallucination as a function of experimental conditions. An analysis of variance was performed on the Hallucination Test data, according to a 3-between design.

The Placebo main effect was statistically significant beyond the .05 level ($F(1,96) = 5.04$), and the Abstract Conditioning X Instructions interaction approached statistical significance ($F(3,96) = 2.49$; p be-

tween .10 and .05). The means relevant to the Placebo main effect are presented in Table 15a; those relevant to the Abstract Conditioning X Instructions interaction are presented in Table 15b. In

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Insert Table 15 about here
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addition, the means relevant to the Abstract Conditioning X Instructions interaction are plotted in Figures 3 and 4.

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Insert Figures 3 and 4 about here
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Table 15a shows that more subjects in the No Pill subgroup than in the Pill subgroup reported seeing the steady white light flicker after being told that that was what they would see. In actuality the light did not flicker. As reported above, the difference observed between the two groups is statistically significant.

Table 15b and Figure 3 show that although the mean Hallucination Test scores for the No Instructions subgroup subjects exposed to the Positive Abstract Conditioning procedure or to the Temporal Control procedure are higher than the means for the Instructions subgroup subjects exposed to those two Abstract Conditioning levels, the No Instructions subgroup subjects exposed to the Negative Abstract Conditioning procedure and the Instructions subgroup subjects exposed to the Negative Abstract Conditioning procedure had identical frequencies of reporting experiencing the hallucination, and the Instructions subgroup subjects exposed to the Sensory Control procedure reported experiencing the hallucination much more frequently than the No Instructions sub-

Table 15

Table 15a: Hallucination Test means of the Pill and No Pill subgroups

Pill	No Pill
0.232	0.429

Table 15b: Hallucination Test means of subgroups relevant to the Abstract Conditioning X Instructions interaction

	Pill	No Pill
Positive Abstract Conditioning	0.286	0.429
Negative Abstract Conditioning	0.357	0.357
Sensory Control	0.500	0.143
Temporal Control	0.143	0.429

Figure 3: Graphic representation of the Instructions X Abstract Conditioning interaction for Hallucination Test scores

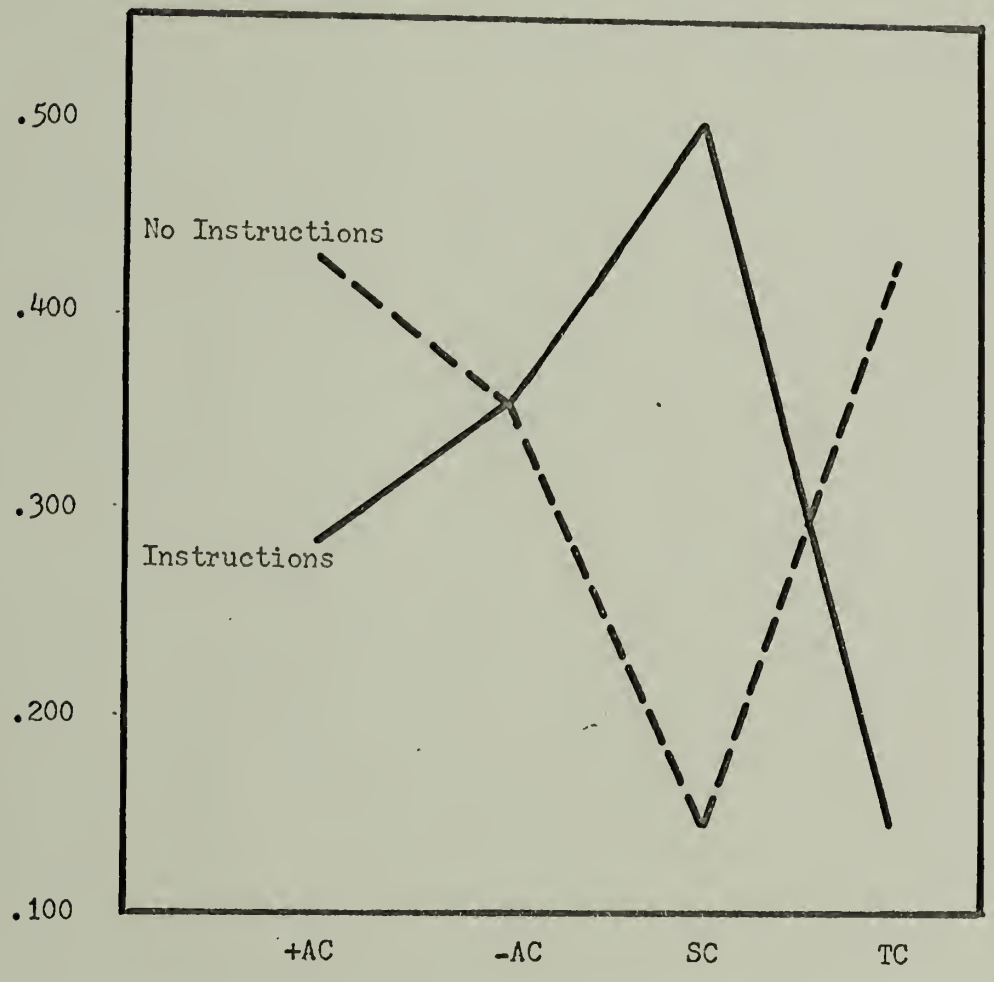
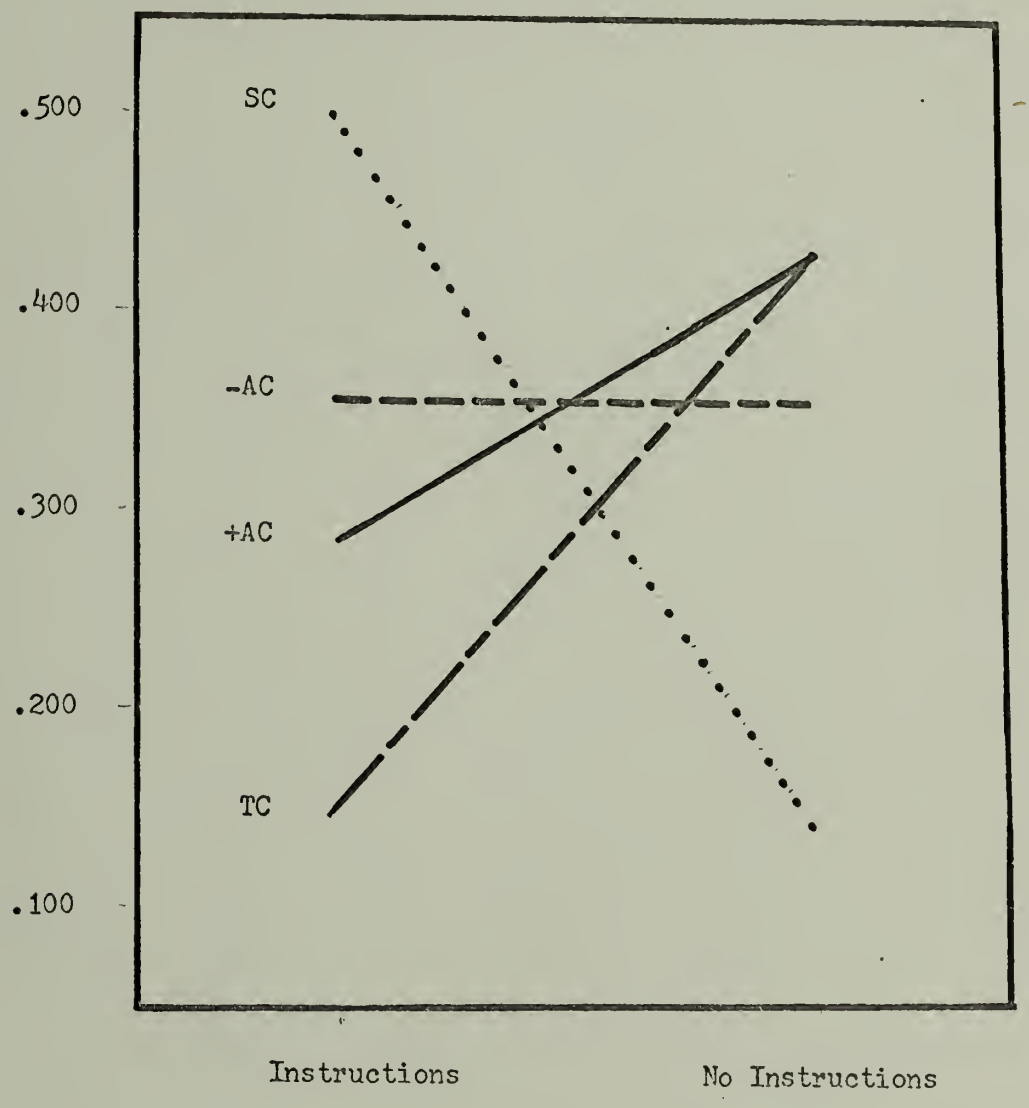


Figure 4: Graphic representation of the Abstract Conditioning X Instructions interaction for Hallucination Test scores



group subjects exposed to the Sensory Control procedure.

Tests were made of the simple effects of the Instructions variable at the Positive Abstract Conditioning level, the Sensory Control level, and the Temporal Control level of the Abstract Conditioning variable.

At the Positive Abstract Conditioning level, the Hallucination Test mean of the No Instructions subgroup is not significantly higher than that of the Instructions subgroup ($F(1,12) = 1.00$). At the Sensory Control level, the Hallucination Test mean of the Instructions subgroup is significantly higher than that of the No Instructions subgroup ($F(1,12) = 6.80$; p less than .025). At the Temporal Control level, the fact that the No Instructions subgroup mean is higher than that of the Instructions subgroup approaches statistical significance ($F(1,12) = 4.00$; p between .10 and .05).

Figure 4 shows the Abstract Conditioning X Instructions interaction from a different point of view. It can be seen in Figure 4 that for every level of the Abstract Conditioning variable except the level of Sensory Control, Instructions subgroup subjects reported experiencing the hallucination no more frequently than, and indeed in 2 cases less frequently than, No Instructions subgroup subjects; whereas at the Sensory Control level the Instructions subgroup subjects reported experiencing the hallucination significantly more than did the No Instructions subgroup subjects.

A test of the contrast between the Sensory Control level of the Abstract Conditioning variable and the other levels of that variable was highly significant statistically ($F(1,24) = 16.40$; p less than

.001).

This same contrast was tested individually for Instructions subjects and for No Instructions subjects, and it was found on the one hand that Sensory Control level subjects who received instructions for increased body-sway scored significantly higher on the Hallucination Test than subjects exposed to any other level of the Abstract Conditioning variable who received instructions for increased body-sway ($F(1,12) = 6.80$; p less than .025), and on the other, that Sensory Control level subjects who did not receive instructions for increased body-sway scored significantly lower on the Hallucination Test than subjects exposed to any other level of the Abstract Conditioning variable who did not receive instructions for increased body-sway ($F(1,12) = 9.81$; p less than .001).

Inspection of Figure 4 suggested further that subjects exposed to the Temporal Control level of the Abstract Conditioning variable and who received instructions for increased body-sway scored lower on the Hallucination Test than those exposed to the Positive Abstract Conditioning level, the Negative Abstract Conditioning level, or the Sensory Control level of the Abstract Conditioning variable who received instructions for increased body-sway. A test of the relevant contrast simple effect proved statistically significant ($F(1,12) = 5.77$; p less than .05).

These Hallucination Test results may be summarized as follows: 1) subjects who received the pill reported experiencing the hallucination significantly less frequently than did subjects who did not re-

ceive the pill; 2) as far as the influence of the Instructions variable went, subjects exposed to the Sensory Control level of the Abstract Conditioning variable responded differently from subjects exposed to the other three levels of the Abstract Conditioning variable, in that subjects exposed to the Sensory Control level tended to report the hallucination when they had received instructions for increased body-sway, and tended not to report it when they had received no such instructions, whereas subjects exposed to the other three levels of the Abstract Conditioning variable tended to report the hallucination when they had not received instructions for increased body-sway, and tended not to report it when they had received such instructions; 3) in detail, not only did Sensory Control subgroup subjects who had received instructions for increased body-sway report experiencing the hallucination significantly more frequently than did their compeers in the other three Abstract Conditioning subgroups, but also Sensory Control subgroup subjects who received no instructions for increased body-sway reported experiencing the hallucination significantly less frequently than did their compeers in the other three Abstract Conditioning subgroups; 4) when subjects in the subgroup relevant to the Temporal Control level of the Abstract Conditioning variable had received instructions for increased body-sway, they reported experiencing the hallucination significantly less frequently than subjects in the subgroups relevant to the other three levels of the Abstract Conditioning variable who had received instructions for increased body-sway.

Hallucination as a function of instructions and body-sway performance. The prediction was tested that subjects who found that what the experimenter told them about how they would perform in the body-sway situation was true, were those who would tend to report experiencing the visual hallucination during experimental Phase 5, and that those who found that what the experimenter told them about how they would perform in the body-sway situation was false, were those who would tend not to report experiencing the visual hallucination. To test this prediction, subjects exposed to instructions for increased body-sway were dichotomized into those who increased vs. those who decreased their sway, and each of these two categories was further subdivided into those who hallucinated vs. those who did not hallucinate. The same sort of classification was performed for subjects who, having received instructions for increased body-sway, reported on the Subjective Effects Questionnaire that they increased their sway, vs. those who, having received instructions for increased body-sway, reported on the Subjective Effects Questionnaire either that they remained the same or that they decreased their sway.

Of the 30 subjects exposed to instructions for increased body-sway who did in fact increase their sway, 10, or 33%, reported that they experienced the visual hallucination, and 20, or 67%, reported that they did not. Of the 26 subjects exposed to instructions for increased body-sway who decreased their sway, 8, or 30%, reported experiencing the visual hallucination, and 18, or 70%, reported not experiencing the hallucination. It was not felt to be worthwhile to

perform a chi-square on the 2 X 2 table containing these frequencies, since it is clear without resort to a statistical test that there is not the slightest tendency for the differences between the reported frequencies and the percentages based on them to be significant.

When the measure of body-sway increase used was a subject's belief that she had increased her sway or that she had not increased her sway, the outcome was essentially the same. Out of 25 subjects who received instructions for increased body-sway and who also reported on the SEQ that they had increased their sway, 8, or 32%, reported experiencing the visual hallucination, and 17, or 68%, reported not experiencing the visual hallucination. Out of 31 subjects who received instructions for increased body-sway and who reported on the SEQ that they had not increased their sway (i.e. that they had remained the same or decreased), 10, or 32%, reported experiencing the visual hallucination, and 21, or 68%, reported not experiencing the visual hallucination.

Hallucination as a function of Instructions, Abstract Conditioning, and body-sway performance. Finally, subjects who were exposed to both instructions for increased body-sway and to the Positive Abstract Conditioning procedure and who did in fact increase their sway, were compared to subjects who were exposed to both instructions for increased body-sway and to the Positive Abstract Conditioning procedure who in fact decreased their sway, with respect to reporting or failing to report experiencing the visual hallucination. Once again, both objective body-sway change and subjective body-sway change were considered.

The figures were the same whether the measure involved was actual body-sway change or subjective body-sway change. Of the 9 subjects exposed to the combination of experimental procedures under discussion who increased their sway, whether the increase was objective or subjective, 3, or 33%, reported experiencing the visual hallucination, and 6, or 67%, reported failing to experience it; while of the 5 subjects exposed to the combination of experimental procedures under discussion who did not increase their sway, whether the failure to increase was objective or subjective, 1, or 20%, reported experiencing the visual hallucination, and 4, or 80%, reported failing to experience the visual hallucination. A test of statistical significance was deemed unnecessary -- the differences in the frequencies and in the percentages based thereon are clearly nonsignificant.

Analysis of Drug Effects Questionnaire data

An analysis of variance performed upon the Drug Effects Questionnaire (DEQ) data in accordance with a 3-between design produced no statistically significant main or interaction effects, and none approaching statistical significance.

The Placebo X Instructions interaction produced an F-ratio of .0328, on 1 and 96 d.f., the reciprocal of which was 30.49, which, evaluated on the same d.f., is significant beyond the .001 level. In other words, the F-ratio for this source of variance was significantly nonsignificant.

The mean DEQ score for the entire subject sample was 11.11.

Analysis of Semantic Differential data

Subjects had rated 8 concepts on the Semantic Differential. The Semantic Differential ratings of the 2 concepts whose real-world referents were, a priori, most directly involved in the experimental situation, were subjected to separate analyses of variance, in conformity to a design wherein the experimental variables Placebo, Abstract Conditioning, and Instructions, completely crossing, were between subjects variables, while Semantic Differential Factors and Scales Nested within Semantic Differential Factors were within subjects variables.

The 2 concepts chosen for these analyses were "Pill" and "Experimenter in this Experiment."

The concept "Pill." The analysis of variance performed on the Semantic Differential ratings of the concept "Pill" produced 4 statistically significant F-values. The sources of variance responsible for the significant F-values were the Placebo main effect ($F(1,96) = 3.82$; $p < .05$), the Semantic Differential Factors main effect ($F(2, 192) = 150.60$; $p < .001$), the Scales Nested within Semantic Differential Factors main effect ($F(6,576) = 76.34$; $p < .001$), and the Instructions X Semantic Differential Scales interaction effect ($F(2,192) = 2.27$; $p < .05$).

The Semantic Differential Factors main effect and the Scales Nested within Semantic Differential Factors main effect are of trivial interest, reflecting respectively the fact that the concept "Pill" received significantly different ratings on the Semantic Differential

Factors of Potency, Activity, and Evaluation, and the fact that within each of those Semantic Differential Factors, the concept "Pill" was rated significantly differently on the three bipolar scales of which the Factor was comprised.

Tables 16a and 16b present, respectively, the means relevant to

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Insert Table 16 about here
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the Placebo main effect, and the means relevant to the Instructions X Semantic Differential Scales interaction effect.

Table 16a shows that when ratings on the three Semantic Differential Factors of Potency, Activity, and Evaluation were summed and averaged, the No Pill subgroup rated the concept "Pill" higher than did the Pill subgroup. In fact, as Table 16c makes clear, the No Pill subgroup rated the concept "Pill" higher on each of the three Semantic Differential Factors than did the Pill subgroup; that is to say, the No Pill subgroup rated the concept "Pill" as being more potent, more active, and more good, than did the Pill subgroup, although when differences between the No Pill subgroup and Pill subgroup means were tested for each of the Semantic Differential Factors individually, only the difference at the level of the Potency Factor was found to be statistically significant ($F(1,48) = 4.16$; p less than .05). The difference between the No Pill and Pill subgroup means approached statistical significance at the level of the Activity Factor ($F(1,48) = 2.05$; p between .20 and .10), but was clearly nonsignificant at the level of the Evaluation Factor ($F(1,48) = .35$).

Table 16

Table 16a: Means of the Pill and No Pill subgroups for the summed and averaged Semantic Differential Factor ratings of the concept "Pill"

Pill	No Pill
4.18	4.49

Table 16b: Means relevant to the Instructions X Semantic Differential Factors interaction for the Semantic Differential ratings of the concept "Pill"

	Potency	Activity	Evaluation
Instructions	3.39	4.43	5.11
No Instructions	3.11	4.38	5.31

Table 16c: Pill and No Pill subgroup mean Semantic Differential ratings of the concept "Pill" on each of the three Semantic Differential Factors

	Potency	Activity	Evaluation
Pill	3.10	4.27	5.16
No Pill	3.41	4.53	5.26

Table 16b presents the means relevant to the Instructions X Semantic Differential Factors interaction effect. Figure 5 makes clear the nature of this interaction. It can be seen that whereas

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Insert Figure 5 about here
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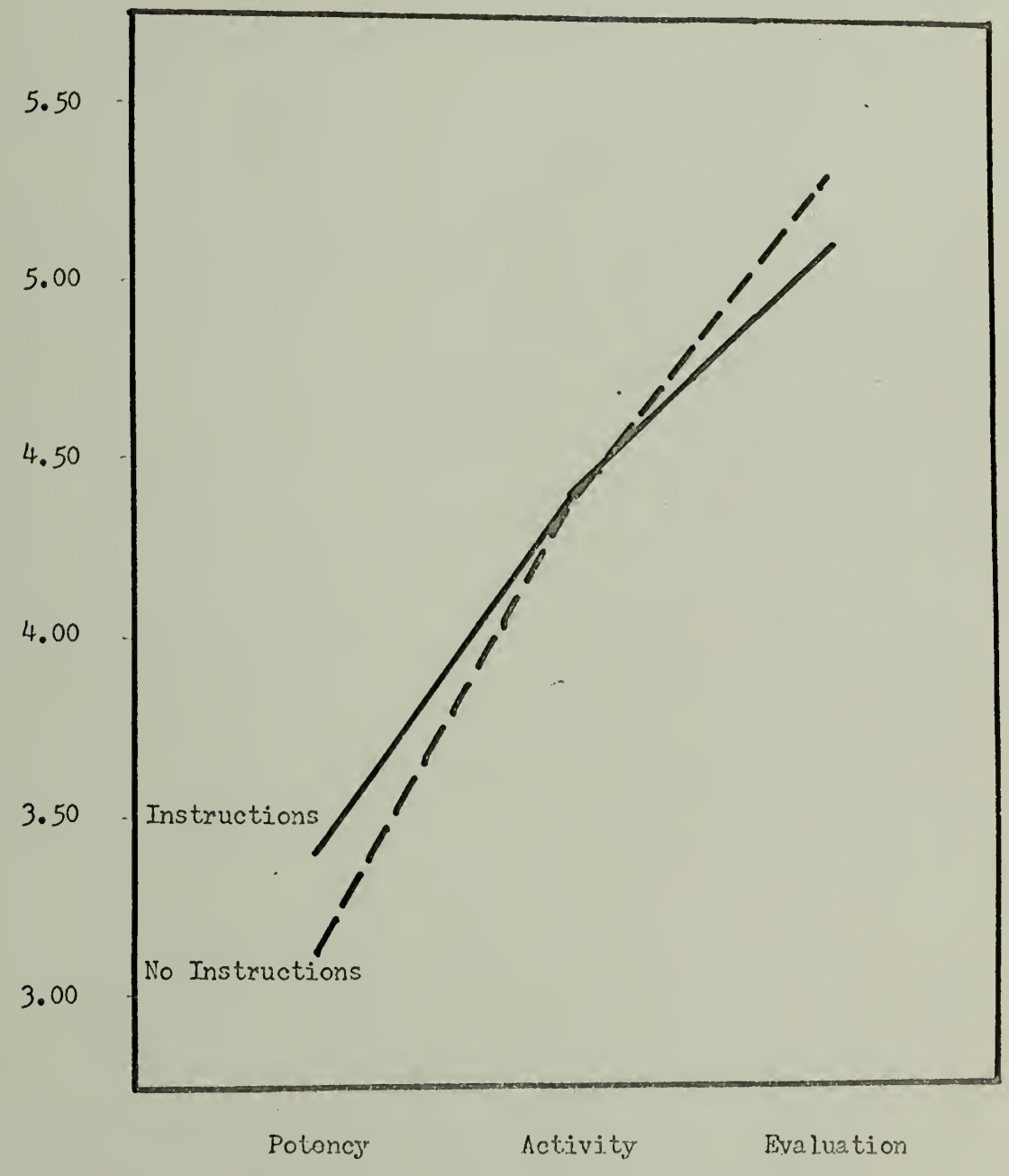
the No Instructions subgroup rated the concept "Pill" lower on the Potency Factor than did the Instructions subgroup, the No Instructions subgroup rated the concept "Pill" higher on the Evaluation Factor than did the Instructions subgroup. Both differences approached statistical significance -- there was a noteworthy tendency for the Instructions subgroup subjects to rate the concept "Pill" higher on the Potency Factor than did the No Instructions subgroup subjects ($F(1,48) = 2.64$; p between .20 and .10), and likewise for the Instructions subgroup subjects to rate the concept "Pill" lower on the Evaluation Factor than did the No Instructions subgroup subjects ($F(1,48) = 1.70$; p between .20 and .10).

The results of these analyses of the Semantic Differential ratings of the concept "Pill" may be summarized as follows: 1) summing across Semantic Differential Factors, the average ratings of the "Pill" concept produced by subjects who did not receive a pill were significantly higher than the average ratings of the "Pill" concept produced by subjects who did receive a pill; 2) when ratings were considered Factor by Factor, subjects who did not get a pill rated the concept "Pill" significantly higher on the Potency Factor than did subjects who did get a pill, and showed a tendency approaching statistical signifi-

Figure 5: Graphic representation of the Instructions X Semantic Differential Factors interaction for the Semantic Differential ratings of the concept "Pill"



cance to rate the concept "Pill" higher on the Activity Factor than subjects who did get a pill, but not higher on the Evaluation Factor, although even on the Evaluation Factor the No Pill subgroup subjects' ratings were higher than those of the Pill subgroup subjects; 3) there was a significant reversal of the relative positions of the mean "Pill" ratings of the Instructions and No Instructions subgroups with respect to the Semantic Differential Factors of Potency and Evaluation, in that subjects who did not receive instructions for increased body-sway rated the concept "Pill" lower on the Potency Factor, to a degree approaching statistical significance, than did subjects who received instructions for increased body-sway, and higher on the Evaluation Factor, to a degree approaching statistical significance, than did subjects who received instructions for increased body-sway.

The concept "Experimenter in this Experiment." Only two sources of variance were associated with statistically significant F-ratios: the main effect for Semantic Differential Factors ($F(2,192) = 138.26$; $p < .001$), and the main effect for Scales Nested within Semantic Differential Factors ($F(6,576) = 17.35$; $p < .001$).

In other words, the concept "Experimenter in this Experiment" was rated significantly differently on the three Semantic Differential Factors of Potency, Activity, and Evaluation, and was also rated significantly differently on the bipolar scales within each Factor, of which that Factor was comprised.

No other main effects or interactions were statistically signifi-

cant or approached statistical significance.

It should be noted that one interaction, the Placebo X Instructions interaction, was significantly nonsignificant, generating an F-ratio, on 1 and 96 d.f., of .00096. The reciprocal of this figure, 104.17, evaluated on the same d.f., is significant beyond the .001 level.

Analysis of Awareness Questionnaire data

Awareness as a function of experimental conditions. Subjects were required to complete a relatively open-ended Awareness Questionnaire, which can be seen in Appendix E.

After going over subject protocols to get an idea of the kinds of assertions that subjects had made, the experimenter constructed a checklist consisting of 16 statements bearing on the purposes of the experiment. Each statement was assigned a weight according to the experimenter's judgement of the statement's closeness to reflecting the purposes of the experiment. The higher the weight, the more reflective of experimental purposes the statement was considered to be. This checklist can be seen in Appendix F. The Awareness Questionnaire protocol of each subject was examined for statements expressing the same meaning as those on the checklist; if some statement by a subject was similar in meaning to one on the checklist, the subject received credit for the relevant checklist statement. For each subject, the weights of the checklist statements for which the subject received credit were summed; the sum of the weighted checklist statements was then designated as the Awareness Questionnaire score.

Awareness Questionnaire scores were subjected to analysis of variance, according to a 3-between design.

The matrix mean score on the Awareness Questionnaire was 6.58. Three sources of variance were found to be associated with statistically significant F-ratios, the main effect for Placebo ($F(1,96) = 8.40$; $p < .01$), the main effect for Instructions ($F(1,96) = 9.96$; $p < .005$), and the Placebo X Instructions interaction ($F(1,96) = 4.35$; $p < .05$).

The means relevant to these significant sources of variance are set forth in Table 17.

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Insert Table 17 about here
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Table 17a shows that what accounted for the significant Placebo main effect was the fact that the Awareness Questionnaire mean of subjects who received a pill was higher than that of subjects who did not receive a pill.

Table 17b shows that the significant Instructions main effect was due to the fact that the Awareness Questionnaire mean of subjects who received instructions for increased body-sway was higher than that of subjects who did not receive instructions for increased body-sway.

Examination of Table 17c reveals that the significant Instructions X Placebo interaction was due to the fact that the difference between the Awareness Questionnaire means produced by subjects assigned to the Instructions subgroup, as opposed to those produced by

Table 17

Table 17a: Awareness Questionnaire means of the Pill and No Pill subgroups

Pill	No Pill
7.98	5.18

Table 17b: Awareness Questionnaire means of the Instructions and No Instructions subgroups

Instructions	No Instructions
8.11	5.05

Table 17c: Awareness Questionnaire means of subgroups relevant to the Placebo X Instructions interaction

	Instructions	No Instructions
Pill	8.50	7.46
No Pill	7.71	2.64

subjects assigned to the No Instructions subgroup, changed depending on whether or not the subjects had also been exposed to the pill.

Alternatively, it may be said that the difference between the Awareness Questionnaire means produced by subjects assigned to the Pill subgroup, as opposed to those produced by subjects assigned to the No Pill subgroup, changed depending on whether or not the subjects had also been exposed to instructions for increased body-sway.

Comparison of the means relevant to the Instructions X Placebo interaction were made to clarify its nature. A test of simple effects performed on the difference between the Awareness Questionnaire means of the (Pill).(Instructions) and (Pill).(No Instructions) subgroups was statistically nonsignificant ($F(1,24) = .58$). A test of simple effects performed on the difference between the Awareness Questionnaire means of the (No Pill).(Instructions) and (No Pill).(No Instructions) subgroups was highly significant statistically ($F(1,24 = 19.00$; p less than .001). Analogously, a test of simple effects performed on the difference between the Awareness Questionnaire means of the (Pill).(Instructions) and the (No Pill).(Instructions) means was statistically nonsignificant ($F(1,24) = .28$), while a test of simple effects performed on the difference between the Awareness Questionnaire means of the (Pill).(No Instructions) and (No Pill).(No Instructions) subgroups was highly significantly ($F(1,24) = 11.34$; p less than .005).

In other words, subjects who received a pill and who also received instructions for increased body-sway, were not significantly

more aware of experimental purposes than subjects who received a pill but who did not receive instructions for increased body-sway. Similarly, subjects who did not receive a pill but who received instructions for increased body-sway were essentially as aware of experimental purposes as those who received both instructions and the pill. All of these subgroups were significantly more aware of experimental purposes than subjects who received neither pill nor instructions for increased body-sway.

What this all reduces to is that receiving the pill and/or receiving instructions for increased body-sway were associated with subjects' indicating on the Awareness Questionnaire a significantly greater knowledge of experimental purposes than was indicated thereon by subjects who received neither instructions for increased body-sway nor the pill.

Because the Placebo and Instructions main effects were on the same degrees of freedom and had the same error term, direct comparison of the F-ratios associated with each source of variance gives an idea of the relative strengths of the contributions of those two variables to the results. In addition, however, point estimates were made for the variables. The relevant statistic, theta-hat, was found to be 3.46 for the Placebo effect, and 4.19 for the Instructions effect, indicating that the Instructions effect was stronger than the Placebo effect.

These results may be summarized as follows: 1) subjects who received the pill were significantly more aware of experimental purposes

than subjects who did not; 2) subjects who received instructions for increased body-sway were significantly more aware of experimental purposes than subjects who did not; 3) the difference in awareness of experimental purposes between subjects exposed to instructions for increased body-sway and those not so exposed changed significantly depending on whether the subjects had or had not also received the pill -- more concretely, subjects exposed to instructions for increased body-sway and also exposed to the pill were not significantly more aware of experimental purposes than subjects not exposed to instructions for increased body-sway but exposed to the pill, while subjects exposed to instructions for increased body-sway but not exposed to the pill were significantly more aware of experimental purposes than subjects not exposed to instructions for increased body-sway and also not exposed to the pill; 4) the effect on awareness of experimental purposes of exposure to instructions for increased body-sway was greater than the effect thereon of exposure to the pill.

Objective and subjective body-sway performance as a function of awareness. To determine whether there was a relationship between being aware of experimental purposes and body-sway performance, two sets of chi-square analyses were carried out. One set of analyses used objective body-sway change as the dependent measure, the other set used subjective body-sway change as the dependent measure.

The method of analysis involved the simultaneous dichotomization of subjects into those above vs. those below the matrix mean of 6.58 on the Awareness Questionnaire (i.e. aware subjects vs. unaware sub-

jects), and into those who increased vs. those who did not increase their body-sway from pre-trial to post-trial, both when the measure of sway change was objective and when it was subjective.

In addition, changes in frequencies of subjects falling into the cells generated by the above dichotomizations, as a function of membership in the experimental subgroups relevant to the Placebo variable, the Instructions variable, and the interaction of the Placebo and Instructions variables, were investigated.

Table 18 presents the results of this analysis for objective body-sway change data. Table 19 presents the results of this analysis for subjective body-sway change data.

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Insert Tables 18 and 19 about here
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It can be seen from Table 18a that there was an overall tendency for subjects who were aware of experimental purposes to decrease rather than to increase their objective body-sway from pre-trial to post-trial, and for subjects who were not aware of experimental purposes to increase rather than to decrease their body-sway from pre-trial to post-trial. The chi-square associated with Table 18a can be seen to be 2.87, on 1 d.f., significant between the .10 and .05 levels.

Table 18b shows that there was a tendency, weakly approaching statistical significance, for subjects who were exposed to instructions for increased body-sway and who were aware of experimental purposes to decrease rather than to increase their sway, and for those exposed to instructions for increased body-sway and who were not aware of experimental purposes to increase rather than to decrease

Table 18

Table 18a: Overall relationship between awareness of experimental purposes and direction of objective body-sway change

	Increased Objective Sway	Decreased Objective Sway	
Aware	28 (E = 32.41)	27 (E = 22.59)	55
Unaware	38 (E = 33.59)	19 (E = 23.41)	57
	66	46	112

Chi-square = 2.87

p between .10 and .05

Table 18b: Relationship between awareness of experimental purposes and direction of objective body-sway change for subjects who received instructions for increased body-sway

	Increased Objective Sway	Decreased Objective Sway	
Aware	18 (E = 20.36)	20 (E = 17.64)	38
Unaware	12 (E = 9.64)	6 (E = 8.36)	18
	30	26	56

Chi-square = 1.83

p between .20 and .10

Table 19

Table 19a: Overall relationship between awareness of experimental purposes and direction of reported body-sway change

	Reported Sway Increase	Did Not Report Sway Increase	
Aware	27 (E = 32.41)	28 (E = 22.59)	55
Unaware	39 (E = 33.59)	18 (E = 23.41)	57
	66	46	112

Chi-square = 4.32

p less than .05

Table 19b: Differences between direction of reported body-sway change for Instructions and No Instructions subgroup subjects who were aware of experimental purposes

	Reported Sway Increase	Did Not Report Sway Increase	
Instructions	15 (E = 19.00)	23 (E = 19.00)	38
No Instructions	12 (E = 8.00)	4 (E = 8.00)	16
	27	27	54

Chi-square = 5.68

p less than .02

their sway. The chi-square associated with Table 18b may be seen to be 1.83, on 1 d.f., significant between the .20 and .10 levels.

Analysis of the overall table into the subgroups relevant to the Placebo main effect and to the Placebo X Instructions interaction effect failed to reveal any differences that were significant or that approached significance.

Table 19a makes it plain that overall there was a statistically significant tendency for subjects who were aware of experimental purposes to report on the Subjective Effects Questionnaire that they had not increased their body-sway rather than that they had increased it, and for subjects who were not aware of experimental purposes to report on the Subjective Effects Questionnaire that they had increased their body-sway rather than that they had decreased it. The chi-square generated by this table may be seen to be 4.32, on 1 d.f., significant beyond the .05 level.

Table 19b shows that there was a statistically significant tendency for subjects who received instructions for increased body-sway and who were aware of experimental purposes to report on the Subjective Effects Questionnaire that they did not increase their sway rather than that they increased it, and for subjects who did not receive instructions for increased body-sway but who were nonetheless aware of experimental purposes to report on the Subjective Effects Questionnaire that they increased their sway rather than that they failed to increase it. The chi-square associated with this table may be seen to be 5.68, on 1 d.f., significant beyond the .02 level.

In summary, when either objective or subjective body-sway change is considered, there was a tendency for subjects who were aware of experimental purposes to fail to increase rather than to increase their body-sway from pre-trial to post-trial, and for subjects who were unaware of experimental purposes to increase rather than to fail to increase their body-sway from pre-trial to post-trial. This overall tendency was stronger for subjective body-sway change than for objective body-sway change.

Furthermore, there was a weak tendency for subjects who received instructions for increased body-sway and who were aware of experimental purposes to fail to increase rather than to increase their objective body-sway from pre-trial to post-trial, as opposed to those who received instructions for increased body-sway but were unaware of experimental purposes, who increased rather than failed to increase their objective body-sway from pre-trial to post-trial.

Finally, there was a pronounced tendency for subjects who received instructions for increased body-sway and who were aware of experimental purposes to report the subjective experience of failing to increase rather than of increasing their body-sway, as opposed to those who did not receive instructions for increased body-sway but were also aware of experimental purposes -- these latter reported the subjective experience of increasing rather than of failing to increase their body-sway.

PART 4

DISCUSSION

Near failure of the random assignment procedure

The near-failure of the procedure whereby subjects were randomly assigned to experimental subgroups, escapes comprehension. Taller, heavier subjects tended to cluster in certain subgroups of the experimental design.

Review of the random assignment procedure does not suggest any reasons for the failure. In fact, as assignment of subjects to experimental subgroups was actually carried out, events occurred that should have further mitigated against the systematic assignment of subjects having certain, presumably normally-distributed characteristics of height and weight, to certain of the experimental subgroups. For example, it was frequently the case that a subject telephoned to say that she could not be present at the time she had originally signed up for and to request a new time-slot, or that subjects among themselves swapped time-slots, so that by no conceivable mechanism could the experimenter have known beforehand just what the height and weight characteristics would be of the subject who, by virtue of showing up in thus and such an ordinal position in the subject sequence, was assigned to thus and such an experimental subgroup.

It is interesting to notice, however, that the subgroups with reference to which there was the most clear violation of random assignment assumptions were those relevant to the Instructions main ef-

effect and to the Abstract Conditioning X Instructions interaction effect, both of which the experimenter hoped would show statistical significance. Furthermore, the experimenter was well aware that taller, heavier subjects were likely to sway more than those not so tall and/or heavy. If it be argued that somehow the experimenter was stacking the deck in favor of his hypotheses, it should be pointed out that the hypothesis concerning the effect of Instructions called for those who received instructions for increased body-sway to increase their sway more than those who did not, but in fact the failure of the random assignment procedure resulted in taller, heavier subjects, who would perhaps not only sway more but also increase their sway more, clustering in the No Instructions subgroup.

On the other hand, however, at the Instructions level of the Abstract Conditioning X Instructions interaction, the hypothesis was that the subgroup that would increase its sway the most would be the Positive Abstract Conditioning subgroup, the subgroup that would increase its sway the least would be the Negative Abstract Conditioning subgroup, and the two control subgroups would increase their sway an intermediate amount. In fact, the tallest, heaviest girls were in the subgroup that was expected to increase its sway the most, the least tall, least heavy girls were in the subgroup that was expected to increase its sway the least, and the girls of intermediate tallness and heaviness fell into the two control subgroups.

In all fairness it should be pointed out that, although in every case except that of the Negative Abstract Conditioning procedure the

experimenter hoped to find that those who received instructions for increased body-sway would increase their sway more than those who did not, at every level of the Abstract Conditioning variable but one (the Temporal Control level), the taller, heavier girls were in the No Instructions subgroup. But once again, the difference was most pronounced for the Negative Abstract Conditioning subgroup, the one subgroup in which subjects exposed to instructions for increased body-sway were supposed to increase their sway less than subjects not exposed to instructions for increased body-sway.

If these outcomes were more than coincidental, the means whereby they were produced is mysterious.

Placebo and Instructions effects

Lack of expected effects in the originally proposed analysis. In the introduction to this dissertation, several predictions were made concerning experimental outcomes likely to be observed should the thesis presented there in fact have been true. The results of the originally proposed statistical treatment of the data did not confirm any of the predictions.

Although the hypothesis about the effects of the Abstract Conditioning variable and its interactions with the Placebo and Instructions variables were the principal concern of the study, this discussion will begin with consideration of the hypotheses about the effects of the Instructions and Placebo variables alone and in interaction with each other.

The reason for beginning off-center, as it were, is that the Abstract Conditioning hypotheses were relatively untested in the literature, which is to say that data bearing on those hypotheses were scarce and that Abstract Conditioning was therefore the variable the effects of which were most uncertain. Thus the failure of the present study to confirm the Abstract Conditioning hypotheses cannot be said to be contradictory to well-established findings.

The case is somewhat different with respect to hypotheses concerning the Instructions and Placebo variables.

In the present study, giving instructions for increased body-sway was the same thing as giving "suggestions." Giving instructions, as herein understood, and giving suggestions, both consist primarily of communicating to a person an assertion that some kind of behavior, either nonverbal and/or in the form of protocol statements, will be forthcoming from the person. The effect of such a communication is generally said to consist of a strong tendency for the person given the communication to produce the behaviors designated. This effect has been noted in a variety of circumstances, from those in which instructions have been called "persuasive communications," as in the work reviewed by Cohen (1964) and by Hovland, Janis, and Kelley (1953), to those in which instructions have been called "hypnotic suggestions," as in the work of Hilgard (1965), Hull (1933), Weitzenhoffer (1963), Wells (1924), and countless others, or simply "suggestions," as in the work of Stukat (1958), or "task-motivating instructions," as in the work of T.X. Barber and his colleagues (1969).

As was shown by the literature cited in the introduction, the effect of giving a person a pharmacologically inert medication is also well documented, especially when the person given the medication is told specifically what effect it is going to have on him, as in Lyerly et al. (1964) and Brodeur (1965), but also when the person is not so informed, as in Baker and Thorpe (1957).

Therefore it is indeed a matter of some curiosity when a study such as the present one reveals no significant effect of either Instructions or Placebo.

First we will consider the Placebo variable.

It was hypothesized that subjects who received a pill would show greater differences between their pre-trial and post-trial body-sway than subjects who did not receive a pill. The direction of sway change under the influence of the pill was not predicted, and was considered actually unpredictable in view of the facts that 1) Pill subgroup subjects were not told that it was their body-sway upon which the pill was supposed to act, and 2) Pill subgroup subjects were not told in which direction their body-sway was to change.

Since the direction of the difference between pre- and post-trial body-sway of Pill and No Pill subgroup subjects was not predicted, the suitable analysis was one performed on absolute sway changes, regardless of direction.

Such an analysis showed that subjects who received the pill did change their body-sway more from pre-trial to post-trial than subjects who had not received the pill (8.60 units, or 1.97 mm., as op-

posed to 6.68 units, or 1.53 mm.). The difference between these average absolute sway changes showed an attenuated tendency to approach statistical significance ($F(1,96) = 1.65$; p between .30 and .20).

This outcome is in line with the prediction. It is what would be expected under the suppositions that 1) in the main, subjects in the experiment had learned pre-experimentally that pills were effective, that 2) subjects who got the pill did figure out that the pill was supposed to affect their body-sway, and that 3) subjects who got the pill were not able to figure out the direction of effect the pill was supposed to have.

Under such circumstances, some subjects who got the pill might have decided that the pill was supposed to make them steadier on their feet, and some might have decided that the pill was supposed to make them unsteadier on their feet. Some, then, if expectation has any force at all, would have decreased their body-sway from pre-trial to post-trial, and some would have increased it, and since both these decreases and these increases would have been greater for subjects who got the pill than for those who did not, subjects who got the pill would show a greater absolute sway change from pre-trial to post-trial than subjects who did not.

However, although supposition #2 above is supported by the Awareness Questionnaire data, supposition #3 is not. The Awareness Questionnaire data made it clear that subjects who got the pill were as aware of experimental purposes as subjects who were given explicit communi-

cation that their body-sway would increase from pre-trial to post-trial. Consequently, it is not unreasonable to presume that subjects who got the pill knew not only that it was their body-sway that the pill was supposed to affect, but also believed that their body-sway was expected to increase.

Furthermore, taking the Awareness Questionnaire data into consideration, supposition #1 above is not supported by an analysis of variance of body-sway scores wherein subjects were blocked into those who scored high vs. those who scored low on the Drug Effects Questionnaire (see section 4:2:2:2, below). If subjects who got the pill knew that their body-sway was supposed to increase, and if subjects who scored high on the Drug Effects Questionnaire were those who had learned to a greater extent than those who scored low on that questionnaire that medications were effective, then subjects who scored high on the Drug Effects Questionnaire and who got the pill should have increased their body-sway more than those who scored high on the Drug Effects Questionnaire who did not get the pill. Such was not the case. Not only did the analysis of variance under consideration produce an insignificant F-ratio for the Placebo X Drug Effectiveness X Trials interaction, but also subjects who scored high on the Drug Effects Questionnaire and who got a pill did not increase their body-sway as much from pre-trial to post-trial as subjects who scored high on the Drug Effects Questionnaire but did not get a pill. Nor did nonparametric analyses of the body-sway performance of those who scored above as opposed to those who scored below the matrix mean of 11.11 on the Drug Effects Question-

naire, produce any compelling evidence that those who had learned -- as measured by the Drug Effects Questionnaire -- that medication was effective, were more prone to increase their body-sway from pre- to post-trial when in receipt of a pill, than those, also in receipt of a pill, who had learned that medication was relatively ineffective (see Table 22a). At the same time, however, Table 22a does show a slight trend for subjects scoring high on the DEQ and receiving a pill to increase rather than to decrease their body-sway from pre- to post-trial, as compared to subjects scoring low on the DEQ but also receiving a pill.

If it is true that subjects who got the pill were as aware of experimental purposes as those who received instructions for increased body-sway -- an assertion supported by the Awareness Questionnaire data --, and that subjects who got the pill therefore knew that their body-sway was supposed to increase, it is perhaps interesting to point out that in the original analysis of body-sway data not only was the difference in body-sway increase from pre- to post-trial between the Pill and No Pill subgroups utterly free from the faintest hint of statistical significance, but also it turned out to be the subjects who did not receive the pill who increased their body-sway more from pre- to post-trial than the subjects who received the pill (3.85 units of increase, or .880 mm., as opposed to 2.05 units of increase, or .470 mm.). In view of the lack of statistical significance or any approach thereto associated with this difference, lingering upon this outcome is unwarranted. Nevertheless it should be said, since it is reasonable to designate the No Pill subgroup as the control group with respect to investigation of the Placebo effect, that whatever effect

the pill had was apparently to inhibit increase of body-sway from pre- to post-trial. This inhibition cannot be ascribed to ignorance on the part of the subjects who received the pill that the pill was supposed to cause them to increase their sway, because subjects who received the pill may have been as aware that their sway was supposed to increase as subjects told specifically that their sway was supposed to increase.

The Instructions effect on body-sway was also not to be found in the originally proposed treatment of the data. What was expected was that subjects who were told that they would find that their body-sway increased from pre-trial to post-trial, would in fact increase their body-sway from pre-trial to post-trial significantly more than those who were not so instructed.

What actually happened was that the source of variance for the Instructions X Trials interaction (the Instructions main effect in the revised analysis), which, statistically significant and with the relevant means correctly ordered, would have reflected the expected outcome, was significantly nonsignificant, although the relevant means were indeed correctly ordered. That is to say, Instructions subgroup subjects produced a larger mean increase from pre-trial to post-trial (3.30 units, or .757 mm.) than did No Instructions subgroup subjects (2.60 units, or .619 mm.), but the difference must be laid at the door of random variation.

No predictions were made in the introduction to this dissertation about the existence or nature of a Placebo X Instructions interaction. The reason for this omission was that, of the placebo studies reviewed

preparatory to the execution of the present experiment, there was none set up to provide information on such an interaction, and hence there was no empirical basis for predicting what kind of interaction of this sort, if any, would be forthcoming.

To demonstrate a Placebo X Instructions interaction and its nature, the experimental design of a study must include all the cells shown in Table 20, where "No Placebo" means no administration of any

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Insert Table 20 about here
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substance whatsoever that might pass as a drug or as a drug-carrying agent.

In all the studies reviewed, one or more of these cells, in most cases an entire column or row, is missing or spoiled.

For example, in the Baker and Thorpe (1957) comparison of the effect of the active drug mepazine with the effect of inert medication in the control of incontinence in deteriorated psychotics, the "No Placebo" column means "Active Drug," and the "Instructions" row is missing. Brodeur's (1965) study of the effects of stimulant and tranquilizer placebos on healthy subjects is missing the "No Placebo" condition. The Gliedman et al. study (1958) replaces the "No Placebo" condition with "Psychotherapy," a replacement that does not conform to the design depicted in Table 20 insofar as there is evidence that "Psychotherapy" and "Placebo" are equivalent -- one of the points of the Gliedman et al. study --; furthermore, although it is difficult to know for certain from the written report whether or not the placebo

Table 20: Subgroups essential in an experimental design if a Placebo X Instructions interaction is to be demonstrated

	Placebo	No Placebo
Instructions		
No Instructions		

subjects in this study were told what effects the "drug" they were being given would have on them, it seems that the "No Instructions" condition was missing. The Lyerly et al. study (1964) lacks the "No Placebo" condition, since those of their subjects who were not given either amphetamine sulphate or chloral hydrate were given a placebo in the form of either a capsule or of something that might pass as a drug-carrying agent, namely, a glass of orange juice; in addition, while those placebo subjects who got orange juice got no information, or instructions, concerning what to expect as drug effects, there was no matching orange juice placebo subgroup that was given information, or instructions, concerning what to expect as drug effects. As a final example, the Goldman et al. study (1965) is missing the "No Instructions" condition.

Nevertheless, the dearth of data from appropriately designed experiments bearing on the nature of the interaction between a Placebo variable and an Instructions variable need not deter one completely from speculating as to what would have been a sensible outcome for such an interaction in the present experiment.

At the very least, it makes intuitive sense to have predicted that those subjects who both received the pill and were told that the pill would cause them to be more unsteady on their feet during the second trial on the sway platform, would increase their body-sway significantly more from pre- to post-trial, than those who neither received the pill nor were told that they would find themselves more unsteady on their feet during the second trial on the sway platform. In the

present experiment, the actual outcome with respect to the Placebo X Instructions interaction was that the source of variance for that interaction was hugely nonsignificant, -- the reciprocal of the F-ratio associated with it was 83333.33. Furthermore, members of the (Pill).(Instructions) subgroup increased their sway less than members of the (No Placebo).(No Instructions) subgroup, although minimal weight should be given this finding since, by standard statistical convention, the difference is clearly attributable to random variation.

With respect, then, to the Placebo effect, the Instructions effect, and the effect of the Placebo X Instructions interaction, what was found was F-ratios that, from Placebo effect to Instructions effect to interaction effect, were increasingly less than 1.00. Myers (1966) states that a frequent reason for the existence of F's less than 1.00 is the presence of some systematic effect not accounted for by the experimental design.

The search for hidden systematic bias. Because the F-ratio associated with the Placebo effect was not significant one way or the other, it seems reasonable to conclude that the pill, by itself, had no discernible effect on body-sway changes from pre-trial to post-trial.

But the smallness of the F-ratios associated with the Instructions effect and with the effect of the interaction of Placebo and Instructions, suggests that there is a hidden systematic effect connected with those sources of variance.

One circumstance that might account for minuscule F-ratios would

be that each level of a particular experimental variable had the effect of causing some subjects to respond in one way on the dependent measure, and other subjects to respond in a roughly equal but opposite way. If, then, subjects who responded in these opposite ways were randomly assigned to the experimental subgroups relevant to the experimental variable in question, differences between the subgroups would be minimized as scores with opposite valences tended to cancel each other out.

To make this more concrete, suppose that in the present study the effect of receiving a pill was to make some subjects, of some so far unidentified type, increase their body-sway from pre-trial to post-trial, and other subjects, of a type opposite to that of the former, decrease their body-sway from pre-trial to post-trial. Let us call subjects who when given a pill increase their sway "sway increasers," and those who when given a pill decrease their sway "sway decreasers."

Consider, for the moment, that these are two classes of subjects, and that a subject drawn from the sway increaser class, that is, a subject who increased her sway when given a pill, is one who, had she been assigned to the No Pill subgroup, would not have increased her sway as much or who would have decreased her sway, whereas a subject drawn from the sway decreaser class, that is, a subject who decreased her sway when given a pill, is one who, had she been assigned to the No Pill subgroup, would not have decreased her sway as much or would have increased her sway. Without burdening these speculations

with quantitative factors, such as the amount of sway increase or decrease to be expected from each type of subject under each experimental condition, it is clear that the random assignment of subjects of the two hypothesized opposite types to the experimental conditions of Pill and No Pill, would tend to result in a cancelling out of body-sway changes of opposite directions, thereby obscuring the Placebo effect.

The two opposite types of subjects would, of course, be the source of systematic effect unaccounted for by the experimental design.

The basic investigatory procedure: Although in the present experiment no subject was exposed to more than one experimental condition, so that it certainly could not be determined how a subject assigned to a certain experimental condition would have responded if assigned to another, an attempt was made to find out whether some such set of circumstances as those set forth above might obtain and serve as an explanandum for the puzzling outcomes observed.

The investigatory procedure involved, first, dichotomizing subjects into those who increased their body-sway from pre-trial to post-trial, and those who decreased their body-sway from pre-trial to post-trial. The next step was to examine the other measures that were obtained on the subjects, specifically those other measures analyses of variance of which indicated little or no effect of the original experimental variables or their interactions. In short, those other measures were examined for which neither the original experimental variables nor the interactions thereof produced significant or nearly

significant differences between subgroups of the experimental design. Such measures, it was reasoned, were relatively orthogonal to the original experimental variables.

The two measures found to be relatively orthogonal to the original experimental variables and their interactions were scores on the Drug Effects Questionnaire (DEQ), and Semantic Differential ratings of the concept "Experimenter in this Experiment."

For each of these measures, the mean score for the entire experimental matrix was determined, and subjects were dichotomized into those who scored above the matrix mean on a particular measure vs. those who scored below the mean on that measure.

It was then determined, first for the overall matrix, and second for experimental subgroups relevant to various of the original experimental variables or interactions thereof, and taking into consideration the classes of those who fell above vs. those who fell below the matrix mean for a particular measure, whether there were differences in the distribution of subjects into sway increaser and sway decreaser categories. The experimental subgroups considered were those relevant to the Placebo and Instructions main effects, and to the Placebo X Instructions interaction effect. A nonparametric statistical analysis was used, the chi-square test.

Results of the search for hidden systematic bias: the Drug Effects Questionnaire effect: Investigation, according to the above procedure, of subjects dichotomized on Semantic Differential ratings of the concept "Experimenter in this Experiment," did not prove worth

pursuing.

This left only scores on the Drug Effects Questionnaire as likely candidates in the search for the source of hidden systematic effect.

Table 21 shows the overall DEQ effect when neither the original

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Insert Table 21 about here
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experimental variables nor the interactions thereof were taken account of. It can be seen in Table 21 that of the 49 subjects in the entire experimental design matrix who scored above the DEQ mean of 11.11, 35, or 71%, increased their body-sway from pre-trial to post-trial, and 14, or 29%, decreased their body-sway from pre-trial to post-trial, while of the 63 subjects who scored below the DEQ mean of 11.11, 31, or 49%, increased their body-sway from pre-trial to post-trial, and 32, or 51%, decreased their body-sway from pre-trial to post-trial. The chi-square associated with Table 21 can be seen to be 5.61, on 1 d.f., significant beyond the .02 level. It can therefore be said that there was a highly significant tendency, overall, for subjects who reported on the DEQ that medication worked for them to increase their body-sway from pre-trial to post-trial rather than to decrease it, and for subjects who reported on the DEQ that medication did not work for them to decrease their body-sway from pre-trial to post-trial rather than to increase it.

When subjects were dichotomized into those who scored above the matrix mean of 11.11 on the Drug Effects Questionnaire vs. those who

Table 21: Overall Drug Effects Questionnaire effect

	Above DEQ Mean	Below DEQ Mean	
Increased Objective Sway	35 (E = 28.88)	31 (E = 37.12)	66
Decreased Objective Sway	14 (E = 20.12)	32 (E = 25.88)	46
	49	63	112

Chi-square = 5.61

p less than .02

scored below that mean, and the distribution of these subjects into sway increaser vs. sway decreaser categories as a function of membership in the experimental subgroups relevant to the Placebo and Instructions main effects and to the Placebo X Instructions interaction effect were examined, Tables 22 through 24 resulted. The tables of parti-

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Insert Tables 22 through 24 about here
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cular interest are 22a and b, 23a and b, and 24c.

Table 22b shows that of the 24 No Pill subgroup subjects who scored above the matrix mean of 11.11 on the DEQ, 19, or 79%, increased their body-sway from pre-trial to post-trial, and only 5, or 21%, decreased their body-sway from pre-trial to post-trial, whereas of the 32 No Pill subgroup subjects who scored below the matrix mean of 11.11 on the DEQ, 16, or 50%, increased their body-sway from pre-trial to post-trial, and 16, or 50%, decreased their body-sway from pre-trial to post-trial. The chi-square associated with Table 22b was 4.98, on 1 d.f., significant beyond the .05 level.

Table 22a indicates that of the 25 Pill subgroup subjects who scored above the matrix mean of 11.11 on the DEQ, 16, or 64%, increased their body-sway from pre-trial to post-trial, and 9, or 36%, decreased their body-sway from pre-trial to post-trial, whereas of the 31 Pill subgroup subjects who scored below the matrix mean of 11.11 on the DEQ, 15, or 48%, increased their body-sway from pre-trial to post-trial, and 16, or 52%, decreased their body-sway from pre-trial to post-trial. The chi-square associated with Table 22a is 1.36, on

Table 22

Table 22a: The Drug Effects Questionnaire effect for subjects in the Pill subgroup

	Above DEQ Mean	Below DEQ Mean	
Increased Objective Sway	16 (E = 13.84)	15 (E = 17.16)	31
Decreased Objective Sway	9 (E = 11.16)	16 (E = 13.84)	25
	25	31	56

Chi-square = 1.36

p between .30 and .20

Table 22b: The Drug Effects Questionnaire effect for subjects in the No Pill subgroup

	Above DEQ Mean	Below DEQ Mean	
Increased Objective Sway	19 (E = 15.00)	16 (E = 20.00)	35
Decreased Objective Sway	5 (E = 9.00)	16 (E = 12.00)	21
	24	32	56

Chi-square = 4.98

p less than .05

Table 23

Table 23a: The Drug Effects Questionnaire effect for subjects in the Instructions subgroup

	Above DEQ Mean	Below DEQ Mean	
Increased Objective Sway	17 (E = 12.32)	13 (E = 17.68)	30
Decreased Objective Sway	6 (E = 10.68)	20 (E = 15.32)	26
	23	33	56

Chi-square = 6.50

p less than .02

Table 23b: The Drug Effects Questionnaire effect for subjects in the No Instructions subgroup

	Above DEQ Mean	Below DEQ Mean	
Increased Objective Sway	18 (E = 16.71)	18 (E = 19.29)	36
Decreased Objective Sway	8 (E = 9.29)	12 (E = 10.71)	20
	26	30	56

Chi-square = 0.519

p between .50 and .30

Table 23, continued

Table 23c: The Instructions effect on objective body-sway for subjects who fell below the mean of the Drug Effects Questionnaire

	Increased Objective Sway	Decreased Objective Sway	
Instructions	13 (E = 16.24)	20 (E = 16.76)	33
No Instructions	18 (E = 14.76)	12 (E = 15.24)	30
	31	32	63

Chi-square = 2.67

p between .20 and .10

Table 24

Table 24a: The Drug Effects Questionnaire effect for subjects in the (Pill).(Instructions) subgroup

	Above DEQ Mean	Below DEQ Mean	
Increased Objective Sway	7 (E = 5.50)	7 (E = 8.50)	14
Decreased Objective Sway	4 (E = 5.50)	10 (E = 8.50)	14
	11	17	28

Chi-square = 1.35

p between .30 and .20

Table 24b: The Drug Effects Questionnaire effect for subjects in the (Pill).(No Instructions) subgroup

	Above DEQ Mean	Below DEQ Mean	
Increased Objective Sway	9 (E = 8.50)	8 (E = 8.50)	17
Decreased Objective Sway	5 (E = 5.50)	6 (E = 5.50)	11
	14	14	28

Chi-square = 0.150

p between .80 and .70

Table 24, continued

Table 24c: The Drug Effects Questionnaire effect for subjects in the (No Pill).(Instructions) subgroup

	Above DEQ Mean	Below DEQ Mean	
Increased Objective Sway	10 (E = 6.86)	6 (E = 9.14)	16
Decreased Objective Sway	2 (E = 5.14)	10 (E = 6.86)	12
	12	16	28

Chi-square = 4.15

p less than .05

Table 24d: The Drug Effects Questionnaire effect for subjects in the (No Pill).(No Instructions) subgroup

	Above DEQ Mean	Below DEQ Mean	
Increased Objective Sway	9 (E = 8.14)	10 (E = 10.86)	19
Decreased Objective Sway	3 (E = 3.86)	6 (E = 5.14)	9
	12	16	28

Chi-square = 0.495

p between .50 and .30

1 d.f.; the probability of such a value for chi-square on 1 d.f. lies between .30 and .20.

A direct comparison of the No Pill subgroup subjects who were above the DEQ mean and the Pill subgroup subjects who were above the DEQ mean yielded no significant difference.

The facts remain, however, that 1) overall there was a highly significant tendency for those scoring above the DEQ mean to increase their body-sway from pre-trial to post-trial rather than to decrease it, and for those scoring below the DEQ mean to decrease their body-sway from pre-trial to post-trial rather than to increase it; 2) this DEQ effect continued to appear for subjects who did not get the pill; 3) this DEQ effect disappeared for subjects who got the pill. The most pertinent summary statement would seem to be that receiving the pill inhibited the DEQ effect.

Table 23a shows that of the 23 Instructions subgroup subjects who scored above the matrix mean of 11.11 on the DEQ, 17, or 73%, increased their body-sway from pre-trial to post-trial, whereas of the 33 Instructions subgroup subjects who scored below the matrix mean of 11.11 on the DEQ, only 13, or 39% increased their body-sway from pre-trial to post-trial, and 20, or 61%, decreased their body-sway from pre-trial to post-trial. The chi-square generated by Table 23a may be seen to be 6.50, on 1 d.f., significant beyond the .02 level.

Table 23b shows that of the 26 No Instructions subgroup subjects who scored above the matrix mean of 11.11 on the DEQ, 18, or 69%, increased their body-sway from pre-trial to post-trial, and 8, or

31%, decreased their body-sway from pre-trial to post-trial, while of the 30 No Instructions subgroup subjects who scored below the matrix mean of 11.11 on the DEQ, 18, or 60%, increased their body-sway from pre-trial to post-trial, and 12, or 40%, decreased their body-sway from pre-trial to post-trial. The chi-square generated by Table 23b may be seen to be .52, on 1 d.f.; the probability of such a value for chi-square on 1 d.f. lies between .50 and .30.

A direct comparison of Instructions subgroup subjects who scored above the DEQ mean and No Instructions subgroup subjects who scored above the DEQ mean yielded no difference significant or approaching significance. But a direct comparison of Instructions subgroup subjects who scored below the DEQ mean and No Instructions subgroup subjects who scored below the DEQ mean produced Table 23c, which shows that, of the 33 Instructions subgroup subjects who scored below the DEQ mean, 13, or 39%, increased their body-sway from pre-trial to post-trial, and 20, or 61%, decreased their body-sway from pre-trial to post-trial, whereas of the 30 No Instructions subgroup subjects who scored below the DEQ mean, 18, or 60%, increased their body-sway from pre-trial to post-trial, and 12, or 40%, decreased their body-sway from pre-trial to post-trial. The chi-square generated by Table 23c may be seen to be 2.67, on 1 d.f.; the probability of such a value of chi-square on 1 d.f. lies between .20 and .10. Thus there was a notable but nonsignificant tendency for subjects who scored low on the DEQ to decrease their sway rather than to increase it when they received instructions to increase it, and to increase their sway rather

than to decrease it when they did not receive instructions to increase it.

These results may be summarized as follows: 1) the overall DEQ effect continued to appear for subjects who received instructions for increased body-sway; indeed, if one uses the chi-squares obtained as criteria, the DEQ effect was enhanced for subjects who received instructions for increased body-sway, compared to the overall DEQ effect; 2) this DEQ effect disappeared for subjects who did not receive instructions for increased body-sway; 3) there was a nonsignificant but notable tendency for subjects who scored below the DEQ mean to respond differentially to the levels of the Instructions variable, in that if they received instructions for increased body-sway they more often decreased than increased their sway, and if they did not receive instructions for increased body-sway, they more often increased than decreased their sway.

Table 24c shows that, of the 12 (No Pill).(Instructions) subgroup subjects who scored above the matrix mean of 11.11 on the DEQ, 10, or 83%, increased their body-sway from pre-trial to post-trial and only 2, or 17%, decreased it, while of the 16 (No Pill).(Instructions) subgroup subjects who scored below the matrix mean of 11.11 on the DEQ, 10, or 62%, decreased their body-sway from pre-trial to post-trial, and only 6, or 38%, increased it. The chi-square associated with Table 24c may be seen to be 4.15, on 1 d.f., significant beyond the .05 level.

For none of the other subtables of Table 24 were the associated

chi-squares significant or nearly significant.

Thus, with respect to the experimental subgroups relevant to the Placebo X Instructions interaction effect, the differential effect of scoring above or below the matrix mean on the DEQ was limited to subjects who did not receive the pill, but who did receive instructions for increased body-sway. The nature of this differential effect, absent for the other subgroups relevant to the Placebo X Instructions interaction, was similar to that of the overall DEQ effect, in that significantly more subjects who received instructions for increased body-sway, but no pill, and who also scored above the DEQ matrix mean, increased their body-sway from pre-trial to post-trial rather than decreased it, while significantly fewer subjects who received instructions for increased body-sway, but no pill, and who also scored below the DEQ matrix mean, decreased their sway rather than increased it.

These post-hoc nonparametric analyses have indicated that 1) overall, subjects who said on the DEQ that medications worked for them increased rather than decreased their body-sway from pre-trial to post-trial, whereas subjects who said on the DEQ that medications did not work for them decreased rather than increased their body-sway from pre-trial to post-trial; this phenomenon has been called the "DEQ effect;" 2) the DEQ effect continued to appear for subjects who did not receive the pill, but disappeared for subjects who received the pill; therefore the assertion is warranted that the pill inhibited the DEQ effect; 3) the DEQ effect continued to appear, and was apparently augmented, for subjects who received instructions for increased body-

sway, but was not present for subjects who did not receive such instructions; 4) there was a strong tendency, approaching statistical significance, for subjects who said that medication did not work for them and who received instructions for increased body-sway, to decrease rather than to increase their sway from pre-trial to post-trial, and for subjects who said that medication did not work for them and who did not receive instructions for increased body-sway, to increase rather than to decrease their sway from pre-trial to post-trial; 5) of subjects assigned to the experimental subgroups relevant to a Placebo X Instructions interaction, only those in the subgroup receiving instructions for increased body-sway, but no pill, showed the DEQ effect to a statistically significant degree.

The DEQ effect might well be a source of systematic bias not taken account of by the original experimental design. The fact that DEQ scores were not influenced by the original experimental variables or the interactions thereof, and that a DEQ effect was discovered, provides some reason for asserting that the DEQ may have tapped some subject attribute that was independent of the variables originally built into the experimental situation. Failure to account for that subject attribute in the experimental design may be one reason for the absence, contrary to expectation, of Placebo and Instructions main effects in the present study. Certainly when the DEQ effect was taken into account, as above, an inhibiting effect of the pill on body-sway increase from pre-trial to post-trial, and a facilitating effect of instructions for increased body-sway on body-sway increase from pre-

trial to post-trial, became clearly visible.

However, when by way of corroboration each of the 16 smallest cells of the original experimental design was blocked into subjects scoring high on the DEQ vs. subjects scoring low on the DEQ and an analysis of variance was performed on the resulting matrix with body-sway change scores from pre-trial to post-trial as the dependent variable, none of the interactions of the DEQ variable with the main effects or interactions of the Placebo, Abstract Conditioning, or Instructions variables, were significant or approached significance, and the significantly nonsignificant F-ratios obtained in the original analysis persisted.

The discrepancy between the results of the nonparametric analyses and the parametric analysis can be partially accounted for by the facts that 1) since the original experimental matrix had 7 subjects per cell, building in a new dichotomy for parametric analysis while maintaining equal n required dropping one subject from each cell, with a total loss of 16 subjects from the matrix; the nonparametric analyses made use of all the subjects; 2) in setting up the parametric analysis it was not the case that for every experimental subgroup the 3 highest DEQ scores were above the DEQ matrix mean of 11.11 and the 3 lowest DEQ scores were below that mean; in the nonparametric analyses no subject was said to have scored high on the DEQ unless her score was above 11.11, and none was said to have scored low on the DEQ unless her score was below 11.11; and finally 3) the DEQ neither by itself nor in interaction with the original experimental variables

and their interactions discriminated subjects perfectly into those who increased their body-sway vs. those who decreased their body-sway; therefore with the parametric analysis the mean body-sway for each experimental subgroup was based on pooling the sway scores of sway increasers and sway decreasers; the nonparametrics, on the other hand, separating sway increasers and sway decreasers, avoided that pooling.

To further trace the DEQ clue, blocking on DEQ scores was built into matrices for the following dependent measures other than body-sway: Hallucination Test scores, Subjective Effects Questionnaire scores, Semantic Differential ratings of the concept "Pill," Semantic Differential ratings of the concept "Experimenter in this Experiment," and Awareness Questionnaire scores.

Only three of the analyses of variance with the DEQ dichotomization built in produced results that were statistically significant or that approached statistical significance.

First, in the analysis of variance of Semantic Differential ratings of the concept "Pill," the F-value associated with the DEQ main effect was significant beyond the .05 level ($F(1,64) = 4.02$), reflecting the fact that when the mean of the three summed and averaged Semantic Differential factors was considered, subjects who reported on the DEQ that medication worked for them rated the concept "Pill" significantly higher (4.40) than those who reported on the DEQ that medication did not work for them (4.17). In other words, subjects who said that medication worked for them rated the concept "Pill" signi-

ficantly more positively than those who did not.

Second, in the analysis of variance of Hallucination Test scores, the F-value associated with the Instructions X DEQ interaction approached statistical significance ($F(1,64) = 3.37$; p between .10 and .05). Table 25a represents the means relevant to this interaction,

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Insert Table 25 about here
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and shows that although the Hallucination Test mean for subjects who scored high on the DEQ and who also received instructions for increased body-sway is lower than that of subjects who scored high on the DEQ but who did not receive instructions for increased body-sway, the situation is reversed for subjects who scored low on the DEQ -- of these, those who received instructions for increased body-sway produced a higher mean on the Hallucination Test than did those who received no such instructions. Tests of simple effects indicated that the former difference approached statistical significance ($F(1,16) = 1.93$; p between .20 and .10); the latter was nonsignificant. A test of simple effects also showed that the tendency of high DEQ subjects who had not received instructions for increased body-sway to report experiencing the visual hallucination more frequently than the low DEQ subjects who had not received instructions for increased body-sway, was statistically significant ($F(1,16) = 6.12$; p less than .05).

There was thus a tendency for receiving instructions for increased body-sway to be associated, in the high DEQ subjects, with failure to hallucinate, and in the low DEQ subjects, with hallucination; whereas

Table 25

Table 25a: Means relevant to the Instructions X Drug Effects Questionnaire interaction for Hallucination Test scores

	High on the DEQ	Low on the DEQ
Instructions	0.292	0.333
No Instructions	0.500	0.208

Table 25b: Means relevant to the Placebo X Drug Effects Questionnaire interaction for Hallucination Test scores

	High on the DEQ	Low on the DEQ
Pill	0.333	0.083
No Pill	0.458	0.458

at the same time there was a tendency for not receiving instructions for increased body-sway to be associated, in the high DEQ subjects, with hallucination, and in the low DEQ subjects, with failure to hallucinate.

In brief, then, receipt of instructions for increased body-sway inhibited hallucination in subjects who reported on the DEQ that medication worked for them, and facilitated hallucination in subjects who reported on the DEQ that medication did not work for them; while failure to receive instructions for increased body-sway facilitated hallucination in subjects who reported on the DEQ that medication worked for them, and inhibited hallucination in subjects who reported on the DEQ that medication did not work for them.

The DEQ main effect in the Hallucination Test analysis of variance with subjects blocked on DEQ scores, showed a weak tendency to approach statistical significance. The F-value associated with the DEQ main effect was 1.89, on 1 and 64 d.f., with a p-value lying between .10 and .20. This reflects the fact that subjects who reported on the DEQ that medication worked for them scored slightly higher on the Hallucination Test (.40) than subjects who reported on the DEQ that medication did not work for them (.21). In other words, there was a slight tendency for those who said that medication worked for them to say more frequently that they saw the steady white light flicker, than those who said that medication did not work for them.

The Placebo X DEQ interaction in the Hallucination Test analysis of variance into which blocking on DEQ scores was incorporated, also

revealed a slight tendency to approach statistical significance. The F-value associated with the source of variance in question was 1.89, on 1 and 64 d.f., with a p-value lying between .10 and .20.

The means relevant to this interaction are set forth in Table 25b, where it can be seen that although both those subjects who scored high on the DEQ and those who scored low on the DEQ hallucinated less frequently when they had been given the pill than when they had not, the inhibiting effect of receiving the pill was much more pronounced for subjects who scored low on the DEQ. For subjects who scored high on the DEQ, the inhibiting effect of receiving the pill was not statistically significant; for subjects who scored low on the DEQ, the inhibiting effect of receiving the pill was statistically significant ($F(1,16) = 11.59$; p less than .005).

At the same time, subjects who scored high on the DEQ and who received the pill reported experiencing the visual hallucination significantly more frequently than subjects who scored low on the DEQ and who received the pill ($F(1,16) = 4.50$; p less than .05).

There is evidence, then, for saying that receiving the pill inhibited hallucination, and that it inhibited hallucination more for those subjects who reported on the DEQ that medication did not work for them, than for those subjects who reported on the DEQ that medication did work for them. Also, subjects who reported on the DEQ that medication worked for them and who received a pill reported experiencing the visual hallucination significantly more frequently than subjects who reported on the DEQ that medication did not work for them and who

received a pill.

The third analysis of variance into which blocking of DEQ scores was built and which produced F-ratios that were statistically significant or that approached statistical significance, was that of Awareness Questionnaire scores. In this analysis, one main effect and one interaction effect weakly approached statistical significance.

The F-value associated with the DEQ main effect was 1.89, on 1 and 64 d.f., with a p-value lying between .20 and .10, reflecting the fact that subjects who reported on the DEQ that medication worked for them scored lower on the Awareness Questionnaire (5.81) than subjects who reported on the DEQ that medication did not work for them (7.29). Thus, subjects who said that medication worked for them tended to indicate on the Awareness Questionnaire that they did not know what the experiment was about, while subjects who said that medication did not work for them tended to indicate on the Awareness Questionnaire that they did know what the experiment was about.

The interaction effect that showed a tendency to approach statistical significance was that for the Instructions X DEQ interaction ($F(1,64) = 1.85$; p between .20 and .10). The means relevant to this interaction are presented in Table 26, where it can be seen that sub-

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Insert Table 26 about here
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jects scoring high on the DEQ and subjects scoring low on the DEQ both have higher means on the Awareness Questionnaire when given instructions for increased body-sway than when not given such instructions,

Table 26: Means relevant to the Instructions X Drug Effects Questionnaire interaction for Awareness Questionnaire scores

	High on the DEQ	Low on the DEQ
Instructions	7.83	7.83
No Instructions	3.79	6.75

but that for the high DEQ subjects, the ignorance-creating effect of the No Instructions condition was apparently stronger than for the low DEQ subjects. In fact, a test of simple effects indicates that the high DEQ subjects who did not receive instructions for increased body-sway were significantly less aware ($F(1,16) = 8.98$; p less than .01) of experimental purposes, than were the high DEQ subjects who received instructions for increased body-sway, whereas there was no significant difference between awareness levels of the low DEQ subjects who did and who did not receive instructions for increased body-sway ($F(1,16) = .40$).

Summary of the Drug Effects Questionnaire effect: At this point, it might be useful to highlight the results of the various analyses that were undertaken with subjects dichotomized as being either high or low on the Drug Effects Questionnaire:

1. There was an overall DEQ effect on body-sway, of such a nature that subjects high on the DEQ increased rather than decreased their sway from pre-trial to post-trial, while subjects low on the DEQ decreased rather than increased their sway from pre-trial to post-trial.

2. The pill inhibited the DEQ effect on body-sway.

3. Instructions for increased body-sway facilitated the DEQ effect on body-sway.

4. Low DEQ subjects who received instructions for increased body-sway more often decreased than increased their sway from pre-trial to post-trial; low DEQ subjects who did not receive instructions for in-

creased body-sway more often increased than decreased their sway from pre-trial to post-trial.

5. Of subgroups relevant to the Placebo X Instructions interaction, only the subgroup receiving no pill but receiving instructions for increased body-sway showed the DEQ effect on body-sway.

6. High DEQ subjects rated the concept "Pill" significantly more favorably than low DEQ subjects.

7. Overall, high DEQ subjects showed a weak tendency to report experiencing the hallucination more frequently than did low DEQ subjects.

8. High DEQ subjects who received instructions for increased body-sway failed to hallucinate rather than hallucinated, compared to high DEQ subjects who received no instructions for increased body-sway, and low DEQ subjects who received instructions for increased body-sway hallucinated rather than failed to hallucinate, compared to low DEQ subjects who received no instructions for increased body-sway.

9. Receiving the pill tended to be associated in both high and low DEQ subjects with failure to hallucinate, but more so for low DEQ subjects than for high.

10. High DEQ subjects who received the pill reported experiencing the visual hallucination significantly more frequently than low DEQ subjects who received the pill.

11. There was a weak tendency for high DEQ subjects to fail to report that they knew what the experiment was about and for low DEQ

subjects to report that they knew what the experiment was about.

12. High DEQ subjects who received no instructions for increased body-sway reported significantly less awareness of experimental purposes than high DEQ subjects who received instructions for increased body-sway; but for low DEQ subjects, there was no significant difference between the amount of awareness of experimental purposes reported as a function of either receiving or not receiving instructions for increased body-sway.

Interpretation of the Drug Effects Questionnaire effect as due to the personality dimension of neuroticism-nonneuroticism: Several inferences can be drawn from these results. For one thing, it appears warranted to claim with more assurance than before that the Drug Effects Questionnaire was a rough measure of some subject attribute which was related to body-sway changes from pre-trial to post-trial, and which interacted with the original experimental variables and the interactions thereof to influence not only body-sway changes, but scores on the Semantic Differential ratings of the concept "Pill," scores on the Hallucination Test, and scores on the Awareness Questionnaire.

A somewhat coherent picture of the subjects who scored high on the DEQ vs. the subjects who scored low on the DEQ begins to emerge, and is outlined in the findings enumerated above.

The question of what it is that the DEQ is roughly measuring can be answered only tentatively and speculatively. The DEQ is made up of two kinds of questions, questions about health in the three areas of headaches, common colds, and stomach upset, and questions about

medication. A high score on the DEQ implies that a subject reported experiencing headaches and/or common colds and/or stomach upsets, and furthermore, that there were some medications that relieved those ailments for her. Clearly, there couldn't be medications that relieved an ailment for a person unless the person first had the ailment. We can conclude that persons scoring high on the DEQ are persons who report experiencing certain types of physical ailments.

Some of the objective personality inventories, such as the Health Opinion Survey (MacMillan, 1957), the Maudsley Medical Questionnaire, and the Minnesota Multiphasic Personality Inventory, make use of questions about a range of physical ailments, and when these questions are answered in a direction that indicates that the answerer believes that he experiences the physical ailments concerned, the answerer's score on a general scale of "neuroticism" or "maladjustment" is incremented.

Veroff, Feld, and Gurin (1962), in their factor-analytic study of adjustment and maladjustment, isolated as their first factor one they called "Felt Psychological Disturbance," which was contributed to by positive answers to questions indicating that a subject felt himself to be in relatively poor physical health. Examples of such questions were, "Do you feel that you are bothered by all sorts of ailments in different parts of your body?," and, "For the most part, do you feel healthy enough to carry out the things that you would like to do?" Veroff et. al. found this physical ill health component to be highly loaded on the "Felt Psychological Disturbance" factor for women (factor loading from normalized varimax rotation = 45).

Ingham (1954), in his attempt to corroborate Eysenck's 1947 finding that neurotics, as defined by psychiatric diagnosis, were more suggestible than normals, and to rule out the possibility that standing ataxia, or simple unsteadiness on the feet, accounted for Eysenck's findings, discovered that neurotics, as defined by assignment, by whatever means, to the neurosis center of Whitchurch Hospital in Cardiff, Wales, do indeed have more standing ataxia than normals and are also more suggestible. The design of the Ingham study called for a group of neurotics and a group of normals, all males, to have their body-sway individually tested twice, first for $2\frac{1}{2}$ minutes without suggestion, and then for $2\frac{1}{2}$ minutes while listening to a tape-recording of suggestions for ever-increasing forward body-sway. During both trials, both forward and backward sway was taken into account. It can be seen that the experiment lacked a temporal control, so that it is difficult to know whether neurotics would have increased their body-sway from the first to the second trials without suggestions on either trial, more than a comparable group of normals. An educated guess, however, based on the finding of greater standing ataxia for neurotics than for normals during the first, no-suggestion trial, would be that if the requisite temporal control group had been included in the Ingham study, the results would have supported the contention that over time the standing ataxia of neurotics increases more than that of normals.

If in fact the DEQ is a measure, albeit imperfect, of neuroticism, as is implied, however weakly, by the fact that it consists of

questions with a physical health orientation, and that such questions have been found, for women at least, to load heavily on what has been denominated a factor of "maladjustment" (Veroff et al., 1962), and also by the fact that such physical-health-oriented questions are commonly found on other presumed tests of neuroticism, such as the Health Opinion Survey, the Maudsley Medical Questionnaire, and the Minnesota Multiphasic Personality Inventory, then results 1 and 3 from the listing above can be translated respectively into the statements that neurotics have more standing ataxia than normals, and that neurotics are more suggestible than normals. The former statement is consistent with Ingham's (1954) finding, the latter with the findings not only of Ingham, but also of Eysenck (1947), Furneaux (1946, 1952), Himmelweit et al. (1946), and Ingwarson and Lindberg (1935), all of whom report neurotics to be more suggestible than normals.

Not all of the studies just cited dealt with the same dependent measures. Some dealt with both primary and secondary suggestibility measures (Eysenck, 1947), some with only primary suggestibility measures (Furneaux, 1952; Himmelweit et al., 1946; Ingham, 1954), and some with only secondary suggestibility measures (Ingwarson and Lindberg, 1935). It is difficult to define primary and secondary suggestibility intensionally. Most writers on the subject believe that the crucial discriminandum is the presence or absence of direct communication from a suggester. By "direct communication" seems to be meant that the suggester tells the suggestee in so many words what the suggestee is supposed to experience, as when in the Body Sway Test, con-

sidered a test of primary suggestibility, the suggester says, "You will feel yourself falling forward, etc." Indirect communication would be, then, simply the absence of direct communication, in that the suggester does not say in so many words what the suggestee will experience. Instead the suggestee must infer what he is supposed to experience, as when in the Progressive Weights Test, considered a test of secondary suggestibility, he is not told that he will find that one member of each pair of weights is heavier than the other; rather, under instructions to judge which member of several pairs of weights is heavier, he first experiences several pairs of weights for which it is true that one member of the pair is heavier than the other, and then the suggester gives him some pairs for which it is not true. At no time does the suggester say, "You will find that for all of these pairs of weights, one member of the pair is heavier than the other." This statement is an inference the suggestee must make on the basis of some such instructions as, "For each of these pairs, judge which member is the heavier," and on the basis of his experience with a series of pairs where one member of the pair is in fact heavier than the other. One intensional difference between tests of primary and tests of secondary suggestibility, then, is direct vs. indirect communication of the experience to be expected.

On the face of it, however, another intensional difference between tests of primary and tests of secondary suggestibility would seem to be whether the experience suggested involves effector systems or affector systems. For example, the Body Sway Test, the Arm Levi-

tation Test, the Handclasp Test, and the Chevreul Pendulum Test, all considered tests of primary suggestibility, involve effector systems -- specifically, they all require that the subject produce some change in his voluntary musculature. On the other hand, the Heat Illusion Test, the Progressive Weights Test, the Progressive Lines Test, and the Odor Test, all considered tests of secondary suggestibility, involve affector systems -- they all require that the subject produce some change in his sensations or perceptions.

In the present study, by the direct vs. indirect communication criterion, both the body-sway measure and the Hallucination Test measure had to do with primary suggestibility, since in both cases subjects were told directly what to expect. By the effector system vs. affector system criterion, the body-sway measure had to do with primary suggestibility, since the subject had to produce some change in the effector system involved in standing steady on the sway platform, and the Hallucination Test measure had to do with secondary suggestibility, since the subject had to produce some change in his visual perceptions.

However the Hallucination Test is classified, and continuing on the assumption that the DEQ was a rough test of neuroticism, it is clear from results 7 and 10 above that there was a tendency for the "neurotics," i.e. those high on the DEQ, to report experiencing the visual hallucination more frequently than the "nonneurotics," i.e. those low on the DEQ, a finding consistent with those of the cited studies that report neurotics to be more suggestible than nonneurotics.

The last evidence to be offered that the DEQ may have been a rough measure of neuroticism has to do with the Awareness Questionnaire results. Subjects who scored high on the DEQ did not seem quite as able to figure out what the experiment was about as subjects who scored low on the DEQ, at least under a condition where the clues to the purposes of the experiment were minimal. Whereas subjects who scored low on the DEQ and who did not receive instructions for increased body-sway were about as aware of experimental purposes as subjects who scored low on the DEQ and who did receive instructions for increased body-sway, the high DEQ subjects who received no instructions for increased body-sway were significantly less aware of experimental purposes than the high DEQ subjects who received instructions for increased body-sway. Furthermore, the high DEQ subjects who received no instructions for increased body-sway were almost significantly less aware of experimental purposes than the low DEQ subjects who received no instructions for increased body-sway ($F(1,16) = 3.81$; p between .10 and .05). These findings suggest that perhaps the high DEQ subjects were less able to make use of information present in the experimental environment than were the low DEQ subjects. The high DEQ subjects therefore might be said to have been less cognitively efficient than the low DEQ subjects.

Thus, if the DEQ was a rough measure of neuroticism, then findings 11 and 12 above are interpretable as resulting from the same kind of cognitive inefficiency that is commonly believed by clinical psychologists to separate the relatively maladjusted from the relatively

adjusted, and that is believed to appear in the test results produced by neurotics on such projective techniques as the Rorschach and the Thematic Apperception Test, where failure to produce good W responses, making use of all the blot information, on the former, and failure to make use of all the pictorial data on the latter, have been said to be signs of psychological maladjustment; and on such intelligence tests as the Wechsler Adult Intelligence Scale, where apparent deficits in functioning on some subtests as compared to good functioning on others, have also been attributed to psychological maladjustment.

Placebo effects re-examined. It will be recalled that the originally proposed analysis of the body-sway data did not uncover an effect for the Placebo variable. But post-hoc analyses taking account of subjects' DEQ scores revealed that giving a subject a pill destroyed what has been called the DEQ effect, in that, for those subjects who did receive a pill, the DEQ effect was absent, while for those subjects who did not receive a pill, the DEQ effect was significantly present.

The DEQ effect on body-sway consisted of the finding that subjects who scored high on the DEQ tended to increase rather than to decrease their body-sway from pre-trial to post-trial, that subjects who scored low on the DEQ tended to decrease rather than to increase their body-sway from pre-trial to post-trial, and that these modes of responding differed significantly when compared. The effect of the pill was apparently to reduce the number of high DEQ and low DEQ sub-

jects who increased their sway. This inhibiting effect was greater for the high DEQ subjects than for the low DEQ subjects, in that, under the No Pill condition, 79% of the high DEQ subjects increased their body-sway, as opposed to 64% under the Pill condition; while the comparable percentages for the low DEQ subjects were 50% and 48%.

The inhibiting effect of the pill on the DEQ effect on body-sway can also be observed in the finding that when subgroups relevant to the Placebo X Instructions interaction were examined, the DEQ effect appeared at a statistically significant level only for the subgroup that received instructions for increased body-sway, but no pill.

The pill can be said to have also inhibited experiencing the visual hallucination. Even when the DEQ variable was omitted, significantly more subjects who did not receive the pill reported experiencing the visual hallucination than did subjects who received the pill. When the DEQ variable was taken into consideration, the finding was that there was a tendency for receipt of the pill to be associated with failure to experience the visual hallucination for both high DEQ subjects and low DEQ subjects, but more for the latter than for the former.

It has already been proposed that high DEQ subjects were in fact subjects who would have scored high on scales believed to measure "neuroticism" or "maladjustment," and that low DEQ subjects were in fact subjects who would have scored low on such scales. Under

this assumption, the differences observed in responding to placebo on the measures of body-sway and visual hallucination would be differences characterizing neurotics or maladjusted individuals on the one hand, and normals on the other, in responding to what may be two different types of suggestion, the body-sway measure having to do with primary suggestion, and the Hallucination Test measure, at least by the effector-affector criterion, having to do with secondary suggestion.

The inhibiting effect of the placebo observed in this study for high DEQ as well as low DEQ subjects conflicts with the reports of Gliedman et al. (1958) and Goldman et al. (1965) that subjects who say that medication works for them are those who respond positively to placebo, though at the same time it should be pointed out that high DEQ subjects who got the pill reported experiencing the visual hallucination significantly more frequently than low DEQ subjects who got the pill.

The discovery of this inhibiting effect also conflicts with the many reports in the literature, some of which were cited earlier, of the placebo's powerful effects, effects so powerful that subject attributes have not generally been deemed important and have not generally been incorporated into experimental designs intended to demonstrate placebo effects.

The possible reasons for the discrepant results of the present study are manifold.

First of all, most studies of the placebo effect reviewed herein have lacked either a group of subjects who received neither the placebo

-- anything potentially identifiable as medication -- nor instructions as to what effect the placebo was supposed to have, or have lacked a group of subjects who received the placebo but no instructions.

When placebo effects are obtained in an experiment from which the former group is omitted, the role of the passage of time in producing behavioral changes cannot be evaluated. Thus one cannot know whether the placebo is more powerful than the passage of time. The Baker and Thorpe (1957) study is a case in point. Subjects receiving placebo showed a stronger behavioral change than subjects receiving an active drug, and although it probably made a priori sense to the experimenters in that study to suppose that subjects who were treated the same as the drug group and the placebo group but did not get either a drug or a placebo would not change their behavior, that possibility has certainly not been ruled out. It might have been, for example, that if the requisite group had been included, patients in that group would have improved even more than those in the drug and placebo groups, so that by comparison the placebo might have been said to have inhibited the behavioral change that would have taken place without it.

When placebo effects are obtained in an experiment from which the (Placebo).(No Instructions) group is omitted, the role of simply receiving a pill cannot be evaluated. The Lyerly et al. (1964) study is a case in point. These investigators crossed all combinations of amphetamine and chloral hydrate instructions with the actual administration of amphetamine, chloral hydrate, and a placebo capsule. They

had three control groups, one that received amphetamine with no instructions, one that received chloral hydrate with no instructions, and one that received orange juice with no instructions. All of the groups received orange juice, however, and were told that the orange juice administration was part of a taste-test for the hospital kitchen. No group received a placebo with no instructions. Nor did any group receive instructions without a capsule of some kind. Lyerly et al. report a "considerable placebo effect in the case of Amphetamine instructions" (p. 324), by which they mean that "the Amphetamine instructed Placebo subjects showed impairment of performance as compared with the Control group..." (Pp. 324-5). But the possibility has not been ruled out that a group receiving a placebo capsule with no instructions would have shown no impairment of performance, in which case the "considerable" amphetamine placebo effect would have to be viewed not as a placebo effect but as the effect of instructions about what to expect. Indeed, it may have been that the administration of the placebo capsule was inhibitory, as it was found to be in the present study, or at the very least, superfluous. In brief, the difference between the Lyerly et al. (1964) study and the present one is that in the Lyerly et al. study the term "placebo" is taken to mean "inert medication plus explicit instructions about what to expect," whereas in the present study it was taken to mean simply "inert medication."

The same construction of the term "placebo" as was used by Lyerly et al. is used by Brodeur (1965), whose experiment lacks a (Placebo).(No Instructions) group and a (No Placebo).(No Instructions) group.

There is on the face of it a clear difference between giving a person a placebo and telling him it is a placebo, and giving him a placebo and not telling him what to expect. Brodeur, it will be recalled, was investigating the effects of amphetamine and chloral hydrate instructions, mediated by an inert capsule, on the performance of normal subjects. His control group was given an inert capsule and told that it was a placebo. Interestingly enough, on at least one dependent measure, that of the difference between cumulative laboratory grade average and average laboratory grade earned on the day of the experiment, the control group obtained the highest score, or, in other words, showed the greatest placebo effect. This finding is somewhat paralleled by the findings of the present study. Furthermore, on the adjective checklist dependent measure, Brodeur did not obtain significant differences between the placebo group that received amphetamine instructions, the placebo group that received chloral hydrate instructions, and the placebo group that received placebo instructions, although the means of these groups were ordered correctly. For the pulse-rate dependent measure the differences observed were significant. But the omission of the requisite (Placebo).(No Instructions) and (No Placebo).(No Instructions) groups makes interpretation of the results problematic, in that the effect of inert medication could not be separated from the effect of instructions about what the inert medication was supposed to do.

Besides the fact that the present study included control groups that made it possible to observe that the giving of an inert pill had an inhibitory effect on both a body-sway response and a visual hallu-

cination response, while other studies lacked those control groups, it is also the case that the clinical studies reporting impressive placebo effects, such as those of Gliedman et al. (1958), Steinbook et al. (1965), and Rosenthal and Frank (1956) used as subjects help-seeking patients. In the present study, female college students were the subjects and, as far as is known, were not looking for help from the experimental situation.

Another important, related way in which the present study differed from clinical studies of the placebo is that in the present study, subjects' expectations were aroused of what might well have been seen by them as a negative, rather than as a positive, change in behavior, and that this negative behavior change was associated, for those who got a pill, with the placebo, whereas in the clinical studies it was a positive change in behavior that was associated with the placebo.

Let us trace how this expectation of negative behavior change and the association of negative behavior change with the placebo may have come about in the present study, even for those subjects who were not told explicitly what the effects of the pill would be.

First, there is some reason for asserting that the specific demand characteristics of the experiment, those implicit cues or communications to the subject of how he is expected by the experimenter to behave in the experimental situation (Orne, 1962), of which he is not necessarily aware, called for subjects to increase their body-sway from pre-trial to post-trial. This is the meaning imputed to the

finding that many more subjects in the (No Pill).(No Instructions) control subgroup increased their body-sway from pre-trial to post-trial (19), than decreased it (9), and to the finding that of the four subgroups that were relevant to the Placebo X Instructions interaction, this control subgroup produced the second highest mean sway increase. Such a behavioral change as increasing one's body-sway from pre-trial to post-trial might be viewed as a behavioral change for the worse, on the grounds that it is better to be steady on one's feet than unsteady on one's feet.

The speculation is that subjects who received the pill were also operating under the influence of the demand characteristics of the experiment to increase their body-sway from pre-trial to post-trial, or in other words, to suffer a behavioral deficit. Being under the influence of the demand characteristics of the experiment and at the same time receiving a pill might have been tantamount to being given a communication that the pill was what would cause them to do worse the second time on the sway platform. In essence, then, even when explicit instructions to that effect were lacking, subjects who received the pill were being told that they would do worse after taking the placebo than before taking it.

Therefore subjects in the present study who got the pill may have been motivated to resist the expectations aroused in them of negative behavioral change, whereas it makes sense to suppose that in clinical studies subjects are motivated to accept the expectations aroused in them of positive behavioral change.

At the same time, however, it might not be the fact that the expectation which was aroused was of a behavioral deficit, that triggered resistance. It might have been simply that people do not like to feel that they have been influenced by an outside agent. After all, being unsteady on one's feet, if it is a behavioral deficit, must have been a behavioral deficit to subjects in the (No Pill).(No Instructions) control subgroup, who nonetheless increased their sway. The only difference between these subjects and other subjects in the experiment was that these subjects were not in a position to be able to make the interpretation that their behavioral deficit was due to the influence of an outside agent.

We shall return to the notion that people might not like to feel that they have been influenced by an outside agent, and that they may resist outside attempts to influence them, below, when Instructions effects are re-examined.

Meanwhile, it is possible that the inhibitory effect of the placebo was due to the fact that administration of the placebo destroyed the credibility of the experimenter. 31 of 56 subjects who received the pill made some statement on the Awareness Questionnaire to the effect that the pill was a fake, whereas none of those in the No Pill subgroup made any such statement. In other words, it may have been not only that Pill subgroup subjects believed they were expected to suffer performance decrement and were motivated to fight that decrement, it may also have been that Pill subgroup subjects figured that the experimentor was trying to mislead them, that, even when the lie

embodied in instructions concerning the expected effects of the pill was missing, the experimenter was not communicating the truth. For even when subjects in the present study received a pill but were not told what to expect the pill's effect to be, the implication of what they were told was that the pill would have some effect or other, and these subjects did not believe that implication.

The disbelief of subjects who received the pill may in fact have made it easier for them to fight the expectation arousal. If "belief" or "faith" is a component of placebo responding, and if the kind of attitude change investigated in studies of persuasibility is related to the more general phenomenon of suggestibility, as Abraham (1960) has found, then certainly there is a large corpus of work indicating that credibility of the source of a communication is an important variable determining whether the communication will be accepted or not (Cohen, 1964; Hovland, Janis, and Kelley, 1953). In the present study, subjects who received the pill did not believe that it was genuine; consequently they may not have been prone to accept the communication, whether given to them directly or indirectly, that their body-sway would increase from pre-trial to post-trial.

It is relatively easy to understand why the subjects in the present study were inclined to believe that the pill given them was not genuine. Some of the reasons were made clear to the experimentor during the debriefing session that was held for each subject, when he heard such statements as, "I knew they wouldn't let you give us a real drug," "We read about stuff like this in psychology," "You're not a

doctor and so you couldn't give me any drug that works," and so on. The subjects were all too well aware of the realities of the situation of the psychological experimenter on a college campus, were all too well aware of what was likely to be allowed to the experimenter and what was not likely to be allowed. The fact that a statement on the experimental folder in which the subjects signed up for the experiment claimed that the study had been cleared by the campus Student Health Center, and which was intended to create the illusion that medical men might be involved in the study, was either overlooked or discounted. Furthermore, the subjects were all students in an introductory course in psychology in which, among other things, the placebo response had been touched upon.

In summary, then, the present study, unlike other studies of the placebo, found that presentation of a placebo had relatively minimal effects, and that the effects that did appear were inhibitory of the behavioral change expected by the experimenter and communicated to the subjects either directly or indirectly. These findings were accounted for on the basis of differences in experimental design between other studies and the present one, in terms of subject motivation to avoid performance decrement on the experimental tasks, or perhaps to avoid appearing to be influenced by an outside agent, and in terms of the placebo's destruction of the experimenter's credibility.

Instructions effects re-examined. The basic Instructions effect on body-sway, like the basic Placebo effect on body-sway, was not uncovered by the originally proposed data analysis. Only when the over-

all DEQ effect was taken into consideration by post-hoc data treatments did the effects of instructions for increased body-sway manifest themselves, in that the DEQ effect on body-sway change from pre-trial to post-trial was enhanced for subjects receiving instructions for increased body-sway, and utterly absent for subjects not receiving such instructions. To recapitulate, the overall DEQ effect was for high DEQ subjects to show a clearcut tendency to increase rather than to decrease their sway compared to low DEQ subjects, who showed a clearcut tendency to decrease rather than to increase their sway.

Under the assumption that high DEQ subjects were those possessing a greater degree of neuroticism, and that low DEQ subjects were those possessing a lesser degree of that attribute, the body-sway results of the present study are congruent with those studies of suggestibility in neurotics and normals that have found neurotics to be more suggestible than normals (Eysenck, 1947; Himmelweit et al., 1946; Ingham, 1954; Ingwarson and Lindberg, 1935).

That there was no general Instructions effect on the results of the Hallucination Test is not to be wondered at, since the instructions referred to had to do with body-sway and were on the face of it not relevant to experiencing the visual hallucination.

When the DEQ variable was incorporated into the partitioning of the Hallucination Test data, however, there was a tendency for an Instructions effect to appear, in that high DEQ subjects who received instructions for increased body-sway failed to hallucinate rather than hallucinated, compared to high DEQ subjects who received no instruc-

tions for increased body-sway, while low DEQ subjects who received instructions for increased body-sway hallucinated rather than failed to hallucinate, compared to low DEQ subjects who received no instructions for increased body-sway. Another way of putting this is to say that under the Instructions condition, the low DEQ subjects evidenced a greater tendency to report experiencing the visual hallucination than the high DEQ subjects, whereas under the No Instructions condition, the high DEQ subjects evidenced a greater tendency to report experiencing the visual hallucination than the low DEQ subjects.

An understanding of this finding eludes this investigator, especially in view of the aforementioned fact that instructions for increased body-sway would seem to be irrelevant to responding to suggestions for experiencing a visual hallucination. It was thought that one way in which relevance of body-sway instructions to visual hallucination suggestions could exist would be if those who received instructions for increased body-sway and who did in fact increase their sway were also those who responded positively to the suggestions for experiencing the visual hallucination. There was, however, no hint in the data of such an occurrence. And in any event, it is plain that if the data had fallen out in that way, and if the high DEQ subjects were relative neurotics who responded more readily to the body-sway instructions, then it should have been the high DEQ subjects who received instructions for increased body-sway who hallucinated rather than failed to hallucinate compared to high DEQ subjects who received no instructions for increased body-sway.

As was the case with the Placebo variable, the Instructions variable had an effect on subject accuracy on the Subjective Effects Questionnaire. This effect was similar to the effect on SEQ accuracy of the Placebo variable, in that a larger percentage (44%) of subjects who received instructions for increased body-sway from pre-trial to post-trial and who did in fact increase their sway, reported on the SEQ that their sway had remained the same, relative to the percentage (19%) of those reporting on the SEQ that their sway had remained the same who did not receive instructions for increased body-sway from pre-trial to post-trial and who also did in fact increase their sway. Conversely, relative to the percentage of subjects who did not receive instructions for increased body-sway from pre-trial to post-trial but who in fact increased their sway and reported on the SEQ that they had done so (81%), a smaller percentage of subjects (56%) who received instructions for increased body-sway and who in fact increased their sway, reported on the SEQ that they had done so. It will be recalled that these outcomes could not be explained in terms of differences in discriminability of body-sway increases.

A simpler way of saying this is that if a subject who received instructions to increase her sway did in fact increase it, she tended to say she didn't; whereas if a subject didn't receive instructions to increase her sway but increased it anyhow, she tended to say that she had done so.

It seems likely that the receipt of instructions for increased body-sway mobilized subjects to resist responding in the direction

specified in the instructions. The reason for resisting might have been that the response asked for represented a performance decrement, or negative behavior change. It makes intuitive sense that a person might not be entirely willing to suffer a performance decrement of whatever sort, and might resist attempts to make her do so. Inferences that this is so can be drawn from some of the literature concerning attitude change. For example, Kelley and Volkart (1952) found that boy-scouts who placed a high valuation on membership in the boy-scouts, expressed attitudes that were more positive towards scouting activities after hearing an outside adult give a speech that evaluated scouting activities negatively, than they expressed before hearing the outside adult. It could be said that, for those subjects who placed a high valuation on membership in the boy-scouts, changing their attitudes towards scouting in the direction suggested by the outside adult was seen as a performance decrement, as negative behavior change, which they resisted to such an extent that their attitudes towards scouting activities changed in a direction opposite to that suggested by the outside adult. Weitzenhoffer (1953, p. 199) suggests that the noxious suggestion vs. nonnoxious suggestion distinction is an important one, in that subjects are more likely to wish to resist the former than the latter.

Indeed, it may even be that there is a general tendency in human beings to resist doing what someone else tells them to do. It may be that simply telling someone to do something leads the person told to evaluate the requested action negatively, and to wish to resist it.

It is interesting in this connection that Walster and Festinger (1962) found that communications intended to produce attitude change were more effective in doing so when they were "accidentally overheard" by subjects than when the subjects knew the communications were intended to influence them.

In support of the contention that in the present study telling someone what to do mobilized resistance or, in other words, that subjects tended to resist being influenced when they knew that an attempt was being made to influence them, one may refer back to Tables 18 and 19.

Table 18b shows that subjects who received instructions for increased body-sway and who indicated, relatively speaking, that they were aware of the experimental purposes, including, presumably, the fact that the instructions they received were intended to influence their body-sway performance, tended to decrease rather than to increase their objective body-sway, while those who received instructions for increased body-sway and who indicated, relatively speaking, that they were ignorant of experimental purposes, tended to increase rather than to decrease their objective body-sway. A sensible supposition is that the latter subjects' ignorance of experimental purposes included ignorance of the fact that the instructions were intended to influence their objective body-sway performance, and that, since they did not feel that the experimenter was trying to influence them to increase their body-sway, they could allow themselves to increase it.

Table 19b shows that subjects who received instructions for in-

creased body-sway and who were aware of experimental purposes, including, presumably, the fact that the instructions were supposed to cause them to increase their body-sway, reported on the Subjective Effects Questionnaire that they failed to increase rather than that they increased their sway, while subjects who did not receive instructions for increased body-sway but who were aware of experimental purposes, reported on the Subjective Effects Questionnaire that they increased rather than that they failed to increase their sway.

Interpretation of the performance of the No Instructions subjects is problematic. On the one hand, it could be said that the awareness of these subjects may have included the knowledge that they were in a control subgroup and that if anybody was supposed to increase their sway, it was not them. This interpretation is rendered dubious by the fact that these subjects had no way of knowing what instructions were given to the experimental subgroup subjects. If they had known that the subjects in the experimental subgroup were expected to increase their body-sway, then it might also have been known to them that they themselves, being in a control group, were expected to fail to increase their body-sway. Under these circumstances, they would also have known that reporting sway increases on the SEQ would disconfirm the experimenter's expectations, and their doing so might be considered an indication that subjects who were aware of experimental purposes tended to want to disappoint the experimenter. On the other hand, if they had thought that the subjects in the experimental subgroup were expected to decrease their body-sway, then they might al-

so have thought that they themselves, being in a control group, were expected to fail to decrease their body-sway, in which case their reporting on the SEQ that they did fail to decrease their body-sway could be viewed as an indication that subjects who were aware of experimental purposes attempted to produce evidence confirmatory of experimental hypotheses as they conceived them.

A conservative interpretation is that there was an overall tendency for subjects to increase their body-sway from pre-trial to post-trial, that those who received instructions to do so and who were aware of experimental purposes, including the fact that the experimenter intended for them to increase their body-sway, did not want to appear to have been influenced, and therefore produced protocol statements indicating that they had not been influenced, even when those protocol statements conflicted with what had actually happened; whereas subjects who received no instructions for increased body-sway and who were also aware of experimental purposes, but not of which direction of sway change on their part would make it appear that they had or had had not been influenced by the experimenter, were willing to produce protocol statements indicating that they had increased their sway when in fact they had. In this connection it should be recalled that for the Instructions subgroup subjects who increased their objective sway, the SEQ reports tended to be distorted, were not accurate reflections of direction of objective sway change, while for the No Instructions subgroup subjects who increased their objective sway, the SEQ reports tended to accurately reflect objective sway changes.

The argument is buttressed by consideration of the Subjective Effects Questionnaire data of those who received instructions for increased body-sway from pre-trial to post-trial, but who in actuality decreased their objective body-sway. Both on the basis of common sense and on the basis of the results of the Awareness Questionnaire, it is clear that such subjects know that they were expected by the experimenter to increase their body-sway.

It will be recalled that there was an overall tendency for subjects who decreased their body-sway to report on the SEQ, mistakenly, that their sway had increased or that it had remained the same, rather than that it had decreased (Table 5a).

Table 13b shows, however, that subjects who received instructions for increased body-sway and who in fact decreased their objective sway, deviated from the overall tendency and reported relatively accurately on the SEQ that they had decreased it or that they had remained the same, rather than that they had increased it. Subjects who did not receive instructions for increased body-sway and who in fact decreased their objective sway, failed to deviate from the overall tendency and reported mistakenly that they had increased it rather than that they had decreased it or that they had remained the same.

Although the somewhat unusual standards for sway decreaser accuracy on the SEQ must be kept in mind, such an outcome might reflect a desire on the part of subjects who received instructions for increased body-sway and who were hence presumably aware that the experimenter was trying to influence them, to appear not to have been in-

fluenced. These subjects could therefore report relatively accurately on the SEQ the fact that, as reflected by their remaining the same or actually decreasing their sway, they had not been influenced. On the other hand, subjects who did not receive instructions for increased body-sway and who decreased their objective sway, tended to say that they had increased their sway rather than that they had decreased it or remained the same. It may have been in their case that the demand characteristics of the experiment, implicit in the information on the experimental folder in which they had signed up to participate in the experiment, in the use of the sway platform, in the instructions given them about the way in which they were supposed to stand as motionless as possible on the sway platform, and in the use of two trials on the sway platform, may have communicated to them that they were supposed to increase their sway from pre-trial to post-trial. Since for them there was no blatant attempt by the experimenter to influence them to increase their sway, they were willing to report on the SEQ that they had increased it even when in actuality they hadn't.

If one considers that not only the giving of explicit instructions for increased body-sway but also that the giving of a pill was likely to have been viewed by subjects as an attempt on the part of the experimenter to influence their behavior, further support for the idea that subjects resisted being influenced can be derived from certain data relevant to the Placebo effect.

For example, as was the case with analysis of the Subjective Effects Questionnaire data of subgroups relevant to the Instructions

effect, analysis of the Subjective Effects Questionnaire data of subgroups relevant to the Placebo effect indicated that subjects who received the pill -- i.e. who were exposed to a clear attempt on the part of the experimenter to influence their behavior -- and who actually did increase their body-sway, tended to report, falsely, that their body-sway had remained the same or decreased rather than that it had increased, while subjects who did not receive the pill and who increased their body-sway, tended to report, correctly, that their body-sway had increased rather than that it had remained the same or decreased (Table 12a). The interpretation is that subjects who received the pill and who increased their body-sway attributed the sway increase to the pill and, since they did not want to appear to have been influenced by the pill, reported to the experimenter that they had not been, while those who did not receive the pill and who increased their body-sway were not in a position to feel that the increase had been caused by anything the experimenter had done, and so were free to report, truthfully, the fact that their sway had increased.

Still more support for the notion that subjects did not like for the experimenter to attempt to influence them comes from the Semantic Differential ratings of the concept "Pill." First, there was the fact that subjects who received the pill rated the concept "Pill" significantly more negatively than those who did not receive the pill. When this outcome is taken in conjunction with the fact that subjects who received the pill were significantly more aware than those who did not of the purposes of the experiment, and there-

fore, presumably, more aware too of the fact that the pill was supposed to influence them, it can be speculated that the negative rating given to the pill by those who received it arose from their resistance to being influenced and a consequent tendency on their part to rate influencing agents negatively. Second, there is the fact that subjects who received the pill and who were told specifically what the pill was supposed to do to them, i.e. subjects who could not but have been aware that the pill was supposed to be an influencing agent, rated the pill higher in potency than those who were not told specifically what the pill was supposed to do to them, but lower in "goodness." This outcome could almost be interpreted as reflecting a relationship between perception that a source of influence is indeed capable of influencing one, and a relatively negative evaluation of the source of influence.

These speculations are tempered, however, by the fact that even negative ratings of the concept "Pill" on the Evaluation Factor were positive, compared to the bipolar scale mean of 4.0, and that the concept "Experimenter in this Experiment," the real-world referent of which must clearly have been identifiable as a source of influence, was not rated on the Semantic Differential in a manner analogous to the manner in which the concept "Pill," also identifiable as a source of influence, was rated. The failure of a similar sort of rating to appear for "Experimenter in this Experiment" may have been due to the fact that subjects knew that negative ratings of the experimenter would be seen by him, and that under these circumstances they felt sensi-

tive about giving him negative ratings.

All in all, then, it may not be farfetched to suppose that the subjects in this experiment tended to resist being influenced by the experimenter. As mentioned earlier, the factor of perceived desirability or undesirability of the direction of the influence may also have been an important factor.

The tendencies discussed were much stronger for protocol statements than for objective behavior, for two possible reasons. First, it is easier to say something than to do it. Second, the instrument relevant to protocol statement data in this study, the Subjective Effects Questionnaire, was given after body-sway testing was over, which meant that those subjects who perhaps had not become aware of experimental purposes till the end of the experiment, could change retroactively, as it were, their subjective body-sway performance.

In any event, it is suggested that a subject's SEQ score was an indicator of the way the subject wanted to respond, or at least of the way the subject wanted the experimenter to think she had responded, and that subjects who received either a pill or instructions for increased body-sway from pre-trial to post-trial and who actually did increase their sway were able to interpret the increase as resulting from examiner influence, did not want to appear susceptible to examiner influence, and therefore reported on the SEQ, after the fact, that they had not increased their sway, while subjects who increased their sway without having received either a pill or instructions to do so, did not mind reporting on the SEQ that they had increased their sway,

because they did not have to think of themselves as increasing their sway under the influence of the experimenter.

The point of similarity between the receipt of the pill and the receipt of instructions for increased body-sway that would account for the finding that subjects exposed to either of those stimulus situations and who increased their sway apparently did not want to let the experimenter know that they had done so, is that both stimulus situations were communications that a certain type of behavior was to be forthcoming, both were ways of telling the subjects what to do. It is abundantly clear from the Awareness Questionnaire data that either receiving a pill or receiving instructions was associated with a subject's being able to report to a greater degree what the experiment was about, which may imply knowledge of what kind of responses were expected from her. That is to say, both the pill and the instructions for increased body-sway were presumably effective in giving subjects information about what was supposed to happen, about what the experimenter thought was going to happen. Thus it was more clear to subjects receiving either the pill or instructions for increased body-sway that the experimenter was trying to influence them; it is felt that this knowledge mobilized their resistance.

But those receiving the pill not only tended to tell the experimenter that they had not increased their sway when in fact they had, they also tended to fail to increase their sway, whereas those receiving instructions for increased body-sway, although they too if they did increase their sway told the experimenter that they had not,

did tend, paradoxically, to increase their sway. One could say that the resistance that the pill aroused tended to reveal itself in both motor (body-sway) and verbal (SEQ) modes, whereas that that the instructions for increased body-sway aroused tended to reveal itself only verbally.

If the expression of resistance verbally is equivalent to wanting to resist motorically as well, the implication for the present study is that subjects receiving instructions for increased body-sway, though motivated to resist increasing their body-sway, tended to be unable to resist doing so. This assertion receives further support from the finding herein that subjects scoring high on the DEQ and also receiving instructions for increased body-sway scored as high on the Awareness Questionnaire as subjects scoring low on the DEQ and also receiving instructions for increased body-sway (Table 23a). Both the DEQ highs and the DEQ lows who received instructions for increased body-sway can therefore be said to have been relatively aware that the experimenter was trying to influence them and of what he was trying to influence them to do. If such awareness did in fact mobilize resistance, the high as well as the low DEQ subjects wanted to resist and knew what to do to resist. But of those in the Instructions subgroup, it was only the low DEQ subjects as a set who did resist, in that they decreased rather than increased their objective sway, while the high DEQ subjects as a set might be said to have been unable to resist, in that they increased rather than decreased their sway (Table 23a). Such an outcome as was obtained is

what would have been expected if in fact the high DEQ subjects were more suggestible than the lows -- a speculation tentatively entertained earlier -- since it would make sense for more highly suggestible people to be relatively unable to resist suggestion.

The finding in the present study that some subjects could not resist suggestion would, if solid, be in line with like findings in the literature of hypnosis (Estabrook, 1943; Eysenck, 1943; Schneck, 1947; Watkins, 1947, 1951). If it is objected that the present study did not involve hypnosis, one can only point out that the a priori definition of hypnosis is difficult, and that, as Wells (1924) demonstrated so long ago, and as Barber and his co-workers have consistently demonstrated more recently (Barber, 1969), one does not need either so-called hypnotic induction procedures or the production of a "trance state" to elicit behaviors that have commonly been felt to be elicitable only following hypnotic induction procedures and the production of a trance state.

At any rate, the inference from the data of the present investigation that some subjects could not resist suggestions, should be considered in the light of the circumstance that the Subjective Effects Questionnaire was administered after the motor part of the experiment was over. If the SEQ did function as a measure of how subjects wanted to behave, there is no way of knowing whether their knowledge that the experimenter was trying to influence them, and their consequent wanting to behave in a way contrary to what they believed to be the experimenter's expectations, was present at the time of

the second body-sway trial, or whether it was not till the end of the experiment, when subjects had had time to think events over and figure out that the experimenter had tried to influence them to do thus and such, that the desire to do the opposite occurred, resulting at that time in the SEQ distortion effects.

If knowing that the experimenter was trying to influence them to produce certain behaviors mobilized resistance in the subjects, a bit more can now be said about the finding that there was a weak overall tendency for subjects scoring high on the DEQ to fail to report that they knew what the experiment was about and for subjects scoring low on the DEQ to report that they knew what the experiment was about. It was precisely the high DEQ subjects who tended to increase rather than to decrease their sway and the low DEQ subjects who tended to decrease rather than to increase their sway. A possible interpretation of these results is that the high DEQ subjects, relatively unaware of experimental purposes, relatively unaware that the experimenter was trying to influence them and therefore lacking both the knowledge that would have mobilized their resistance to influence and the knowledge that would have told them how they would have to behave if they were to resist, succumbed to experimenter influence, while the low DEQ subjects, relatively aware of experimental purposes, relatively aware that the experimenter was trying to influence them and therefore possessing the knowledge that both mobilized their resistance to influence and told them how they would have to behave if they were to resist, did not succumb to experimenter influence.

The question remains of why in the present study the effect of Instructions, or suggestions, on body-sway was so fugitive, when there is abundant other evidence that Instructions, or suggestions, have a potent influence on body-sway even when the neurotic-nonneurotic dimension is not taken into account. For example, Stukat (1958), in his thorough investigation of suggestibility, had 52 of 184 subjects, or 28% of his sample, actually fall down. 17 or 1.7% of Eysenck's (1943) 1020 subjects fell -- all 17 that fell were neurotics --, and 4.8% of Ingham's (1954) normals and 8.1% of his neurotics fell. Conservatively one might have expected at least 2 of the subjects in the present study to have fallen. If one based one's estimate on Stukat's figures, one might have expected as many as 31 subjects to have fallen.

There are, however, important differences between the present study and such studies as those cited showing a powerful effect of Instructions, or suggestions, on body-sway. What is probably the most important difference is that in the studies cited, suggestions were given to a subject continually while the subject's sway was being tested, whereas in the present investigation, suggestions were given to the subject approximately 10 minutes before sway was tested. A second difference lies in the content of the suggestions; in the studies cited, the suggestions consisted of telling the subject continually, while his sway was being measured, that he could feel himself falling, was leaning more and more, was falling, etc. The suggestions used in the present study, however, made no mention of the

behavior of falling. Finally, the Stukat (1958) study, which produced the most impressive percentage of fallers, did not make use of a recording or tape, whereas the present study did. In this connection it should be noted that Hilgard (1965) found that direct waking suggestions given live were more effectivo than direct waking suggestions given on tape.

In review, then, the Instructions effect on body-sway, like the Placebo effect on body-sway, while opposite to that of the Placebo in that it facilitated rather than inhibited sway increase, appeared fairly clearly only when subject's scores on the Drug Effects Questionnaire were taken into account. Although it must be remembered that even taking the Drug Effects Questionnaire scores into consideration did not by any means permit the data to be completely ordered, there was said to be some reason for suspecting that the DEQ measured in some imperfect way the trait of neuroticism, and if so, it was continued, the findings of the present study concerning the effect of Instructions on body-sway could be understood by invoking the findings of other investigators, of differences in suggestibility between neurotics and nonneurotics. At the same time, inferences were drawn from the Subjective Effects Questionnaire data and the Awareness Questionnaire data, taken in conjunction with the body-sway data, that the receipt of instructions for increased body-sway tended to arouse resistance to experimenter influence in subjects, resistance possibly mediated by subjects' awareness that the experimenter was trying to influence them and in what direction. There was also reasoning pre-

sented to indicate that, as has been reported by other researchers, some subjects could not resist suggestion despite a desire to do so. Finally, the failure of the present study to demonstrate the powerful Instructions, or suggestions, effects reported by other investigators, many of whom did not need to take the neurotic-nonneurotic personality dimension into consideration, was attributed to crucial differences in procedure between the present study and those others.

The Placebo X Instructions interaction effect. A simple model: The Placebo X Instructions interaction effect was completely absent from the originally proposed analyses of body-sway data; in fact, the F-ratio associated with this effect was monumentally small. Nor did the post-hoc analyses taking DEQ scores into account bring to light any interaction between the Placebo and Instructions variables.

Only one of the studies cited herein, that of Lyerly et al. (1964) was conducted in a manner that would provide some indication of the nature of a Placebo X Instructions interaction, and although, as has been pointed out above, the Lyerly et al. study is not truly comparable with the present one, it should be mentioned that not only did the Lyerly et al. study, like the present one, find no Placebo X Instructions interaction effect, it also, like the present one, produced significantly nonsignificant F-ratios for such an effect.

It is interesting and perhaps worthwhile to briefly point out that such an outcome would be expected if certain inferences are drawn from assertions made in the discussions above of the Placebo and Instructions main effects obtained in the present study, namely, that

one of the levels of the two variables is inhibitory in its effect, and one of the levels of the other variable is facilitatory.

In the present study, the variable one of the levels of which was inhibitory was the Placebo variable; specifically, giving the pill was inhibitory. The facilitatory variable was Instructions; specifically, giving instructions for increased body-sway was facilitatory.

Under the assumption that both facilitatory effects and inhibitory effects appearing in an investigation of suggestion are mediated by the information-giving properties of the suggestion, a model can be constructed which correctly orders the body-sway means relevant to a Placebo X Instructions interaction. This model makes no pretensions to mathematic sophistication, but is nonetheless offered as partial corroboration of some of the contentions made earlier.

Consider the matrix presented in Table 27. The upper and left-

****
Insert Table 27 about here
****

hand values are weights, derived from the Awareness Questionnaire data, representing the information-giving properties of each level of each variable. It was clear from the Awareness Questionnaire data that both the No Instructions and the No Pill conditions were associated with subjects' attaining scores of approximately 5.00 on the Awareness Questionnaire, while both the Instructions and the Pill conditions were associated with subjects' attaining scores of approximately 8.00 on the Awareness Questionnaire (Table 17). The signs given the upper and left-hand marginals represent inhibition (negative sign) and

Table 27: Matrix illustrating model generating outcomes obtained in the present investigation for the Placebo main effect, the Instructions main effect, and the Placebo X Instructions interaction effect

	Instructions +8	No Instructions +5	
Pill -8	0	- 3	- 3
No Pill +5	+13	+10	+ 23
	+13	+ 7	

facilitation (positive signs). When the upper and left-hand marginals relevant to each cell in the matrix are algebraically summed, the numbers appearing in each cell of the matrix result, and ranking the cells of the matrix in the natural number system order of the value assigned each cell reproduces the order of the ranking of cells that was obtained in the present study when body-sway increase was the quantity on which the ranking was performed.

Furthermore, when one sums algebraically across rows and across columns, ranking the row and column sums obtained reproduces correctly the standings of the body-sway means obtained in the present study for, respectively, the Placebo effect and the effect of Instructions.

A basic flaw in experimental logic: It has been reported that even taking the Drug Effects Questionnaire scores into account did not eliminate the minuscule F-ratios obtained in the present study, the presence of which have been taken to indicate the operation of a systematic bias. The implication is that some powerful systematic bias still remains. What that bias might be is the concern of the following section.

The failure of the originally proposed analysis of variance to demonstrate any suggestion effects, placebo or otherwise, of the sort expected, may stem from the fact that the experimental design employed, which was intended to separate the effects of giving a subject an inert medication from the effects of giving a subject a communication about what she was supposed to experience, perforce involved a subtle flaw.

To explain this flaw, it is necessary first to point out that

direct suggestion and direct communication by Person A to Person B of what Person B is going to experience, are one and the same. Likewise, indirect suggestion and indirect communication by Person A to Person B of what Person B is going to experience, are identical.

A distinction has long been made in the literature on suggestion and suggestibility, between direct and indirect, or primary and secondary, or prestige and nonprestige suggestion. There is abundant evidence (Duke, 1964; Eysenck, 1957; Stukat, 1958; Weitzenhoffer, 1953) that the two types of suggestion are not related, that people who respond to direct suggestion are not necessarily those who also respond to indirect suggestion, and that people who respond to indirect suggestion are not necessarily those who also respond to direct suggestion. According to the analysis made earlier, the subgroups of the experimental design relevant to the Placebo X Instructions interaction may be classified as follows with respect to a direct vs. indirect suggestion dichotomy:

(Pill).(Instructions) -- direct

(Pill).(No Instructions) -- indirect

(No Pill).(Instructions) -- direct

(No Pill).(No Instructions) -- neither

The (No Pill).(No Instructions) subgroup is classified as being neither direct nor indirect suggestion because it can be demonstrated that subjects in that subgroup received virtually no communication, that they were able or willing to articulate, concerning what they were supposed to experience, if, as it makes sense to suppose, knowing what they were

supposed to experience is the same thing as awareness of experimental purposes.

It is clear that when the analysis of variance was used to compare the subjects who received the pill with the subjects who did not receive the pill, it could not be claimed that the Pill subjects and the No Pill subjects, each considered as a separate group, were identical except for the fact that one group received a pill and one group did not. The group that received the pill can be seen to consist of subjects exposed to direct suggestion and subjects exposed to indirect suggestion; the group that did not receive the pill can be seen to consist of subjects exposed to direct suggestion and subjects exposed to neither direct nor indirect suggestion.

The logic of any kind of experimental design requires that two groups that are being compared be of identical composition; that logic is subtly violated by the design employed in the present experiment.

It is violated not only for the Placebo variable, but also for the Instructions variable, in that the group of subjects who received instructions for increased body-sway may be seen to consist solely of subjects exposed to direct suggestion, whereas the group of subjects who did not receive instructions for increased body-sway may be seen to consist of two subsets, one subset exposed to indirect suggestion, and the other subset exposed to neither direct suggestion nor to indirect suggestion.

The violation persists to some extent even if one considers that

the members of the (No Pill).(No Instructions) subgroup, who at least fell under the influence of the demand characteristics of the experiment, can for that reason be said to have been exposed to indirect suggestion. Under this assumption, however, the violation becomes less clearcut. In the case of Pill vs. No Pill comparisons, both the Pill and No Pill groups may be seen to consist of two subsets of subjects, one subset exposed to direct suggestion and the other to indirect suggestion, although there is a difference between the indirect suggestion subset of the Pill subgroup and the indirect suggestion subset of the No Pill group in that the indirect suggestion subset of the latter group is exposed to no external agent to which subjects can attribute changes in their experimental behavior. In the case of Instructions vs. No Instructions comparisons, the violation all but disappears, since the direct vs. indirect suggestion variable and the Instructions vs. No Instructions variable become, simply, different names for the same thing -- although one must remain aware that the No Instructions or indirect suggestion group may be seen to consist of subjects exposed to two different types of indirect suggestion.

In view of evidence given earlier that the presence of some agent identifiable by subjects as a source of external influence may be crucial, the most reasonable course would seem to be to consider the (No Pill).(No Instructions) subgroup subjects to be qualitatively different from subjects in any of the other subgroups relevant to the Placebo X Instructions interaction, and to hold the first analysis made con-

cerning the characterization of the subgroups relevant to the Placebo X Instructions interaction with respect to the direct vs. indirect suggestion distinction, as the most valid one.

To make the violation of experimental logic under discussion more clear, we may think of a study in which the effects of amphetamines and tranquilizers on running speed are being investigated. Suppose that the amphetamine group subject sample consists of two-legged runners and those with only one leg, and the tranquilizer group subject sample consists of two-legged runners and paraplegics. One could say nothing about the effects on running speed of amphetamines as opposed to tranquilizers, because the subjects comprising each group differ from one another in other ways than merely what kind of drug they have received. It is not the case that all factors are being held constant while one is varied.

This flaw, subtle in terms of discovery but potentially gross in terms of effect on statistical results, may in fact be the basic reason for the failure of the present investigation's originally proposed analysis of variance to demonstrate either the effect on body-sway of receiving a Placebo or not, or the effect on body-sway of receiving instructions for increased body-sway or not, or the effect on body-sway of the interaction between these two variables. Any experiment that attempts to separate receipt of medication from receipt of explicit communication of the effects that medication is going to have, must contain the same flaw. For purely logical reasons, it may be impossible to separate the effects of the two factors.

The effects of Abstract Conditioning

Several predictions were made in the introduction to this dissertation, about the effects of the various levels of the Abstract Conditioning variable.

First, it was stated that there would be a significant Abstract Conditioning X Instructions interaction. The predicted nature of this interaction was that, where A_1 designated Positive Abstract Conditioning, A_2 Negative Abstract Conditioning, A_3 Sensory Control, and A_4 Temporal Control, and where G_1 designated instructions for increased body-sway and G_2 no instructions for increased body-sway, the following relationships would hold among the body-sway increase means of the subgroups: $A_1G_1 > A_3G_1 = A_4G_1 > A_1G_2 = A_2G_2 = A_3G_2 = A_4G_2 > A_2G_1$. In brief, 1) the Abstract Conditioning procedure was expected to make a difference only when there were some instructions for increased body-sway given to the subject; 2) when instructions for increased body-sway were given, the highest mean increase in body-sway from pre-trial to post-trial was expected to be shown by subjects exposed to the Positive Abstract Conditioning procedure (A_1), the lowest by subjects exposed to the Negative Abstract Conditioning procedure (A_2); 3) those subjects who received instructions for increased body-sway and who were previously exposed to either the Sensory or Temporal Control procedures (A_3 and A_4, respectively), were expected to produce mean body-sway increases from pre-trial to post-trial falling between those of the A_1 and A_2 Instructions groups.

Furthermore, subjects exposed to the Positive Abstract Conditioning procedure were expected to report experiencing the visual hallucination more frequently than those exposed to the Negative Abstract Conditioning procedure.

And finally, subjects exposed to the Positive Abstract Conditioning procedure and who not only received instructions for increased body-sway but who also found that they did in fact increase their body-sway, were expected to show more of a tendency to experience the visual hallucination than those exposed to the Positive Abstract Conditioning procedure who also received instructions for increased body-sway but who found that in fact they did not increase their body-sway.

With regard to the first prediction, while it is true that the analysis of body-sway difference scores did produce an Abstract Conditioning X Instructions interaction that approached statistical significance, the ordering obtained of the means relevant to that interaction in no way approached the ordering predicted. For example, of subjects who received instructions for increased body-sway, those exposed to the Negative Abstract Conditioning procedure increased their sway the most (as opposed to the prediction that the Positive Abstract Conditioning subgroup subjects would be in this position), those exposed to the Positive Abstract Conditioning procedure increased their sway the next most (as opposed to the prediction that the Sensory and Temporal Control procedure subgroup subjects would occupy this position), those exposed to the Sensory Control procedure increased their

sway the next most (as opposed to the prediction that subjects in this subgroup would produce a mean sway increase falling between that produced by those exposed to the Positive and Negative Abstract Conditioning procedures), and those exposed to the Temporal Control procedure increased their sway the least (as opposed to the prediction that the Negative Abstract Conditioning procedure subgroup subjects would occupy this position).

One might have contended that the ordering of means obtained for subjects who received instructions for increased body-sway and who were also exposed to one or another of the levels of the Abstract Conditioning variable (Figure 2), could be accounted for by the fact that under both the Positive Abstract Conditioning procedure and the Negative Abstract Conditioning procedure, subjects did in actuality hear the experimenter make true assertions about an experience they were going to have, whereas under neither the Sensory nor the Temporal Control procedures did subjects hear the experimenter make assertions, true or otherwise, about an experience they were going to have, and that this difference was more important than the number of true assertions that subjects heard the experimenter make. But such a contention would stand on a weak foundation for two reasons. The first reason is that the difference between the Positive and Negative Abstract Conditioning subgroup means on the one hand, and the Sensory and Temporal Control subgroup means on the other, was resoundingly nonsignificant ($F(1,12) = 1.13$). The second reason is that such a contention implies that subjects who, after having been told that they

would increase their sway, did increase their sway, would report the visual hallucination when told that they would experience it, more frequently than those who, after having been told that they would increase their sway, failed to increase their sway. Such an outcome was observed neither when the measure of sway change employed was objective nor when it was subjective, i.e. Subjective Effects Questionnaire data.

The second prediction, that subjects exposed to the Positive Abstract Conditioning procedure would report experiencing the visual hallucination more frequently than subjects exposed to the Negative Abstract Conditioning procedure, was not borne out even to the extent of a trend -- the same number of subjects, 10 out of 28, in each of these subgroups reported experiencing the visual hallucination. Furthermore, analysis of the Hallucination Test data produced an insignificant overall effect for the Abstract Conditioning variable, which can therefore be said to have had no discernible effect whatsoever on reported experiencing of the visual hallucination.

The outcome of the third prediction, that subjects exposed to the Positive Abstract Conditioning procedure and who not only received instructions for increased body-sway but who also found that they did in fact increase their body-sway, would show more of a tendency to experience the visual hallucination than those exposed to the Positive Abstract Conditioning procedure who also received instructions for increased body-sway but who found that in fact they did not increase their body-sway, has already been foreshadowed by the failure to find

that subjects who, under instructions to increase their sway did do so, reported the visual hallucination more frequently than subjects who, under instructions to increase their sway, did not do so. Taking account of the Positive Abstract Conditioning procedure did not turn this failure into success, once again whether the sway measure considered was objective or subjective.

With no dependent measure was there an Abstract Conditioning X Placebo interaction effect observed.

All in all, then, the reasonable conclusion is that the present study failed to support the abstract conditioning hypothesis as important for the understanding of the placebo response or for the understanding of response to suggestions.

Why did this study fail to provide support for the hypothesis when the studies cited in the introduction hereto implied so strongly that the hypothesis would be supported?

The first explanation that comes to mind has to do with the flaw in the experimental design discussed above. Although the Abstract Conditioning main effect should not have been influenced by the failure of the experimental design to take account of the direct vs. indirect suggestion distinction -- each level of the Abstract Conditioning variable can be seen to be comprised of identical subgroups, two subgroups exposed to direct suggestions, one exposed to indirect suggestions, and one exposed to neither direct nor indirect suggestions --, the effects crucial to the abstract conditioning hypothesis in fact were the interactions of the Abstract Conditioning variable with the

Placebo or Instructions variables and with the Placebo X Instructions interaction. But it has been shown that the subgroups relevant to the Placebo and Instructions variables and to the interaction thereof violate a basic canon of inductive logic and experimental design. While the precise effects of such a violation cannot be traced, the violation is so fundamental as to receive first consideration as the cause of the study's failure to provide support for the abstract conditioning hypothesis.

A contribution to this failure is also likely to reside in the fact that the present study was conducted in a manner different from those studies that did provide support for the abstract conditioning hypothesis.

One difference which may be important is that the three human studies cited (Corn-Becker, Welch, and Fischelli, 1949; Grings, Carlin, and Appley, 1962; Waters and Kodman, 1962), all presented their subjects with true assertions about experiences in a number of sensory modalities, whereas the present study was basically designed to present subjects with true assertions about experiences in the visual modality. That is, in the three studies cited, subjects were given true statements about what they would experience visually (colored lights, flickering lights), auditorily (music), and tactilely (breeze from an electric fan). In the present study, subjects who were supposedly being abstractly conditioned received true statements only about what they would experience visually. In its barest essentials, however, the abstract conditioning hypothesis as propounded by Welch

(1947) does not have as one of its premises that the true statements presented to the subject must deal with experiences in a wide range of sensory modalities.

Another important difference between the present study and the three human studies cited is that all three of those studies used the GSR as a dependent measure, whereas the present study used as its two basic measures body-sway and visual hallucination. It stands to reason that the GSR is a more easily influenced response than the more complex, more grossly motor behavior of body-sway, on the one hand, and the more complex, more grossly sensory task of experiencing a visual hallucination, on the other -- supposing, for the time being, that subjects who report seeing something that isn't there actually do experience what they say they experience. But of course, interest in the power of the placebo and/or of suggestion on the GSR does tend to be limited, and therefore evidence that Abstract Conditioning can amplify, as it were, the influence of placebo and/or suggestion on the GSR is, by itself, limited in significance.

The present study differed also from the animal studies cited in support of the abstract conditioning hypothesis, primarily in that the present study used human subjects rather than rats or dogs.

Furthermore, except for the Herrnstein (1962) and Balagura and Hoebel (1967) experiments, the animal studies cited used as dependent measures responses involving visceral and smooth muscle, or glandular, autonomic mechanisms, whereas the body-sway response that was the principal dependent measure in the present study involved skeletal, striped

muscle, central nervous system or voluntary mechanisms.

Finally, in linguistic terms, all of the animal studies required their subjects to deal only with sequences of real-world events, while the present study required its subjects to deal with statements about real-world events, or object language, as well as with statements about object language, or metalanguage. Learning that presumably occurred in the animal studies had to do with discerning the relationship of one real-world event to another, that is to say, learning that a hypodermic injection would be followed by a certain complex of bodily changes. Learning that was to have occurred in the present study had to do first with the relationship of language about a real-world event to a real-world event, and then with the relationship of language about language about a real-world event to language about a real-world event. In other words, the human subjects in the present study had first to learn that what the experimenter told them about what they would see in the stimulus box was true, and then had to learn that a metalanguage statement, "What this experimenter tells me is true," was true. Thus the learning required of subjects in the present study was more complex than that required in the animal studies, and failure to learn could have occurred either at the point of discerning that when the experimenter said a red light would be seen in the stimulus box a red light was in fact seen (learning here having to do with finding that the object language was followed by the real-world event that confirmed it), or at the point of discerning that the statement about object language "What this man tells me about real-

world events is true," was followed by an object language-real-world event sequence that confirmed it.

It might be maintained that the present study failed to demonstrate an effect of Abstract Conditioning on body-sway because the true assertions presented to subjects who were supposedly being abstractly conditioned dealt with what they would experience visually, while the experience the expectation of which was to be aroused was kinesthetic. Thus someone might claim that in order for Abstract Conditioning to have an effect, there must be a match between the area of experience about which the true assertions are made, and the area in which the subject is to be led to expect that he will experience something, so that, if one wishes to use Abstract Conditioning to arouse a person's expectations that he will experience something auditorily, one must present him with true assertions about auditory experiences, and if one wishes to use abstract conditioning to arouse a person's expectations that he will experience something olfactorily, one must present him with true assertions about olfactory experiences, and so on and so forth. Aside from the fact that, once again, such a premise is absent from Welch's (1947) conceptualization of Abstract Conditioning, the present study tends to cast doubt upon the validity of adding it. If this premise of the necessity of matching true assertion content to the experiential mode wherein expectation is to be aroused is tenable, one should have found in the present study that at the very least those exposed to one or another of the Abstract Conditioning procedures in which true assertions were

made about what they would see when they looked into the stimulus box, i.e. subjects exposed to either the Positive or Negative Abstract Conditioning procedures, would have more frequently reported seeing the white light flicker when told they would see it do so, than those not in receipt of true assertions about what they would see when they looked into the stimulus box. Such an outcome was not observed.

At the same time, it should be pointed out that there was one level of the Abstract Conditioning variable which, taken in conjunction with the receipt or nonreceipt of instructions for increased body-sway from pre-trial to post-trial, was associated significantly with subjects' reporting during Phase 5 of the experiment either that they saw or did not see the white light flicker. Significantly more subjects who were exposed to the Sensory Control procedure reported seeing the white light flicker, provided they had also been told earlier in the experiment that they would experience a sway increase from pre-trial to post-trial, than subjects not exposed to the Sensory Control procedure but also operating under the instructions for increased body-sway condition. Not only that, but significantly fewer subjects who were exposed to the Sensory Control procedure reported seeing the white light flicker, provided they had not been told earlier in the experiment that they would experience a sway increase from pre-trial to post-trial, than subjects not exposed to the Sensory Control procedure but also operating under the no instructions for increased body-sway condition.

Why this should have been is far from clear. The Sensory Con-

trol procedure, it will be recalled, was the one wherein subjects were to depress the toggle switch on the stimulus box each time they heard a buzzer. At no time were they told what they would or would not see when they locked into the stimulus box. That such subjects should, when under instructions for increased body-sway, report the visual hallucination significantly more, and when not under instructions for increased body-sway report the visual hallucination significantly less, than subjects exposed to other levels of the Abstract Conditioning variable who likewise were under either the Instructions or No Instructions condition, remains a mystery. This outcome can certainly not be considered as evidence for the abstract conditioning hypothesis, as might reasonably have been claimed had subjects who received instructions for increased body-sway actually increased their body-sway more than those who did not receive such instructions and had they thus belonged to a group that found that what the experimenter told them they would experience they experienced. If that had happened, it could have been said that these subjects learned to see the experimenter as a source of true statements in the body-sway situation, and simply generalized this learning to the visual hallucination situation, and that the Sensory Control procedure, involving visual experience as it did, facilitated the generalization. But when one thinks about it further, it is clear that all the other levels of the Abstract Conditioning variable also involved visual experience.

One level of the Abstract Conditioning variable, the Temporal Control level, interacted with the Instructions variable in a way signi-

ficantly different from the interactions of the other levels of the Abstract Conditioning variable with the Instructions variable. For all levels of the Abstract Conditioning variable except the Temporal Control level, subjects who received instructions for increased body-sway increased their sway from pre- to post-trial more than their compeers who did not receive instructions for increased body-sway. But Temporal Control subjects increased their sway more when under no instructions for increased body-sway than when under instructions for increased body-sway (Figure 2).

The Temporal Control procedure involved having a subject circle N's on a page Xeroxed from the Amherst, Mass. telephone directory until the experimenter told her to stop, a period of about 10 minutes. It is suspected that this task was onerous and boring, that it seemed nonsensical and purposeless, and that it therefore motivated subjects exposed to it to resist experiencing what the experimenter suggested that they experience. Alternatively, subjects may have felt that their adequacy was being tested, and may have resented the fact that under the circumstances they could not but appear inadequate since it was impossible to finish the letter-circling task in 10 minutes.

The contention that the Temporal Control condition somehow mobilized resistance in subjects is supported by the fact that subjects exposed to the Temporal Control procedure reported experiencing the visual hallucination significantly less frequently, provided they had also been told that they would increase their body-sway from pre-trial to post-trial, than subjects exposed to the procedures of the other

levels of the Abstract Conditioning variable who also were told that they would increase their body-sway from pre-trial to post-trial (Figure 4).

In other words, it seems that exposure to the task of circling N's on a page Xeroxed from a telephone directory for 10 minutes may have motivated subjects to resist any suggestion the experimenter gave them, whether the suggestion had to do with experiencing increased body-sway or with experiencing a visual hallucination. It is felt therefore that the Temporal Control procedure, instead of being a true control, neutral in its effects, was in fact a motivating condition in its own right. A more appropriate task for the Temporal Control group, one which might at least have made sense to the subjects therein and that would not have been so easily interpretable as a test of adequacy, would have been to have the members of this group fill out some standard personality inventory for 10 minutes, or perhaps for 10 minutes answer a series of such nonthreatening questions as, "How often do you go to the movies?," and "Which of the following four kinds of movies do you prefer?"

In summary, this study failed to support the abstract conditioning hypothesis or the model of expectation arousal presented in the introduction, in any way whatsoever. Factors contributing to this failure were probably the procedural ones discussed and, most basically, the fundamental flaw in the experimental design.

PART 5

CONCLUSION

Essentially, this dissertation dealt with responses to suggestions, and was intended, first, to demonstrate that the so-called abstract conditioning procedure was important for the understanding of responses to suggestions, whether those suggestions were mediated by the receipt of an inert medication or not, and second, to separate the effects of inert medication from those of instructions, or suggestions, alone.

As tested herein, the abstract conditioning hypothesis was found to be untenable, at least in the form espoused by its originator, Welch (1947). It could well be that using the experimental manipulation used by Corn-Becker, Welch, and Fischelli (1949), Waters and Codman (1962), and Grings, Carlin, and Appley (1962) would have produced results supportive of a modification of the abstract conditioning hypothesis, a modification wherein the crucial factor is not simply one person's learning to see another person as a source of true statements, but one person's learning to see another person as a source of true statements about a number of areas of experience. Perhaps it would be worthwhile to redo the present study, substituting the experimental manipulation used in the three cited studies for the manipulations used herein, and also being careful to use control procedures that are empirically neutral, not simply control procedures that on the face of it should have no influence on the dependent measures being taken. For example, on the face of it, there was little reason to sus-

pect that circling N's on a page from a telephone directory would not be completely neutral with respect to body-sway, but, in conjunction with receipt of instructions for increased body-sway, it was far from neutral.

The model of expectancy arousal presented in the introduction was nothing but a speculative extension of the abstract conditioning hypothesis, and therefore, of course, fell with that hypothesis, both in broad outline and with respect to such details as the influence of the number of true assertions presented to a person.

If suggestion is conceived of in its simplest terms as a communication from Person A to Person B that Person B will undergo some sensory and/or motor experience or other, then certainly the results of the present investigation have to do with suggestion. Specifically, on the one hand the results of the present investigation fundamentally have to do with the responding of subjects to inert medication alone, that is to say, to inert medication without explicit communication concerning what the subjects will experience, as well as with the responding of subjects to inert medication in combination with explicit communication concerning what the subjects will experience. The likelihood should be pointed out that even when inert medication is given alone and without explicit communication concerning what its receiver is to experience, it is freighted nonetheless with implicit communications concerning what its receiver is to experience, and can therefore be thought of as falling within the boundaries of the concept "suggestion." Responses observed in these two inert medication situa-

tions make up the category of placebo responding, a type of responding to suggestion.

On the other hand, the results of the present investigation fundamentally have to do with the responding of subjects to explicit communication alone, without the prop of an inert medication, of what the subjects will experience. This is another type of responding to suggestion.

There are two ways of looking at the results of the present investigation. One way of looking at these results is to conclude that the effects of all the types of suggestion involved were found to be relatively minimal. The other way is to conclude that, considering the notable differences between the design and procedures of the present investigation and the design and procedures of studies that have demonstrated a powerful effect of suggestion in either the placebo situation or the nonplacebo situation, it is surprising that any effect of suggestion was found at all.

The present study revealed no overwhelming response to placebo in the direction communicated to subjects by the experimenter. Failure of the placebo response was felt to be due to the fact that within the situation arranged in the present study, the placebo was in general accurately identified by subjects as a fake; the resulting destruction of the experimenter's credibility may then have interacted with a general tendency of the subjects to resist being influenced, especially when the direction of the influence could be seen as productive of a behavioral deficit (becoming unsteady on one's feet), to wipe out

all traces of the placebo response. Future studies involving placebos should include more stringent procedures than the present one did for ensuring that the inert medication administered stands a chance of being seen as being active. Outcomes of the present study might have been different had it been conducted on the premises of the Student Health Center or had the illusion that active drugs might really be involved been created in some other way.

Not only the effect of the receipt of inert medication, but also the effect of the receipt of explicit suggestion, was no doubt obscured by the subtle flaw in the experimental design. This flaw sprang from the endeavor to separate the effects of the two variables. Therefore it is herewith recommended that future studies of the placebo response or effect forego attempting to separate the receipt of inert medication factor from the factor of receipt of communication concerning what the medication is supposed to cause. The design used by Brodeur (1965) seems to be the most appropriate. Brodeur had three groups of subjects. All three groups received a capsule. Subjects in the first group were told that the capsule was an amphetamine, and the effects of amphetamines were spelled out for them. Subjects in the second group were told that the capsule was a tranquilizer, and the effects of tranquilizers were spelled out for them. Subjects in the third group were told that the capsule was a placebo, that they were control subjects and that the capsule would have no effect on them. Thus all three of these groups consisted of subjects exposed to direct suggestion, and the effects of the contents of the different

direct suggestions could be determined, amphetamine suggestions vs. tranquilizer suggestions vs. placebo suggestions, because all three groups were given direct suggestions and the only factor that was varied was the content of the suggestions.

Brodeur's (1965) study was earlier herein taken to task as lacking a control group, a group receiving the capsule but not receiving communication concerning the effects the capsule was supposed to have. But following the analysis of the situation made in section 4:2:5:2, it is clear that the subjects in such a control group would be operating under either an indirect suggestion condition, or under a condition wherein they were exposed to neither direct suggestion nor to indirect suggestion. In comparing any of his other groups with this control, Brodeur could not have known whether differences observed were due to a change in the type of suggestion (i.e. from direct to indirect suggestion), or to a change in the content of suggestion (i.e. from an expression of expectation that a certain type of behavior would be forthcoming from a subject, to no such expression).

In fact, then, the logical requirements of placebo studies and of studies of suggestion in general may make the construction of an appropriate control group difficult. If one wishes to demonstrate that explicit instructions, or suggestions, are potent, one may have to content oneself with showing that explicit instructions with Content A have an effect different from that of explicit instructions with Content B; but one may have to forego the luxury of demonstrating and being able to interpret any differences obtained between the ef-

fects of explicit instructions with Contents A and B and the effects of no instructions at all, since the control group required for such demonstration and interpretation must of necessity confound type of communication and content of communication.

There is at least one other point to be made about a control group in which subjects receive no communication concerning what is supposed to happen, what they are supposed to experience, and that point is that if subjects respond in an experimental situation in terms of what they believe the experimenter wants (Orne, 1962; Rosenthal, 1966; Rosenthal and Rosnow, 1969), and if many experimental results can be attributed to so-called Rosenthal effects, subjects in such a control group as is under consideration are operating blind, so to speak. This may not mean that there are no cognitive influences on their behavior in the experimental situation, but simply that the experimenter has abdicated control over what those cognitive influences are to be. Uncontrolled factors increase error variance; thus in the present study the use of a subgroup that, if results on the Awareness Questionnaire are any indication, was effectively kept ignorant of experimental purposes, probably contributed heavily to the large within-groups mean square obtained in the originally proposed analysis of variance, the error term against which between-groups differences were tested. A large error term would tend to bury whatever effects of the experimental variables might actually have been operating.

One can therefore conceive of an experimental design that would

possibly have been more effective than that of the present study was in revealing the effects of Abstract Conditioning on explicit suggestion. First, the attempt to separate the effects of the inert medication and the effects of communication of what the inert medication is supposed to do, must be abandoned. Then it should be arranged that all subjects receive direct suggestions. There might be four levels of an Abstract Conditioning variable, as in the present study, but with care being taken to see to it that the Temporal Control level of the Abstract Conditioning variable is truly neutral, as it apparently was not in the present study. In addition, the Positive and Negative Abstract Conditioning levels might be changed to conform more to the procedures used in studies that have shown that Abstract Conditioning does have an effect. Crossing with the Abstract Conditioning variable would be three levels of a Placebo variable: placebo with communication for increased body-sway from pre-trial to post-trial, placebo with communication for decreased body-sway from pre-trial to post-trial, and placebo with communication for no change in body-sway from pre-trial to post-trial. The placebo must be capable of being seen by the subjects as an active drug.

Alternatively, in a like design, one might examine the effect of Instructions, or suggestions, alone, without a placebo. But strictly, the logic of experimental design, considered in conjunction with the well-demonstrated empirical difference between direct and indirect suggestion, forbids statistical separation of the effects of inert medication and communication of what the inert medication is supposed

to do.

Another lesson to be learned from the present study and applicable to future research, is that before any experimental manipulations are carried out in a study, a determination should be made of differences between subgroups to which subjects have supposedly been randonly assigned. Adhering to what is believed to be a procedure of random assignment of subjects to groups does not necessarily ensure that assignment is in fact random or that there are no pre-experimental systematic differences between experimental subgroups. In the present endeavor, despite what cannot be faulted as a random assignment procedure, taller, heavier subjects tended to cluster in certain subgroups of the design. Although this near-failure of the randomization process, the reasons for which remain obscure, cannot be blamed for the generally confused nature of the experimental results, nor, for that matter, can be held accountable for what more or less clear findings were extracted from the general confusion, in another study it obviously could cloud results and make interpretation of results difficult.

Finally, it should be reemphasized that the failure of the present study to demonstrate unambiguous results of the sort predicted in the introduction hereto, is probably due not so much to the untenability of the hypotheses on which those predictions were based, as to the fundamental flaw in the experimental design, complicated by such circumstances as that one of the control procedures was far from neutral in its effects, that in the present situation the placebo given ap-

parently did not stand much of a chance of being perceived as an active drug, that there were several outstanding procedural differences between this study and the studies cited herein that suggested that the Placebo variable, the Abstract Conditioning variable, and the Instructions variable should produce clearly discernible results, and that a possibly important personality variable, that of neuroticism and maladjustment vs. nonneuroticism and adjustment, was omitted from consideration. Consequently, it is not felt that the present study constituted a fair test of the abstract conditioning or expectancy arousal hypothesis, or, as far as that goes, of the effects of Placebo or Instructions. Much further research is needed before the abstract conditioning hypothesis can be ruled out. Such research might well start with some such corrected design as was presented above, in which the type of suggestion given, but not the content, is kept consistent across subjects.

Furthermore, the findings that were produced by the present study -- that subjects tended to resist experimenter suggestive influence, that subjects who reported experiencing various types of physical ailments tended to respond to experimenter suggestive influence more readily than those who did not so report, and that some subjects were apparently unable to resist experimenter suggestive influence although they wanted to resist, must be considered in light of the investigations many failings. Particularly striking to the present investigator was the indication that even 10 minutes after a suggestion for increased body-sway was given, there was a tendency for certain subjects

to respond to it. That these findings were extracted at all, considering the experimental design's violation of one of the most fundamental canons of inductive logic, is in itself worthy of note, and implies to this investigator that another, more adequately designed experiment might in fact confirm the statements commonly encountered in the literature of suggestion and suggestibility that suggestion is a powerful variable, and might confirm in addition the notion that arousal of expectation, conceived of as a learning phenomenon, is the key to understanding suggestion and suggestibility.

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Appendix A

FOR FEMALES ONLY

TIME REQUIRED: 45-60 minutes

Subjects will be required to look into a small box to see what is there to be seen. Other phases of the experiment will call for a subject to stand immobile on a platform for brief (5-minute-long) periods of time, and to fill out 4 short questionnaires.

Some subjects will be asked to take a perfectly harmless pharmaceutical agent, in the form of a tiny pill.

IMPORTANT NOTE: Please do not sign up for this investigation if you are regularly taking any medication or drug other than, perhaps, vitamins. Nor will you be able to participate if within the 24 hour period preceding your scheduled time of participation you have taken any medication or drug other than vitamins. Prohibited medications and drugs include alcohol, aspirin, Empirin, Bufferin, Coricidin, Dristan, Allerest, Darvon, Midol, muscle relaxants, tranquilizers of all sorts, energizers (pep-pills or ups) of all sorts, birth-control pills, insulin, etc. The rule is, to be a subject, you must be taking no medication or drug regularly, and you must be medication- and drug-free both at your scheduled time of participation, and for the 24 hour period preceding your scheduled time of participation.

THIS EXPERIMENT HAS BEEN CLEARED WITH THE ASSISTANT DIRECTOR OF STUDENT HEALTH SERVICES.

Appendix B

QUESTIONNAIRE A

1. Do you ever have headaches? Circle YES or NO.

2. If the answer to question #1 is YES, do you take aspirin, Excedrin, Bufferin, Empirin, or any other preparation, commercial or prescribed, which is supposed to have the effect of eliminating the headache? Circle YES or NO.

3. If the answer to question #2 is YES, please rate on the following scale the effectiveness, as you see it, of whatever pain-killing preparation(s) you take -- simply make a check-mark in front of the relevant statement:

___ Always Help

___ Help About 75% of the Time

___ Help About 50% of the Time

___ Help About 25% of the Time

___ Never Help

4. If the answer to question #1 is YES, and the answer to question #2 is NO, is the reason that you take nothing for your headaches that you have found nothing that works for you? Circle YES or NO.

5. Do you ever have an upset stomach? Circle YES or NO.

6. If the answer to question #5 is YES, do you take bicarbonate of soda, Pepto-Bismol, Tums, Alka-Seltzer, Gelusil, or any other preparation, commercial or prescribed, which is supposed to have the effect of eliminating the stomach upset? Circle YES or NO.

7. If the answer to question #6 is YES, please rate on the following scale the effectiveness, as you see it, of whatever stomach preparation(s) you take -- simply make a check-mark in front of the relevant statement:

___ Always Help

___ Help About 75% of the Time

___ Help About 50% of the Time

Appendix B, continued

___ Help About 25% of the Time

___ Never Help

8. If the answer to question #5 is YES, and the answer to question #6 is NO, is the reason that you take nothing for your stomach upsets that you have found nothing that works for you? Circle YES or NO.

9. Do you ever have colds? Circle YES or NO.

10. If the answer to question #9 is YES, do you take vitamin C, Coricidin, Dristan, Super Anahist, or any other preparation, commercial or prescribed, which is supposed to have the effect of alleviating the cold? Circle YES or NO.

11. If the answer to question #10 is YES, please rate on the following scale the effectiveness, as you see it, of whatever cold preparation(s) you take -- simply make a check-mark in front of the relevant statement:

___ Always Help

___ Help About 75% of the Time

___ Help About 50% of the Time

___ Help About 25% of the Time

___ Never Help

12. If the answer to question #9 is YES, and the answer to question #10 is NO, is the reason that you take nothing for your colds that you have found nothing that works for you? Circle YES or NO.

Appendix B, continued

Scoring Key -- Drug Effects Questionnaire

Item Number	Scoring
1, 5, 9	Yes = +1 No = 0
2, 6, 10	Yes = +1 No = 0
3, 7, 11	Always Help = +4
	75% = +3
	50% = +2
	25% = +1
	Never Help = 0
4, 8, 12	Yes = 0 No = +1

Appendix C

QUESTIONNAIRE B

Please rate, on the following scale, how unsteady on your feet you felt the second time you stood on the platform -- simply make a check-mark in front of the relevant statement:

___ Much more unsteady than the first time

___ A little more unsteady than the first time

___ About as steady as the first time

___ A little more steady than the first time

___ Much more steady than the first time

Appendix D

Semantic Differential Instructions

The purpose of this study is to measure meanings of certain things to various people by having them judge them against a series of descriptive scales. In taking this test, please make your judgements on the basis of what these things mean to you. On each page of this booklet you will find a different concept to be judged and beneath it a set of scales. You are to rate the concept on each of these scales in order.

Here is how you are to use these scales: If you feel that the concept at the top of the page is very closely related to one end of the scale, you should place your checkmark as follows:

fair _X_ : ___ : ___ : ___ : ___ : ___ : ___ unfair

OR

fair ___ : ___ : ___ : ___ : ___ : ___ : _X_ unfair

If you feel that the concept is quite closely related to one or the other end of the scale (but not extremely), you should place your checkmark as follows:

fair ___ : _X_ : ___ : ___ : ___ : ___ : ___ unfair

OR

fair ___ : ___ : ___ : ___ : ___ : _X_ : ___ unfair

If the concept seems only slightly related to one side as opposed to the other side (but is not really neutral), then you should check as follows:

fair ___ : ___ : _X_ : ___ : ___ : ___ : ___ unfair

OR

fair ___ : ___ : ___ : ___ : _X_ : ___ : ___ unfair

The direction toward which you check, of course, depends upon which of the two ends of the scale seem most characteristic of the thing you're judging.

If you consider the concept to be neutral on the scale, both sides of the scale equally associated with the concept, or if the scale is com-

pletely irrelevant, unrelated to the concept, then you should place your checkmark in the middle space.

IMPORTANT:
1) Place your checkmarks in the middle of spaces, not on the boundaries.
2) Be sure to check every scale for every concept -- do not omit any.
3) Never put more than one checkmark on a single scale.

Sometimes you may feel as though you've had the same item before on the test. This will not be the case, so do not look back and forth through the test. Make each item a separate and independent judgement. Work at fairly high speed through this test. Do not worry or puzzle over individual items. It is your first impressions, the immediate "feelings" about the items, that we want. On the other hand, please do not be careless, because we want your true impressions.

Appendix D, continued

Sample Semantic Differential rating sheet, with Semantic Differential Factors to which each bipolar scale is relevant

DOCTOR

Large	___	___	___	___	___	___	___	Small
Passive	___	___	___	___	___	___	___	Active
Clean	___	___	___	___	___	___	___	Dirty
Strong	___	___	___	___	___	___	___	Weak
Slow	___	___	___	___	___	___	___	Fast
Light	___	___	___	___	___	___	___	Heavy
Unfair	___	___	___	___	___	___	___	Fair
Hot	___	___	___	___	___	___	___	Cold
Good	___	___	___	___	___	___	___	Bad

Key: 1, 4, 6 -- Potency

2, 5, 8 -- Activity

3, 7, 9 -- Evaluation

Appendix E

QUESTIONNAIRE C

We would like to know how sophisticated or clever a researcher you might be or become, and to that end would appreciate your answering the following questions:

1. Do you think any trickery was involved in this experiment? Circle YES or NO.

2. If the answer to question #1 is YES, please indicate briefly, on the lines following, what you think the trickery was, or when in the experiment it occurred. If you think trickery was involved, but are unable to say what the trickery was or when it occurred, you may write, "I don't know:"

3. Do you have any idea what this experiment was trying to find out, and if so, what was it? Write down briefly in the space remaining on this paper what you think the purpose of the experiment was. If you feel you have no idea, it is perfectly permissible to write down any guess or hunch you might have about the experiment's purpose.

Appendix F

Awareness Questionnaire checklist

Weight			C	C x W
1	1.	STATEMENT THAT BALANCE WAS BEING TESTED	_	____
1	2.	SUSPICION OF TRICKERY, NO RELEVANT ELABORATION	_	____
1	3.	STATEMENT THAT PILL INACTIVE, NO RELEVANT ELABORATION	_	____
1	4.	STATEMENT THAT LIGHT DID NOT FLICKER, NO RELEVANT ELABORATION	_	____
4	5.	STATEMENT THAT WHAT IS BEING INVESTIGATED IS THE EFFECT OF EXTRAEXPERIMENTALLY GAINED ATTITUDES TOWARDS PILLS &/OR PILL-RELATED CONCEPTS, ON RESPONSE TO A PILL	_	____
4	6.	STATEMENT ABOUT PSYCHOLOGICAL EFFECT OF MEDICATION, POTENCY UNSPECIFIED, ON UNSPECIFIED RESPONSES	_	____
5	7.	STATEMENT ABOUT PSYCHOLOGICAL EFFECT OF MEDICATION, POTENCY UNSPECIFIED, ON BALANCE	_	____
6	8.	STATEMENT ABOUT PSYCHOLOGICAL EFFECT OF IMPOTENT MEDICATION ON UNSPECIFIED RESPONSES	_	____
7	9.	STATEMENT ABOUT PSYCHOLOGICAL EFFECT OF IMPOTENT MEDICATION ON BALANCE	_	____
7	10.	STATEMENT THAT SUGGESTIBILITY IS INVOLVED, TARGET RESPONSE UNSPECIFIED	_	____
8	11.	STATEMENT THAT SUGGESTIBILITY IS INVOLVED, TARGET RESPONSE SPECIFIED AS BEING BALANCE	_	____
8	12.	STATEMENT THAT SUGGESTIBILITY IS INVOLVED, TARGET RESPONSE SPECIFIED AS BEING LIGHT HALLUCINATION	_	____
7	13.	STATEMENT THAT E MADE S EXPECT THAT SOME (UNSPECIFIED) RESPONSE WAS GOING TO OCCUR, &/OR THAT SOME (UNSPECIFIED) RESPONSE WAS SUPPOSED TO OCCUR BECAUSE OF EXPECTATION AROUSAL	_	____

Appendix F, continued

Weight			C	C x W
8	14.	STATEMENT THAT E MADE S EXPECT THAT IMBALANCE WAS GOING TO OCCUR, &/OR THAT IMBALANCE WAS SUPPOSED TO OCCUR BECAUSE OF EXPECTATION AROUSAL	—	———
8	15.	STATEMENT THAT E MADE S EXPECT THAT LIGHT FLICKER WAS GOING TO OCCUR, &/OR THAT LIGHT FLICKER WAS SUPPOSED TO OCCUR BECAUSE OF EXPECTATION AROUSAL	—	———
10	16.	STATEMENT THAT WHAT IS BEING INVESTIGATED IS THE EFFECT OF E'S TRUTH-TELLING ON S'S RESPONSE TO SUGGESTION ORIGINATING WITH E, WHETHER THAT SUGGESTION IS MEDIATED BY A PILL OR NOT	—	———