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THE EFFECTS OF DRUGS ON HUMAN PERFORMANCE:

The Development of Analytical Methods and Tests of Basic Human Abilities

TECHNICAL REPORT No. 2

by

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DIGEST

This report represents a part of the continuing effort to develop a comprehensive battery of basic ability tests to be used in the assessment of drug effects on human performance. Specifically, the present report describes the methods of analysis devised to cope with some of the difficult problems produced by the nature of the performance changes caused by chemical agents. The methods were applied to a study previously reported (Study I) as well as to a new study (Study II).

Study I employed a battery of nine ability tests while Study II used seven others. Both studies included control and experimental groups tested repeatedly over a two-day period. The first day served simply as a training period for all subjects The data from the second day reflected the effects of the training in the control group data, while the further effects of a 12 μg/kg IM injection of scopolamine were reflected in the experimental group data.

Information from the control subjects was used to establish the expected levels of performance for undrugged subjects which should be used as a frame of reference in determining if the experimental group had been influenced by the drug. It also provided estimates of test reliabilities. Experimental group data then indicated the extent to which the drug altered the expected levels of performance.

One major result of the analyses was a series of recommended changes to improve the nature of the data obtained from each of the tests. A second major finding was that of a consistent subject-by-treatment interaction in the experimental group data for each of the abilities tested. This latter finding led to recommendations for improving the experimental design currently being employed and also suggested the direction which correlational or multivariate analyses must take in order to adequately summarize the nature of the interrelationships among the data being gathered.

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THE EFFECTS OF DRUGS ON HUMAN PERFORMANCE:

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CHAPTER I

INTRODUCTION

1.0 Background and General Objectives

This report is one of a series concerned with the assessment of the effects of chemical agents on human performance. The research is being carried out by the American Institutes for Research for the U. S. Army's Edgewood Arsenal Research Laboratories (EARL).[1] The program has as its major goals the development of a comprehensive testing facility comprised of basic ability tests and the validation of predictions made from these laboratory results to performance on complex operational tasks.

Technical Report No. 1 (Elkin, Freedle, Van Cott & Fleishman, 1965a) covered the general results of the first study (Study I) to be run under this program. This second report re-examines the same data, but uses them to develop specific analytical methods by means of which more detailed, relevant sources of information could be extracted from the data. This report, then, goes into more detail than was possible in the first report, and it applies the methods developed for analysis to the data from a new study (Study II).

The rationale for the overall project is presented fully in the First Annual Report (Elkin, Fleishman, Van Cott, Horowitz & Freedle, 1965b) while the background and procedures for Study I are covered in Technical Report No. 1 (Elkin, et al., 1965a). The tests chosen for inclusion in

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1. Formerly called the Chemical Research and Development Laboratories (CRDL).

Studies I and II were drawn, for the most part, from the results of factor analytic studies of the many specific tests available for the assessment of broad categories of human behavior. They are intended, ideally, to represent the "best" or most basic tests of those human abilities which have, thus far, been identified with a reasonable degree of consistency. As a matter of convenience, the total realm of human abilities has been divided into five broad categories: physical, psychomotor, sensory-perceptual, cognitive, and social. Tests within these categories have been drawn primarily from the work of Fleishman (1960, 1962, 1964) and French, Ekstrom, & Price (1963).

Once a fundamental set, or battery, of tests of these categories has been developed, the battery will be used to assess and compare various chemical agents of interest. Through a knowledge of the relationship of these basic abilities to specific military operations, the project is intended to provide a reliable and controlled means of predicting from drug effects on these laboratory-administered tests to the effects of chemical agents on the performance of military tasks in the field.

2.0 Purpose of This Report

For the purposes of this report an explicit statistical model of the experiments was constructed and used to analyze the data from Studies I and II. Overall score distributions were examined and consideration was given to sources of variation other than those related solely to the drug administered. Where Technical Report No. 1 examined only differences between control and experimental group means, attention here was directed toward effects on individual subjects, isolating subject-by-treatment interactions, effects of replication of tests within test sessions, distinctions between experimental and sampling error, and estimates of test-retest reliabilities of the tests employed. These analyses were then used

as a basis for the study of possible need for test modifications and improvement of the experimental design.

3.0 General Experimental Procedure

Both studies (I and II) used, as a standard agent, a 12 µg/kg intramuscular (IM) injection of scopolamine for the experimental groups and a placebo injection for the control groups, and then examined the effects of these treatments on performance over time. Study I included nine tests while Study II included seven others. These are specifically identified in Table 1 in terms of the tests used and the basic abilities measured. Each of these tests was implemented, as far as possible, in the manner in which they were developed and standardized, but the difficulties of using them with drugged subjects forced some modification in the procedures.

The specific procedures utilized in each experimental session for the administration of each test used in Study I were reported in Technical Report No. 1 (Elkin, et al., 1965a). Those for the tests used in Study II are included here as Appendix A.

The basic structure for the experimental design used in both studies is illustrated in Figure 1. All subjects were treated alike on the first day of testing, each going through an orientation (OR) session covering each test in the battery, and then being tested four additional times (B_1 to B_4) throughout the day at times roughly equivalent to those which were used on the second day of testing. On the second day, all subjects were given a pre-drug (PD) test session at about 8:30 a.m. The subjects assigned to the experimental group wer given a 12 µg/kg IM injection of scopolamine at approximately 9:15 a.m., while all control subjects received an inactive or placebo injection at that time. Then all subjects were tested at five time points (D_1 to D_5) after the injections. For Study I these time points were 45, 135, 255, 375, and 525 minutes after it. Thus, in the experimental pattern of Figure 1, Quadrants A and C represent training session data while Quadrants B and D represent control and drug treatment data respectively.

TABLE I

TESTS USED IN STUDIES I AND II AND THE BASIC ABILITIES MEASURED BY THEM

Study	Test	Ability Measured
I	Orthorater	Far Visual Acuity
	Orthorater	Near Visual Acuity
	Minn. Manipulation	Manual Dexterity
	Hand Grip	Static Strength
	Balance A	Gross Body Equilibrium
	Addition	Number Facility
	Auditory Number Span	Short Term Memory
	Empty Interval Production	Time Estimation
	Simple Visual Reaction Time	Simple Reaction Time
II	Track Tracing	Arm-Hand Steadiness
	Broad Jump	Explosive Strength
	Block Manipulation	Manual Dexterity
	Purdue Pegboard	Finger Dexterity
	Two-Hand Coordination	Multi-limb Coordination
	Bend, Twist, & Touch	Dynamic Flexibility
	Pull-ups	Dynamic Strength

Treatment Group	Subj.	Rep.	TEST SESSION											
			OR	B 1	B 2	B 3	B 4	PD	D 1	D 2	D 3	D 4	D 5	
CONTROL	1	1												
		2												
	2	1												
		2												
	•	•												
	•	•												
	•	•												
	N_c	1												
		2												
EXPERIMENTAL	1	1												
		2												
	2	1												
		2												
	•	•												
	•	•												
	•	•												
	N_e	1												
		2												

A B C D

Figure 1. Structure of the experimental design used in Studies I and II.

The training session data are not of particular interest here, even though the sessions do have an important function in the experimental design. Theoretically, basic abilities should be relatively stable characteristics of an individual, and measures of these should not show significant changes over short periods of time. However, no test can measure an ability as such. A test can only provide the subject with a means of producing what is presumed to be a measurable response related to a given ability. On the basis of the response, the tester presumes the existence of the ability, and the response measure is treated as a measure of the amount of the ability possessed. Where the mode of response provided by the test involves generally unpracticed activities (this is especially characteristic of psychomotor tests), initial performances will reflect a high degree of instability and often will also exhibit practice effects or learning trends. These do not imply a learning of the ability, but rather, an increasing skill in the handling of the apparatus. These early sources of variability are, for the most part, adequately eliminated by the first day of training, and this permits a much clearer and more sensitive assessment of the influence of the drug on performance during the second day. The continued assessment of the control group on the second day provides a check on the assumption that performance has gen rally stabilized by that time, and that it will change only if the drug causes it to change.

It should be pointed out that, as a matter of practical necessity, subjects could not always be assigned randomly to control or experimental groups even though the method of analysis to be employed would ordinarily demand this. The control group was, for the most part, limited to those subjects who, for medical reasons, were classified as unsuitable for the administration of drugs. The experimental group, of course, only contains those who were medically acceptable. The limited number of those available for the latter group generally precluded assigning them to a non-drug condition. To what extent this would introduce a bias in the interpretation

of results that are dependent upon control group data is not, at this time, known. There would, a priori, appear to be no necessary reason for assuming that one group should differ from the other in the basic abilities being assessed but, of course, the possibility does exist. It should become evident in the analyses to follow that the data interpretations made were not contingent upon assuming equality in the absolute level of performance of the two groups, nor on the equality of their variances, but rather, on the assumed comparability in the trends over time as a function of the presence or absence of learning, fatigue, or other factors unrelated to drug effects as such. The adequacy of this assumption may be tested relatively easily in future pilot studies presently being devised for the development of new tests. Subjects may be similarly classified and tested, but neither group will be given a drug.

Within each test session subjects were tested at least twice on most of the variables assessed in these two studies, i.e., the tests were replicated (R_1 and R_2) at each test session. This was essential in order to obtain a means of testing whether or not the subject-by-time-period interaction represents only sampling error for the variable in question, or an additional effect in and of itself.

4.0 Subjects

The subjects employed in Studies I and II were selected from a pool of enlisted men, military personnel who volunteered for participation in this program. Each of these was put through an extensive medical and psychiatric screening, and a subject was permitted to receive a drug treatment only in those cases where no evidence of risk in being treated with psychochemicals was indicated. The subjects, however, were not informed of the results of the screening process. All of the subjects used are described, in terms of some basic biographical data, in the First Annual Report for this project (Elkin, Fleishman, Van Cott, Horowitz, & Freedle, 1965b).

CHAPTER II

DEVELOPMENT OF THE BASIS FOR DATA ANALYSIS

1.0 General Considerations

R. W. Russell (1964), in the Annual Review of Psychology, pointed out that faulty or inefficient research designs have flooded the literature with reports that confused, rather than contributed to, the state of knowledge in psychopharmacology. He is not alone in this opinion; other reviewers in this inherently interdisciplinary field reflecting a clinical, experimental, medical, or pharmacological interest have expressed similar attitudes (Beecher, 1959; Modell, 1959; Trouton & Eysenck, 1961; Davis, 1965.) One obvious cause of the problem is that few researchers in the field are adequately equipped to recognize and to control all of the variables in all of the fields concerned. The psychologist, emphasizing the behavior to be measured, may fail to consider sufficiently stringent pharmaceutical controls or fail to appreciate the complexity of the pharmaceutical action of the agent in the analysis of his results. The pharmacologist, on the other hand, may slight the controls required in the adequate measurement of a behavioral response variable. As Hebb (Harlow & Woolsey, 1958) emphasized so well, in a chapter entitled, "Alice in Wonderland, or Psychology Among the Biological Sciences," there is an obvious need for the development of clear communication among the various fields concerned.

Added to the above, when concern centers on the effects of chemical agents in human subjects, are the restrictions imposed by ethical and safety requirements on the number of subjects available and the treatments to which they may be subjected. Quite often these considerations have led to necessary or practical (?) compromises in the implementation of fundatmental principles of experimental design.

Nash (1960) drew up a list of requirements which are considered essential to sound experimentation in the field. He pointed out that there is a need:

(1) to eliminate or segregate the effects of any factor which may introduce a bias into the experiment.

(2) to avoid confounding drug effects with the effects of factors such as learning.

(3) to describe fully the nature of the population and the representativeness of the sample studied.

(4) to employ a randomizing procedure in the assignment of subjects to treatments.

(5) to include a minimum of two observations for each dosage form in order to obtain an estimte of experimental error.

(6) to satisfy the theoretical assumptions underlying the statistical procedures to be used in analyzing the data obtained.

These requirements, slanted mainly toward analysis of the response variable, should be augmented by a list of stimulus and organismic concerns as well (see, e.g., Modell, 1959; Trouton & Eysenck, 1961). It should be emphasized that the controls suggested in these review articles are not ideals; rather, they are fundamental requirements of any study which is intended to permit a meaningful interpretation of results. Granted that considerations can arise requiring the elimination of one or more of these controls from a design, if such an elimination renders the data gathered suspect or ambiguous, it might be more practical not to do the study at all. It is possible to choose a compromised approach to anything; but having done this, it is not possible to avoid the consequences of the compromise.

As an absolute minimum, it is incumbent upon the investigator to make such compromises clear in the reporting of his results. Further, he should, where it is possible, investigate the implications of his compromises to de-

termine the extent to which his results may be affected. An example of this is illustrated in the General Experimental Procedure given in the first chapter of this report (p. 3). There it was indicated that the manner of assignment of subjects to treatments violated the requirement, as listed by Nash, that this assignment be random. It was assumed that this would not destroy the utility of the control group for satisfying Nash's second requirement. A means of testing the validity of this assumption was offered so that it can and will be tested and reported on in the near future.

It is further implied rather than explicitly stated in the experimental design, which is concerned primarily with adequate control of the response variable, that relevant stimulus (drug) and organismic variables are either being held constant or caused to vary randomly with respect to each of the treatments or time points being observed. The administration of the agent (scopolamine) as a function of body weight, µg/kg, is assumed to provide an equally effective dose of the agent for all subjects. In spite of the screening process, organismic variables of a physiological or biochemical nature are presumed to be distributed randomly between groups and among subjects within groups. The same is assumed to be true for psychological variables such as personality, motivation, etc. Equal amounts of "past experience" with the response variables are provided to all subjects in the first day of training. Basically these assumptions must be true if a clear interpretation of the results is to be accomplished. From a practical standpoint, the perfect experiment will never be accomplished, but in an area as tenuous as psychopharmacology, detailed consideration of the implications of a given experimental design would seem to be essential.

2.0 Statistical Model for the Analyses

It would be a relatively straight-forward procedure to produce a mathematical model which would encompass the entire experimental structure illustrated in Figure 1 (p. 5), but an attempt to impose a statistical model on this would encounter several serious problems. The statistical model required

to cover the general experimental design, for a parametric analysis, would be at least a three-factor (subjects, groups, and sessions) design with repeated measures on one of the factors (subjects within groups). Such a design is illustrated by Winer (1962, Pp. 302 ff.). However, it was clear from these studies, as well as others conducted in psychopharmacology, that drug treatment seriously alters the variance as well as the mean of a subject's performance so that the drug data (Quadrant D) differed markedly from the remainder of the data gathered. While the F tests (significance tests) of analyses of variance which do not include the use of repeated measures (the testing of the same subjects at each of several time points) are relatively unaffected by some degree of heterogeneity of variance, a repeated measures design is far more susceptible to biasing of its results under such conditions (Winer, 1962; Box, 1953). A repeated measures design must assume homogeniety of covariance as well as variance terms, and it is sensitive to departures from this assumption.

In other words, even when concern is limited to Quadrants B and D for the assessment of drug effects, the combining of control and test subjects into a single analysis would make significance tests from such an analysis almost meaningless and often misleading. The problem is further compounded by the fact that the number of subjects differed for the two groups. This in itself would lead to computational and interpretive problems (Tsao, 1946). Thus, the broadest practical statistical model that could be considered was one covering any one quadrant of the data. This model is outlined in Table 2.

This model treats subjects as a random factor, and sessions or time periods as a fixed factor[2]. Thus, the subjects were considered to be a random sample from a population of such subjects which might be obtained from similar screening processes, but they are not to be considered as a random sample of military personnel in general. The replication effect was also treated as a fixed factor. The specific application of the model to Quadrants B

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2. For a definition or explanation of the terms "fixed" and "random" as used in this context, see Winer, 1962, Pp. 143-4, or Snedecor, 1956, Pp. 338 ff.

TABLE 2

MATHEMATICAL AND STATISTICAL MODELS FOR THE ANALYSIS OF ANY ONE QUADRANT OF DATA FROM STUDIES I AND II

1. Mathematical Model:

$$X_{ijk} = \bar{X}... + A_i + B_j + AB_{ij} + R_k + E_{ijk}$$

Where $\bar{X}...$ = overall mean of the data.

A_i = main effect associated with Subject "i", and $i = 1, 2, \ldots, a$.

B_j = main effect associated with test session "j", and $j = 1, 2, \ldots, b$.

AB_{ij} = the interaction effect of A_i at B_j, a measure of the extent to which a given observation departs from a simple addition of the two main effects. Constitutes experimental error.

R_k = effect associated with replication "k", and $k = 1, 2, \ldots, r$.

E_{ijk} = sampling error, uncontrolled error variance.

2. Statistical Model

Source of Variation	Degrees of Freedom	Expected Mean Squares
Subjects (A)	(a-1)	$\sigma_\varepsilon^2 + rb\sigma_A^2$
Sessions (B)	(b-1)	$\sigma_\varepsilon^2 + r\sigma_{AB}^2 + ra\sum_j^b B_j^2/(b-1)$
Interaction (AB)	(a-1)(b-1)	$\sigma_\varepsilon^2 + r\sigma_{AB}^2$
Replication (R)	(r-1)	$\sigma_\varepsilon^2 + ab\sum_k^r R_k^2/(r-1)$
Error (E)	(ab-1)(r-1)	σ_ε^2
Total (T)	(abr-1)	

and D data is illustrated with respect to most of the variables used in Studies I and II in the discussion of the results in the next chapter.

3.0 Sequence of Analyses

For each dependent variable, the data from the control group were first examined separately. An analysis of variance was performed on the data from the PD to the D_5 test session (Quadrant B). This was done to determine if the variable in question showed any trend or change from session to session simply as a function of repeated testing, and then to determine if the PD session data could be considered as a representative measure of subject performance at each of the subsequent test sessions. Where no general change or trend was noted, specific planned comparisons were carried out to establish the legitimacy of assuming that the one data point obtained at the PD session could serve as a point of comparison for all subsequent sessions.[3]

The logic applied was that if no changes were found in the control data, stability of performance for that variable would be demonstrated; and, if the PD session data did not differ from any one of the subsequent session means for the control group, a similar pattern should logically hold for the experimental group unless the administration of the drug altered its performance. Then, the determination of the presence or absence of a drug effect, for data meeting these tests, could be made simply with respect to the PD data of the experimental subjects. In other words, the control group data would not be required during subsequent analyses of the experimental group data.

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[3] Computational procedures for these planned comparisons (i.e., A Priori as opposed to Post Hoc comparisons) may be found in any standard reference for experimental design such as Winer (1962) or Bennett & Franklin (1954). It should be noted here that the series of comparisons for each successive mean against the PD mean represents a non-orthogonal group of comparisons so that interpretations of the probability of occurrence of the resulting F ratios must be treated conservatively. If more exact results were required, the series of coefficients for the contrast effect equations could be intercorrelated and the resulting product matrix used to orthogonalize the estimates of effects.

On the other hand, if significant changes were found in the control group data, or if the PD session data were not representative of each of the subsequent test sessions for the control group, this would necessitate the assumption of similar changes in the performance of the experimental group independent of or in addition to those which might be produced by the administration of the drug. In that case, the pattern of changes noted in the performance of the control group would have to be considered as more representative of the undrugged performance over time of the experimental group than would the PD data of that group, or any of its earlier data. Thus, the determination of the presence or absence of a drug effect on a variable which independently manifests changes over time would be made with respect to the difference between the drugged subjects' performance and the control group means at each point in time.

There is a general problem which must be noted that results from the discovery of any significant change in performance by the control group. There were several instances where this did occur. In accordance with the experimental design used, all of the control subjects were treated as placebo subjects, i.e., there were no zero level or pure control subjects included. Each control subject was given a non-active injection. If a placebo effect existed as a result of this, it would not be identifiable as such in the data. If an apparent learning curve or fatigue curve, especially the latter, were to appear, it could as easily represent simply those aspects of performance or it could represent some aspect of the placebo phenomenon. Where learning curves level off on the second day of testing, this again could represent subjects approaching an asymptote, or the suppression of further learning by a placebo effect. An interaction between subjects and sessions in the control group creates serious problems for interpreting the experimental data; and yet, this would be a likely result of a placebo effect, appearing in some subjects but not in all of them. Yet, none of these possible effects (instances of which appear to have occurred in the data) is testable in the design as it was implemented.

The zero level was omitted from the control group partly because of the limited number of subjects available, but more because it was assumed that the placebo phenomenon is probably negligible with reasonably knowledgable subjects, and with drug agents possessing a potent aura and obvious effects. Still, having injected these subjects, the possibility of a placebo reaction cannot be ruled out. It then follows that the assessment of control group data, where significant changes were noted, becomes subject to ambiguity in its interpretation. The use to which the control group data is to be put in subsequent analyses of the experimental group data becomes uncertain. A change in the present procedure has been recommended, at least until normative data on a reasonably large number of untreated subjects becomes available.

If the possible significance of a placebo effect is to be denied in these studies, it is then recommended that the non-active injection be eliminated so that these subjects can be assessed unambiguously as a control group for the dependent variable under investigation. As a consequence of the present situation, tests for a placebo effect could not be made. For the most part, where control group data showed significant changes over time, these have been ascribed to learning or fatigue or some other possible relevant source related to the variable in question. Control group data were generally treated as representing the typical performance over time to be expected in the repeated testing of that variable. They were used, then, as a frame of reference in determining the presence or absence of a drug effect in the experimental group data.

It is recognized that the elimination of the placebo treatment from the control group confounds this possible mode of response with the true drug response in the experimental group. Howevever, in these studies, interest is in determining the number of men whose performance deteriorates as a function of exposure to a chemical agent, so it would appear preferable to include possible placebo reactors in experimental group variation rather than

lose sight of a necessary frame of reference in judging the presence of any kind of response. Again, the implication of the basic assumption utilized in this aspect of the design is testable, and, if feasible, should be investigated as soon as possible if the basic experimental design remains as it is.

CHAPTER III

RESULTS

1.0 Study I

The first study included tests of far and near visual acuity, manual dexterity, static strength, gross body equilibrium, number facility, short term memory, time estimation, and reaction time. Each test was administered to 15 subjects according to the schedule given in the first chapter of this report. Four subjects served as placebo controls and 11 as test or experimental subjects. The former received an inactive injection and the latter received the 12 µg/kg IM injection of scopolamine 45 minutes after the predrug (PD) session on the morning of the second day. Following this, all subjects were tested at 45, 135, 255, 375, and 525 minutes after injection.

The apparatus and procedure used in the administration of each of the tests are described fully in Technical Report No. 1 (Elkin, et al., 1965a). The basic data obtained during the second day of testing are included here in Appendix B, Tables B-1.1 to B-1.10, showing the raw data rather than the session means as was done in Technical Report No. 1. A detailed analysis of performance on each of these tests is presented below.

1.1 Far Visual Acuity

The stimulus level data obtained from the Orthorater which was used to test this ability are recorded in Table B-1.1 of Appendix B. The data were analyzed in this form rather than in terms of the corresponding visual angles, as was done in Technical Report No. 1, because it became apparent in the application of analysis of variance to the data that transformation of the data to visual angles induced a radical change in the "within-cell" or error variance as a function of changes in mean level of performance, i.e., it made means and variances dependent. This dependence occurs because the subjects, insofar as they vary at all between replications within a test session, generally did so only between two neighboring stimuli in the series. This difference

of one stimulus level at a high level of performance, e. g., 11 and 12, represents a within-cell variance of .0032 minutes of arc in terms of visual angle; while at a low level, e. g., 3 and 4, it represents a variance of .34 minutes of arc. Thus, there resulted an inverse proportionality between means and variances. In terms of Orthorater units, the variances were equal and independent of mean level of performance. Such independence is required in the use of analysis of variance.

It may further be noted that the stimulus level numbers represent more than just an ordered series of numbers. Actually, the numbers that are assigned to each level correspond to ten times the visual acuity required for the perception of the test target at that level. Visual acuity is, in turn, simply the reciprocal of the visual angle subtended by the test target under the specified viewing conditions. Thus, the stimulus series 1, 2, 3, . . . would in turn be .1, .2, .3, . . . in terms of visual acuity and 10, 5, 3.33, . . . in terms of visual angle.

1.1.1 Analysis of Control Group Data. The analysis of variance of the control group data on far visual acuity, shown in Table 3, indicated first that there was no significant interaction between subjects and test sessions, i.e., the subjects responded in a consistent manner with respect to each other across all six test sessions. Secondly, it indicated that there was no significant difference among the six test session means and no difference between the first and second administration of the test for all of the test sessions (i.e., no replication effect). Following this, specific comparisons of the pre-drug (PD) session mean with each of the subsequent test session means indicated that data obtained from the PD session could be treated as representative of performance in each subsequent session.

Since the control group data represented simply repeated testing on the same variable from session to session, these data were also used to estimate the test-retest reliability of the far visual acuity measures. Table 3 shows that this reliability is estimated as being .933, well within the accepted range for testing and predictive purposes. It can be noted, however, in the Appendix

TABLE 3

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR

FAR VISUAL ACUITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	3	18.73	6.24		
Sessions (B)	5	.69	.14	1.56	>.05
Interaction (AB)	15	1.40	.09	.64	>.05
Replications (R)	1	.19	.19	1.36	>.05
Error (E)	23	3.31	.14		
Total (T)	47	24.31			

Test-Retest Reliability: Across all Sessions = .933

For an Average Session = .919

TABLE 4

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR

FAR VISUAL ACUITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	10	527.51	52.75		
Sessions (B)	5	145.82	29.16	9.35	<.01
Interaction (AB)	50	155.85	3.12	4.16	<.01
Replications (R)	1	.03	.03	.04	>.05
Error (E)	65	48.97	.75		
Total (T)	131	878.18			

table, that the ceiling on the test, a level of 12, was insufficient for work with better than normal subjects. This was why Subject 9, for example, showed no variance in performance throughout the test sessions. This would indicate that, with an unrestrictive ceiling, this estimate of reliability would be somewhat lower than it is. The restrictive ceiling, as found here, would also serve to underestimate error variance in the preceding analysis of variance. Since this would tend to enhance possible significant F ratios, and the above F's were still insignificant, it was not considered critical in that case.

The correlation discussed above, specifically an intra-class correlation coefficient, is equivalent to the average of all possible correlations among pairs of sessions data, and estimates the reliability of a single estimate of performance based on six administrations of the test (with the mean of two replications constituting one administration). A second intra-class correlation of .919 is reported; this represents the average reliability of estimates made in any one test administration.[4] Generally, then, if six determinations of the subject's performance are made, the reliability of his estimated level of performance is .933. If only one determination is made, the reliability is .919. It is this latter figure which is relevant to those cases where the single determination of non-drug performance level taken at the PD session is used as a frame of reference in analyzing the experimental group data.

The above data indicate that far visual acuity can be assessed reliably at the one PD session and that the data derived are stable estimates of subsequent performance. The determination of a drug effect can then be made by comparing the performance of the experimental group after drug administration with its own performance in the PD session. It should not be overlooked, however, that the control group is composed of only four subjects. In gen-

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4. In terms of the analysis of variance, overall reliability = $MS_A / MS_A + (b - 1) MS_{AB}$ and average session reliability = $MS_A - MS_{AB} / MS_A + (b - 1) MS_{AB}$. A general discussion of test reliability in these terms may be found in Burt (1955) and Mahmoud (1955).

eral statistical practice, a minimum of at least 50 cases is recommended for the estimation of a correlation coefficient.

1.1.2 Analysis of Experimental Group Data. The next analysis concerned itself directly with the experimental group. The resulting analysis of variance, reported in Table 4, indicated that the drug caused significant changes in performance across the various test sessions, and it also indicated that this effect differed from subject to subject. This latter aspect, a significant interaction between subjects and sessions, precludes the possibility of offering a clear, general statement about the effect of the drug on far visual acuity. It suggested that an examination of each subject individually would more clearly illustrate the effect of the drug on this variable because the drug did not affect the group as a group. The real question then becomes who in the group was affected or to what extent did individuals differ in degree of response to the agent.

To answer this, a series of analyses of the data of each subject considered separately was carried out. Computatin of a replicate effect was omitted from these analyses since neither the control group data nor the general analysis of the experimental group data indicated that it was a relevant consideration for this variable. The results, presented in Table 5, showed that only Subjects 10, 13, and 14 reacted strongly, (F ratios significant at or beyond the .01 level); while Subjects 11, 12, and 19 showed no significant reactions ($P(F) > .05$). The remainder of the subjects showed only slight reactions on this variable ($.05 > P(F) > .01$). In the absence of any fixed criterion level of performance, the data throughout this report could be interpreted only in these conventional statistical terms. A more directly meaningful approach to assessment will be possible when militarily relevant levels of performance are available for the abilities being tested. Further, with objective criteria it will be possible to examine the power or sensitivity of the statistical tests with respect to detecting changes in performance which have a practical effect on military tasks.

TABLE 5

RESULTS OF INDIVIDUAL ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR FAR VISUAL ACUITY

Subj.	Mean Sq. Variance Sessions	Error	F Ratio	Session Means PD	D_1	D_2	D_3	D_4	D_5
10	17.9	1.25	14.32**	10.5	5.5**	4.5**	3.0**	4.0**	9.0
11	1.2	.67	1.79	11.5	10.5	9.5*	11.0	11.0	11.5
12	.9	.42	2.14	6.5	5.5	4.5*	5.5	5.5	5.0
13	2.0	.17	11.76**	10.0	8.0**	7.0**	8.5**	8.0**	8.5**
14	13.0	1.00	13.00**	10.5	10.0	4.0**	5.5**	7.5*	8.5
17	3.0	.50	6.00*	7.0	4.0**	5.0*	6.5	6.5	7.0
18	6.3	.92	6.85*	11.5	8.5*	7.0**	11.0	10.5	11.0
19	1.0	.50	2.00	11.5	11.0	9.5*	11.0	11.0	11.0
20	4.2	1.00	4.20	12.0	9.5*	7.5**	10.0	9.5*	9.5*
21	10.8	1.67	6.47*	8.0	5.5	3.5*	3.5*	8.0	8.5
23	.5	.08	6.25*	10.0	9.5	10.0	10.0	11.0	10.0
Per Cent Affected (< .01 level) =					27.3	45.5	27.3	18.2	9.1

**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD

Detailed examination of the analysis in Table 5 can reveal some of the difficulties which are encountered in dealing with these results. The data for Subject 21, for example, were characterized by a somewhat large error term, probably obscuring a strong reaction, while those of Subject 23 were characterized by an unusually small error term creating the impression of a slight reaction where one might not really exist. Actually, in the case of Subject 23, the significant result was related to the fact that the D_4 session mean appeared to be significantly higher than the PD mean. These non-statistical impressions were based on an examination of the means and variances of each subject and represent examples of the possible effects of random sampling fluctuations which can cause difficulties in interpretations of the data when dealing with small samples.

In order to specifically locate the time points at which each subject was affected, planned comparisons were carried out comparing the PD session mean of each subject with each of his subsequent drug session means. Table 5 indicates those drug session means that were significantly lower than the PD level of performance. (Again, the probability levels of these series of comparisons are not to be strictly interpreted. See Footnote 3, p. 13).

It should be evident from the pattern of reactions displayed in the table that the subjects differed in the strength of the drug effect displayed as well as in the time of the onset of the effect, its duration, and the time of recovery of normal functioning. The maximum effect on Subject 17 occurred at D_1, but at D_2 for Subject 14, and at D_3 for Subject 10. These data indicate how poor a fit would be achieved if the group means at each session were to be used to represent the overall or unqualified effect of the drug on far visual acuity.

Perhaps a better approach to summarizing the overall effect of the drug would be in terms of the number of men who were seriously affected ($<.01$) at each of the time points tested. A graph of the relation between time since drug administration and percentage of men affected is presented for

this variable and most of the others employed in Study I in the summary section as Figure 2, p. 55. It is not intended that this concept of "per cent men affected" should be treated as equivalent to that of incapacitation. The latter should relate to the inability of a man to perform some militarily relevant task. The statistical criterion of affect used here may be more or less stringent than that required for specific tasks, showing, e.g., too great an affect for foot soldiers and too little for artillery spotters. This specificity of criterion to task cuts across the range of partial drug effects between "no effect" and "total incapacitation." For any given criterion, it would be simple to apply this same approach to per cent affected or per cent casualties.

1.1.3 Critique of the Far Visaul Acuity Test. In general, the Orthorater provided precise, reliable estimates of far visual acuity, and sensitively reflected changes in this variable which occurred under the influence of scopolamine. However, the limited ceiling on the test, that makes it inadequate for work with better than average subjects, creates a serious problem. This, of course, is a characteristic of many screening devices which are developed solely for the detection of poorer than normal or average performance. Such instruments are not intended to be used as experimental instruments to cover the whole range of vision. Because of this limitation, plans are presently underway for the development and implementation of a more flexible visual acuity testing apparatus to replace the Orthorater.

1.2 Near Visual Acuity

The Orthorater data obtained for this variable are presented in Table B-1.2 of Appendix B. Again, these are given in terms of Orthorater units as was done for the far visual acuity data.

1.2.1 Analysis of Control Group Data. Similar to the findings for far visual acuity, the analysis of variance of the control group data for near acuity, presented in Table 6, showed no interaction, i.e., consistency of performance of subjects with respect to each other across the several

TABLE 6

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR

NEAR VISUAL ACUITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	3	56.17	18.72		
Sessions (B)	5	1.50	.30	1.87	>.05
Interaction (AB)	15	2.33	.16	2.00	>.05
Replication (R)	1	.08	.08	1.00	>.05
Error (E)	23	1.92	.08		
Total (T)	47	62.00			

Test-Retest Reliability: Across all Sessions = .959

For an Average Session = .951

TABLE 7

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR

NEAR VISUAL ACUITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	10	789.97	79.00		
Sessions (B)	5	572.15	114.43	15.94	<.01
Interaction (AB)	50	358.88	7.18	7.80	<.01
Replications (R)	1	3.03	3.03	3.29	>.05
Error (E)	65	59.94	.92		
Total (T)	131	1783.97			

sessions, no difference among the session means, and no effect due to replicating the test. The overall reliability of the data was .959, while the single session reliability was .951. Specific comparisons of the PD mean with each of the subsequent session means demonstrated that the PD data could be used as a frame of reference in analyzing the experimental group data.

1.2.2 Analysis of Experimental Group Data. The results of an analysis of the experimental group data for near visual acuity were similar to those for far acuity. The data demonstrated a significant interaction between subjects and sessions as well as a significant difference among the session means. The replication effect was negligible. In general, a comparison of F ratios would suggest that the effect of the drug on near acuity was stronger than it was on far acuity. This is further supported by the graphs on p. 55.

The presence of an interaction effect again indicated that a clear analysis of the drug effect would require examination of each subject's data separately. The results of this are presented in Table 8. Comparison of this table with Table 5 indicates the difference in the severity of effect for the two abilities. Where only three subjects showed severe effects on far acuity, eight subjects were affected on near acuity. The pattern of individual comparisons between PD and drug session means again suggests the difficulty of trying to develop a general model of the drug effect which would hold for all subjects. However, the distinction between affected and unaffected subjects is drawn more clearly with this variable. Three of the subjects showed no significant change in performance while the remaining eight showed changes which were significant at or beyond the .01 level.

1.2.3 Critique of the Near Visual Acuity Test. Since the same apparatus, the Orthorater, was used to assess this ability as was used for the test of far acuity, the comments made in Section 1.1.3 apply here as well. An examination of the Appendix data indicates that the restrictive ceiling on the test was even more in evidence when it was used to assess near acuity.

TABLE 8

RESULTS OF INDIVIDUAL ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR NEAR VISUAL ACUITY

Subj.	Mean Sq. Variance Sessions	Error	F Ratio	Session Means PD	D_1	D_2	D_3	D_4	D_5
10	25.10	.58	43.28**	10.5	3.0**	2.0**	1.5**	1.0**	2.5**
11	2.15	.58	3.71	10.5	11.0	10.5	9.0	12.0	11.5
12	7.90	.25	31.60**	7.0	6.5	3.0**	2.0**	3.5**	4.5*
13	7.70	.42	18.33**	11.0	8.0*	6.0**	7.5**	6.0**	6.0**
14	40.35	.08	504.37**	12.0	8.5**	1.0**	1.0**	3.0**	3.0**
17	1.70	2.08	.82	12.0	11.0	11.0	10.0	10.0	9.5
18	2.95	1.25	2.36	11.0	11.5	8.5	11.0	10.5	12.0
19	29.40	1.17	25.13**	12.0	11.0	3.0**	4.5**	6.5*	11.0
20	27.10	.58	46.72**	12.0	8.0**	2.5**	4.0**	3.5**	8.5*
21	23.80	.83	28.67**	11.0	8.5	2.5**	3.0**	8.0	9.0
23	17.00	1.33	12.78**	11.0	9.5	6.5**	2.5**	7.5*	8.0*
Per Cent Affected (< .01 level) =					27.3	72.7	72.7	45.5	27.3

**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD

1.3 Manual Dexterity

The raw data for the test of this ability, presented in Table B-1.3, were obtained by use of the Minnesota Manipulation Test, and represent the number of blocks moved in a one minute period. However, the experimental group data, as noted below, were modified prior to analysis.

1.3.1 Analysis of Control Group Data. For this response variable, the analysis of the control group data shown in Table 9 indicated a significant session and replication effect, as well as a suggestion of a possible interaction effect. This last, however, just significant at the .05 level, was not considered to be sufficiently clearly defined as to be worth further consideration in this analysis. Its lack of importance is further reflected in the fact that the reliability estimates are as high as .963 and .956.

The replication effect observed was of such a nature that the second trial within each test session produced a generally better score (more blocks moved) than the first. This is a "massed practice" effect which, if not accounted for through the statistical model being applied, would make the estimate of sampling error obtained appear to be substantially larger than it should be. For this effect, performance on the first trial served to facilitate performance on the second. That this is an effect different from the general learning trend which can be inferred from the session effect observed was indicated by the appearance of this same pattern in the experimental group in spite of generally decreasing levels of performance in the first three sessions for that group.

Even for the control group, the significant session effect was not just a simple learning curve, i.e., it did not represent a continual increase in general level of performance. The six session means for the control group (42.4, 46.0, 46.4, 43.1, 45.0, and 48.7) showed a drop between sessions D_2 and D_3, but gains between all other successive sessions. The time period between D_2 and D_3 included the lunch break, while between the other sessions the subjects were generally inactive. Thus, the active

TABLE 9

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR MANUAL DEXTERITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	3	2546	848.67		
Sessions (B)	5	216	43.20	6.68	< .01
Interaction (AB)	15	97	6.47	2.47	< .05
Replications (R)	1	43	43.00	16.47	< .01
Error (E)	23	60	2.61		
Total (T)	47	2962			

Test-Retest Reliability: Across all Sessions = .963

For an Average Session = .956

TABLE 10

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR MANUAL DEXTERITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	10	1829	182.90		
Sessions (B)	5	8116	1623.20	52.46	< .01
Interaction (AB)	50	1547	30.94	5.06	< .01
Replications (R)	1	532	532.00	87.07	< .01
Error (E)	65	397	6.11		
Total (T)	131	12421			

period generated a form of what learning theorists would refer to as "pro-active inhibition." This occurs when a more or less unrelated form of activity, interpolated between learning trials, produces a drop in performance on later trials. The occurrence of this phenomenon illustrates the necessity of controlling inter-session activity and of having a control group which not only does all of the tasks required of the experimental group at each session, but which is similarly occupied between sessions. Not all response variables are sensitive to this phenomenon, but that it is characteristic of manual dexterity tests was reaffirmed in Study II where a different test of this same ability produced similar results.

1.3.2 Analysis of Experimental Group Data. The pattern of changes noted here for the control group should also occur for the experimental group, whether the drug affects performance or not. Therefore, the effect of the drug on this response should be reflected in the differences between the actual data observed and the control group means for each of the sessions. As a simple example of what is being done here, if the control group data provided means of 10, 15, and 20, and an experimental group subject had scores of 8, 10, and 12, the extent to which the drug affected his performance would be reflected in the difference scores of -2, -5, and -8. The subtraction, in effect, removes the normal trend of the variable and leaves the drug effect as such for analysis. It was the difference scores which were analyzed, and the results of these analyses are presented in Tables 10 and 11.

Table 10 clearly indicates significant session, interaction, and replication effects. The last was still based on the superiority of the second trial over the first within each session, even where performance in general was dropping off between sessions. The presence of the interaction again required treating the set of scores from each subject as an experimental unit. One point, however, may be noted in the group session trend. There was a strong, general improvement in performance for the experimental group between sessions D_2 and D_3, much greater than, and

TABLE 11

RESULTS OF INDIVIDUAL ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR MANUAL DEXTERITY

Subj.	Mean Sq. Variance Sessions	Error	F Ratio	Session Means PD	D_1	D_2	D_3	D_4	D_5
10	256.4	4.4	58.27**	+6.5	-24.5**	-16.0**	-10.0**	-1.0*	-1.5*
11	38.0	6.0	6.33*	-5.0	-14.5*	-18.0**	-12.0*	-10.0	-12.0*
12	71.2	2.4	29.67**	-7.0	-21.0**	-23.0**	-19.5**	-16.0**	-12.5*
13	159.2	2.4	66.33**	+6.0	-18.0**	-16.5**	-7.0**	-3.0**	-5.5**
14	157.0	7.0	22.43**	-2.5	-23.0**	-26.5**	-14.5**	-10.0*	-11.0*
17	83.6	1.8	46.44**	-1.5	-18.5**	-12.5**	-8.0**	-3.5	-4.0
18	173.8	4.6	37.78**	+5.5	-17.5**	-16.5**	-4.0**	0.0*	-1.5*
19	140.0	9.2	15.22**	+7.5	-16.0**	-11.0**	-3.5*	-.5*	-.5*
20	169.4	4.2	40.33**	+5.0	-16.5**	-19.5**	-7.0**	-5.0**	-1.5*
21	555.2	9.6	57.83**	+4.5	-25.5**	-36.0**	-15.5**	-1.0	+3.0
23	128.6	4.2	30.62**	-.5	-21.0**	-21.5**	-12.5**	-10.5**	-8.0*
Per Cent Affected (<.01 level) =					90.9	100.0	81.8	36.4	9.1

**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD

opposite in effect to, the proactive inhibition phenomenon noted for the control group. This might suggest that eating or greater activity served to offset a significant portion of the drug effect on this mode of behavior. While interferring slightly with the learning trend, as in the control group, the main effect of the activity here might be to overcome some of the soporific effect of the agent.

Table 11 presents the results of the individual analyses. The F ratios reported and the general pattern of differences between PD and drug session means suggests that this response was much more affected by the agent than were either of the visual acuity tests. All subjects were seriously affected at D_2 (135 minutes after drug treatment) and all but one had recovered by D_5 (525 minutes after treatment). Subject 21 showed an unusually sharp drop and rapid recovery in performance, quite different from the patterns observed in the remainder of the sample.

1.3.3 Critique of the Manual Dexterity Test. Data obtained from the Minnesota Manipulation Test proved to be highly reliable, although sensitive to extraneous (in terms of these studies) sources of variation. A significant amount of learning continued during the second day of testing, and inter-session activity also appeared to influence performance. These factors would indicate that critical attention must be given to the inter- and intra-session activities of both the control and the experimental groups when this test is used.

1.4 Static Strength

This ability was assessed by use of a hand dynamometer. The results are reported in kilograms in Appendix Table B-1.4. Each trial represented a single squeeze of the dynamometer handle.

1.4.1 Analysis of the Control Group Data. The control group data analysis, contained in Table 12, indicated no trend or session effect. The replication effect suggested was such that the first trial was generally

TABLE 12

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR

STATIC STRENGTH

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	3	9280	3093.33		
Sessions (B)	5	14	2.80	.23	>.05
Interaction (AB)	15	184	12.27	3.10	<.01
Replications (R)	1	19	19.00	4.80	<.05
Error (E)	23	91	3.96		
Total (T)	47	9588			

Test-Retest Reliability: Across all Sessions = .983

For an Average Session = .977

TABLE 13

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR

STATIC STRENGTH

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	10	9501	950.10		
Sessions (B)	5	949	189.80	12.95	< .01
Interaction (AB)	50	733	14.66	1.87	< .01
Replications (R)	1	7	7.00	.89	> .05
Error (E)	65	510	7.85		
Total (T)	131	11700			

stronger than the second, but this effect was slight with respect to the total variation observed. More important for this analysis was the appearance of a significant interaction in these data, which indicated a lack of consistency among the control group subjects. There was, then, no general group trend even though the variable in question was not particularly stable over time. The data obtained were still highly reliable (.983 and .977) since the subjects did not interchange orders from trial to trial, but they did manifest significant differences in trend directions from one trial to another with respect to each other. Thus, the range of differences between subjects was large and, while there were significant changes from session to session, these were not consistent between subjects, across the sessions.

With the appearance of an interaction in the control group, no general frame of reference could readily be provided for the analysis of changes in the experimental group data. The logical approach would require that a subject from each group be matched for trends and that performance differences between pairs be analyzed to assess the drug effect. Such a degree of precision is not feasible in these studies, nor are a sufficient number of control subjects available for such matching.

1.4.2 Analysis of the Experimental Group Data. Lacking a frame of reference, the experimental group data could only be analyzed as they were obtained. Thus, the ensuing interpretation of the analysis becomes questionable.

Table 13 indicates a significant session and interaction effect. Whether or not the latter represents something related to the drug (i.e., different from or in addition to the effect noted in the control group) cannot be ascertained here. The specific sources of these effects were indicated in the analyses of the individual subjects' data.

Table 14 shows that the session effect was related almost completely to the changes in performance of Subjects 21 and 23 in terms of lower scores, but also to Subjects 11 and 17 who showed initial drops followed by

TABLE 14

RESULTS OF INDIVIDUAL ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR STATIC STRENGTH

Subj.	Mean Sq. Variance Sessions	Error	F Ratio	Session Means PD	D_1	D_2	D_3	D_4	D_5
10	34.4	11.8	2.92	48.5	50.0	50.0	45.5	50.0	58.0
11	59.0	.6	98.33**	44.0	40.0**	44.0	44.5	51.5*	54.5**
12	30.0	3.2	9.37*	34.0	26.5*	26.0*	27.5*	29.5	35.0
13	8.0	10.2	.78	54.5	51.0	51.5	53.0	56.0	55.0
14	48.4	10.8	4.48	53.5	47.0	51.5	44.0*	56.0	56.0
17	31.0	.8	38.75**	51.0	43.5**	52.0	50.5	51.5	55.5**
18	13.2	3.4	3.88	56.0	54.5	55.0	59.0	58.5	61.0*
19	14.8	13.6	1.09	64.5	59.5	56.5	62.0	62.0	60.5
20	24.2	6.2	3.90	64.0	55.0*	58.5	58.5	58.0	63.5
21	42.0	.8	52.50**	51.0	38.5**	40.5**	43.5**	45.5**	47.5*
23	31.4	.4	78.50**	65.5	55.0**	54.5**	58.5**	59.0**	59.5**
Per Cent Affected (<.01 level) =					36.4	18.2	18.2	18.2	9.1

**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD

data which were higher than the PD performance in the D_5 session. The two former subjects may represent drug induced variation while the latter two may reflect changes similar to those noted in the control group. This, of course, is speculation since an estimate of how each experimental subject would have behaved in the absence of the drug is lacking.

In estimating the per cent of men affected at the bottom of Table 14, only changes which represented performances below the PD level were considered since the main concern in these studies is with loss of ability to perform. Thus, the data for Subjects 11 and 17 were not included in the estimate for the D_5 session.

1.4.3 Critique of the Static Strength Test. Because of the large range of differences among subjects, data on this variable were highly reliable. Whether or not this represents a "fortunate" accident in sampling or a reasonable estimate of the range to be expected must await a cross-validation of these results. At the same time, significant changes in performance in the control group did occur, but not in a consistent manner. If the finding of an interaction in the control group data is confirmed in future studies employing this test, its utility in this type of research may be open to question. At least, this would hold for studies where the detection of relatively small effects on this variable would have practical significance.

A possible explanation of the changes noted in both groups of subjects might be found in the inadvertent introduction of muscle strain on a particular trial. In attempting to squeeze as hard as possible, a subject may induce a strain of sufficient magnitude that it would carry over to one or more subsequent trials. This, rather than the drug, might explain the data obtained from Subjects 21 and 23. Because such an event cannot be adequately controlled, it may be better to eliminate this test and variable from the battery. This decision, however, should await results in future studies.

1.5 Gross Body Equilibrium

The basic data obtained from the Balance A Test which was used to assess this ability are contained in Table B-1.5 in terms of seconds up to a maximum of 20. It is evident in the control group data as well as the PD and D_5 sessions data of the experimental group that the arbitrary upper limit of 20 seconds prevented the appearance of any kind of distribution of scores on this variable. Because of this, an analysis of the results as such was not possible.

1.5.1 Analysis of the Control Group Data. About all that can be said in terms of the control group data is that most subjects are quite capable of balancing for 20 seconds on one or both trials at each test session. Data were not obtained for Subject 22 at the PD session, but he appears to have been the only subject who might have had difficulty with this task in the earlier test sessions.

1.5.2 Analysis of the Experimental Group Data. Since no distribution of scores was available for this test, the most obvious alternative was to look for a criterion on the basis of which to dichotomize the data so that some form of analysis could be carried out. The criterion chosen for this was that a trial was considered "passing" or "successful" if the subject was able to balance for 20 seconds on one of the two trials at a given session; otherwise, performance was rated as failing. Success and failure are indicated as "1" or "0" respectively in Table 15. Applying this criterion to the control group, only one failure at one session for one subject was observed. In general, then, it would be hypothesized that the undrugged subject should be able to consistently pass this test.

For the experimental group, only one failure was noted at the PD session, so that the effect of the drug should be reflected in increases beyond this failure rate (9.1%). Thus, the "per cent of men affected" row reflects the actual per cent of failures less this expected rate. How this might relate to the previous use of the per cent affected concept cannot be established here.

TABLE 15

ANALYSIS OF EXPERIMENTAL GROUP DATA FOR GROSS BODY EQUILIBRIUM CLASSIFIED AS PASS (1) OR FAIL (0) BASED ON WHETHER OR NOT ONE TRIAL IN TWO EXCEEDED 20 SECONDS AT A GIVEN TEST SESSION

	Test Sessions					
Subj.	PD	D_1	D_2	D_3	D_4	D_5
10	0	0	0	1	1	1
11	1	0	0	1	0	1
12	1	0	0	0	1	0
13	1	0	0	0	1	1
14	1	0	0	1	1	1
17	1	0	0	0	1	1
18	1	0	0	1	1	1
19	1	0	1	1	1	1
20	1	0	0	1	0	1
21	1	0	0	0	1	1
23	1	0	0	1	1	1
Per Cent Men Affected =	90.9	81.8	27.3	9.1	0.0	

TABLE 16

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR GROSS BODY EQUILIBRIUM

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects	10	1.00	.10		
Sessions	5	9.00	1.80	15.00	<.01
Error	50	6.00	.12		
Total	65	16.00			

If a test of the effect of the drug (in terms of the criterion proposed) across sessions is desired, the data can be treated as in an ordinary analysis of variance (Winer, 1962, p. 138) as indicated in Table 16. More precisely, a chi-square value may be computed from the data in the table as chi-square with (b-1) degrees of freedom = $a(b\text{-}1SS_B/SS_T\text{-}SS_A$ (Cochran, 1950). Both techniques indicate a session effect significant beyond the .01 level.

1.5.3 Critique of the Gross Body Equilibrium Test. In its present form, this test permits only a gross assessment of any changes in performance. The control group data do not provide information on reliability, nor can degrees of change be assessed over time. This follows from the fact that the upper limit of 20 seconds makes the test too simple for the undrugged subjects.

It is recommended that the test procedure be modified so that the subjects are required to keep their hands on their hips during the balancing, and that the upper limit be extended to one minute for each of the trials. This should provide more meaningful quantitative data for future analyses.

1.6 Number Facility.

The data for the assessment of this ability were obtained by use of the Moran and Mefferd (1959) addition tests developed by these authors for inclusion in the Texas Battery of cognitive tests. There are twenty forms of the addition test available, all of which are rated as being of comparable difficulty. Six different forms were utilized for the six test sessions on the second day, but all subjects received the same form at any given session.

The data are reported in Table B-1.6 of Appendix B in terms of the number of correct responses at each session. Since only one form of the test was administered at each session, no estimate of sampling error could be made nor could a test be made of the interaction in the control or experimental groups. It is for these tests that replication within a session is required.

1.6.1 Analysis of the Control Group Data. The control group data, presented in Table 17, showed no significant session effect. This may be interpreted as indicating that performance during repeated testing does not improve with practice or, alternatively, as demonstrating that the difficulty level of the different forms is equivalent for each session. The specific comparisons of PD data with subsequent session means did reveal that the D_1 data differed at the .05 level, but that none of the other data did. This was interpreted as indicating that the form used at D_1 was slightly easier than the others and, since this was identical for the experimental group, no adjustment of the data for that group would be required.

TABLE 17

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA

FOR NUMBER FACILITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	3	558	186.0		
Sessions (B)	5	43	8.6	1.28	> .05
Interaction (AB)	15	100	6.7		
Total (T)	23	701			

Test-Retest Reliability: Across all Sessions = .847

For an Average Session = .817

1.6.2 Analysis of the Experimental Group Data. The number facility data obtained from the experimental group, similar to the data from the balance test, were not amenable to the standard method of analysis developed for use in this study. The zeroes appearing in Table B-1.6 of the Appendix for the experimental group represent, for the most part, cases where the subject was untestable. Obviously, the appropriate number facility score has not been shown to be zero, and the accumulation of 18 zeroes artificially truncates the distribution of scores for this variable. This also makes it impossible to compute a mean for the group at any time point where zeroes occur.

In order to perform some type of analysis, a criterion was selected on the basis of which to rate a given performance during the drug sessions as "passing" or "failing". The criterion chosen, mainly because it served to approximately halve the data, was to consider a score as passing if it was equal to or greater than 50% of the score obtained at the PD session. Performance, rated in this way, appears in Table 18, and the per cent men affected was determined on the same basis.

The analysis of variance of the dichotomized data for the experimental group is shown in Table 19. This indicates that the session effect was significant.

Since this test was of the paper-and-pencil variety, a question naturally arose about the possible relationship between performance on this test and the effect of the drug administered on near visual acuity. To establish some concept of the relationship between these two variables, the data obtained from each were dichotomized within each session for both variables, and tetrachoric correlations were computed to estimate the degree of relationship in each of the sessions. The resulting correlations were .16, .68, .87, .84, .87, and .68 for the PD through the D_5 sessions respectively. Thus, the data were initially unrelated at the pre-drug stage, but after the administration of the drug, changes in near visual acuity were definitely related

TABLE 18

ANALYSIS OF EXPERIMENTAL GROUP DATA FOR NUMBER FACILITY CLASSIFIED AS PASS (1) OR FAIL (0) BASED ON WHETHER ON NOT DRUG SESSIONS EXCEEDED 50% OF PRE-DRUG SESSION PERFORMANCE

		Test Sessions				
Subj.	PD	D_1	D_2	D_3	D_4	D_5
10	30	0	0	0	0	0
11	38	1	0	0	1	1
12	38	1	0	0	0	0
13	69	1	0	0	0	1
14	33	0	0	0	0	0
17	35	1	1	1	1	1
18	49	1	0	1	1	1
19	47	1	0	0	0	0
20	33	0	0	0	0	0
21	39	0	0	0	1	1
23	42	1	0	0	1	1
Per Cent Men Affected =		36.4	90.9	81.8	54.5	45.4

TABLE 19

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR NUMBER FACILITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects	10	5.80	.58		
Sessions	4	2.45	.61	5.08	<.01
Error	40	4.75	.12		
Total	54	13.00			

to changes in performance on the number facility test. On this basis, the changes noted in the analysis of the number facility data cannot be unequivocally related to this ability as such. An investigation of the effect of providing the subject with corrective lenses during the testing of this ability has been completed and will be published as Technical Report No. 3 (Freedle and Elkin, 1966). This report indicates that significant improvement in performance can be achieved on this variable by providing corrective lenses, and that the effect of the drug on this ability is not as strong as would be indicated by the data reported here.

1.6.3 Critique of the Number Facility Test. The lack of replication in the administration of this test leaves several important questions untestable in a detailed analysis of the results. The problem can be overcome by scoring the odd-numbered items separately from the even-numbered items on each form rather than by trying to administer two forms at any one test session.

The effect of the loss of near visual acuity on this and any other type of paper-and-pencil test must be dealt with if attempts are to be made to assess cognitive abilities as such in this manner. Either the subjects must be provided with corrective lenses before taking these tests, or they must be treated, medically, to offset the loss in acuity which is incurred. It is quite possible that, with corrective lenses, some degree of performance would have been measurable at each of the post-drug test sessions, and that a far more sensitive analysis of the data could,therefore, have been performed. The question of related ability losses must be dealt with in terms of all of the variables being assessed in these studies, but this point will be discussed in the summary section of this report.

1.7 Short Term Memory

The basic data from the auditory number span test, appearing in Table B-1.7, represent the largest number of digits recalled in the first and in the second series of presentations of the number items at each session. The

series are reported separately here in order to obtain replication within each session, even though this altered the scoring concept used in Technical Report No. 1 (Elkin, et al., 1965a).

1.7.1 Analysis of the Control Group Data. The control group data, in Table 20, did not indicate a general trend or change across sessions, but the interaction did indicate that changes occurred, differing from one subject to another. These data, then, like those obtained for static strength, did not provide a clear frame of reference for determining changes in the experimental group. There was a strong replication effect, representing generally poorer performance on the second series than on the first. This may be a function of the timing between the presentations of items in the test.

1.7.2 Analysis of the Experimental Group Data. The analysis of the experimental group data, shown in Table 21, indicated a significant session effect and a slight interaction, but no systematic change related to replication. Analysis of the performance of the individual subjects, as given in Table 22, showed that only two subjects (18 and 21) were seriously affected on this ability. In general then, it might be concluded that the drug had little effect on short term memory.

1.7.3 Critique of the Short Term Memory Test. Before the conclusion of little effect, stated above, is accepted, some consideration should be given to the manner in which this test is scored. A subject receives credit for a "level" (number of elements in a test item) only if all elements within an item are correctly recalled. Yet, seldom does a subject miss all elements within an item. Not scoring partial reproduction makes the difference between levels of 6 and 7 items correct much greater than the difference between 3 and 4. Thus, the scoring system becomes progressively more gross as item length increases. It may be the grossness of the scoring system which is masking patterns of change on this ability. Some consideration might be given to developing a different scoring system or implementing a different test of this same ability which would provide finer measurement.

TABLE 20

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR

SHORT TERM MEMORY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	3	65.00	21.67		
Sessions (B)	5	2.25	.45	.40	>.05
Interaction (AB)	15	16.75	1.12	2.60	<.05
Replications (R)	1	4.17	4.17	9.70	<.01
Error (E)	23	9.83	.43		
Total (T)	47	98.00			

Test-Retest Reliability: Across all Sessions = .795

For an Average Session = .754

TABLE 21

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR

SHORT TERM MEMORY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	10	53.05	5.30		
Sessions (B)	5	70.97	14.23	15.14	< .01
Interaction (AB)	50	46.86	.94	1.77	<.05
Replications (R)	1	1.49	1.49	2.81	>.05
Error (E)	65	34.51	.53		
Total (T)	131	206.88			

TABLE 22

RESULTS OF INDIVIDUAL ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR SHORT TERM MEMORY

Subj.	Mean Sq. Variance: Sessions	Mean Sq. Variance: Error	F Ratio	Session Means: PD	D_1	D_2	D_3	D_4	D_5
10	.88	.48	1.83	4.0	3.0	2.0*	3.0	3.0	2.5
11	.68	.28	2.43	4.5	3.0*	3.5	3.0*	3.5	3.0*
12	.60	.60	1.00	4.0	4.0	3.0	3.0	4.0	3.0
13	2.48	.35	7.09*	6.0	3.0**	3.5*	5.0	5.0	5.0
14	2.33	.40	5.82*	4.5	3.5	2.5*	2.0*	2.5*	3.0
17	.95	.55	1.73	4.5	3.5	3.0	4.0	4.5	3.0
18	2.15	.15	14.33**	4.0	3.5	2.0**	2.0**	3.5	4.5
19	1.15	.88	1.31	5.0	3.0	3.0	3.5	4.0	4.0
20	5.40	1.00	5.40*	5.0	2.0*	1.5*	1.5*	5.0	3.0
21	4.40	.33	13.33**	4.5	1.5**	1.5**	2.0**	4.0	4.5
23	2.53	.40	6.32*	6.5	4.0*	4.0*	4.0*	6.0	4.5*
Per Cent Affected (< .01 level) =					18.2	18.2	18.2	0.0	0.0

**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD

At any rate, it is recommended that the procedure with the present test be changed to present two separate lists of items rather than duplicate items within a single sequence as is presently done. This would constitute a clearer measurement of replication effect and may improve test reliability.

1.8 Time Estimation

In testing this ability, an empty interval production technique was used. The subjects were given five opportunities to estimate a ten-second interval at each test session, so that there were five replications of this test. This is indicated by the data in Table B-1.8 in Appendix B. Each trial, then, represents a single guess reported to the nearest hundreth of a second. The missing entries for Subjects 14 and 21 represent test sessions at which these subjects were judged to be untestable. They could not perform the task required.

1.8.1 Analysis of the Control Group Data. The analysis of the control group data, presented in Table 23, did not reveal a session effect, but it did indicate a strong interaction and replication effect. The latter showed that, in general, the time estimates tended to increase from a low of 9.92 seconds on the first trial to a high of 10.71 seconds on the fifth. This trend holds within sessions for the group, but not across sessions. The appearance of the interaction, and the consequent loss of a clear frame of reference for analyzing changes in the experimental group, was not too unexpected for this variable. Of all of the response modes tested in these two studies, this variable probably represents the most unfamiliar or unpracticed mode of response for the subjects and, therefore, should lead to a great deal of instability in subject performance.

Nevertheless, differences between subjects were sufficiently large, and maintained with a fair degree of consistency so as to provide reliability estimates of .768 and .723. These are high enough to justify continued work with this ability.

TABLE 23

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR TIME ESTIMATION

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	3	136.12	45.37		
Sessions (B)	5	26.47	5.29	1.93	>.05
Interaction (AB)	15	41.10	2.74	6.85	<.01
Replications (R)	4	10.56	2.64	6.60	<.01
Error (E)	92	36.60	.40		
Total (T)	119	250.85			

Test-Retest Reliability: Across all Sessions = .768

For an Average Session = .723

TABLE 24

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR TIME ESTIMATION

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	8	278.49	34.81		
Sessions (B)	5	48.16	9.63	5.38	<.01
Interaction (AB)	40	71.60	1.79	2.18	<.01
Replications (R)	4	29.14	7.28	8.88	<.01
Error (E)	212	174.36	.82		
Total (T)	269	601.75			

1.8.2 Analysis of the Experimental Group Data. Since data were missing for Subjects 14 and 21, these two subjects were not included in the general analysis of the experimental group, but their data were considered in the later analyses of the individual subjects. This means, of course, that group estimates of performance at the various time points would be higher than they should be. It may be noted, in the data obtained for these subjects, that they provided rather short time estimates from D_1 to D_3.

Recognizing the above exception, the experimental group data analysis, shown in Table 24, indicated a significant session, interaction, and replication effect. This last effect again indicated a general tendency for the earlier estimates to be shorter than the later ones, and the change covered a similar range (8.9 to 9.9 seconds) as that found for the control group although the absolute levels were lower due to the effect of the drug on this ability.

The pattern of the drug effect is shown by the individual analyses presented in Table 25. The extent of some of the changes in time estimation indicated in the table would suggest that tasks requiring subjective timing would be seriously affected by this type of chemical agent. In the determination of the per cent of men affected at the base of the table, both significantly high and low estimates were included, and the untestable cases were included as well.

1.8.3 Critique of the Time Estimation Test. It was noted earlier that instability of performance due to the unfamiliar nature of the task required by this test was not unexpected. In the administration of this test for Study I, immediate knowledge of results was given to the subjects after each trial at the first, or orientation, session on the first day of testing. It was hoped that this would provide sufficient training in the task to overcome most of this instability, but, apparently, it was not sufficient. It is recommended that the procedure be changed to provide knowledge of results after each trial throughout all of the sessions during the first day of testing. This

TABLE 25

RESULTS OF INDIVIDUAL ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR TIME ESTIMATION

Subj.	Mean Sq. Variance: Sessions	Mean Sq. Variance: Error	F Ratio	Session Means: PD	D_1	D_2	D_3	D_4	D_5
10	2.12	1.90	1.13	10.25	9.25	8.71	10.50	9.75	9.69
11	2.65	.62	4.27**	9.30	7.15**	7.97*	8.04*	7.51**	7.97*
12	1.90	1.02	1.86	8.64	8.44	9.52	7.76	8.09	8.86
13	7.41	1.59	4.66**	14.00	10.56**	12.32*	12.13*	11.36**	11.06**
14	3.83	.49	7.82**	9.77	7.35**	***	8.46**	8.85	8.38*
17	.40	.64	.62	9.14	9.71	9.30	9.84	9.77	9.62
18	3.84	.32	12.00**	10.13	8.61**	8.49**	8.75**	9.01**	10.58
19	1.79	.27	6.63**	10.75	9.28**	9.46**	9.19**	10.04*	9.45**
20	2.22	1.12	1.98	10.83	8.84**	9.73	10.20	9.61	9.39*
21	15.07	.46	32.76**	9.76	7.16**	***	***	11.25**	10.21
23	1.64	.15	10.93**	10.96	9.39**	9.97**	9.64**	10.20**	9.57**
Per Cent Affected (< .01 level) =					72.7	45.5	45.5	45.5	36.4

***Untestable at this session
**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD

should improve the reliability of the data obtained, and it may serve to eliminate the interaction pattern noted in the control group data for this study.

1.9 Simple Reaction Time

The data for this variable were obtained through use of a visual reaction time apparatus which recorded responses to the nearest hundreth of a second. Each data point was based on the mean of 10 observations. The data, as reported in Appendix Table B-1.9, were converted into a natural log form in order to reduce the extremely skewed nature of the raw score response distribution. This is a transformation which is quite commonly used with reaction time data.

1.9.1 Analysis of the Control Group Data. The control group analysis, contained in Table 26, showed no changes related to interaction, sessions, or replication. The comparisons of the PD mean with each of the subsequent session means indicated that the PD data could be considered as representative of later performance. The general reliability of .758, and the average session reliability of .710 were somewhat low, but these should improve as more data become available.

1.9.2 Analysis of the Experimental Group Data. The experimental group data, in view of the above, were analyzed without further reference to the control group. The analysis, contained in Table 27, showed a significant interaction and sessions effect. Examination of the data for the individual subjects, presented in Table 28, demonstrated that only five of the 11 subjects were seriously affected on this variable, and that among the five there were differences in time and degree of effect.

1.9.3 Critique of the Simple Reaction Time Test. In general, the data obtained from this test were satisfactory. However, since the usual reliability associated with tests of this mode of response is almost always in the .90's, a close examination of the apparatus being used in this test,

TABLE 26

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR SIMPLE REACTION TIME

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	3	.372	.1240		
Sessions (B)	5	.069	.0138	1.75	>.05
Interaction (AB)	15	.118	.0079	1.04	>.05
Replications (R)	1	.013	.0130	1.71	>.05
Error (E)	23	.175	.0076		
Total (T)	47	.747			

Test-Retest Reliability: Across all Sessions = .758

For an Average Session = .710

TABLE 27

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR SIMPLE REACTION TIME

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	10	3.816	.3816		
Sessions (B)	5	6.257	1.2514	8.21	<.01
Interaction (AB)	50	7.625	.1525	5.80	<.01
Replications (R)	1	.046	.0460	1.75	>.05
Error (E)	65	1.708	.0263		
Total (T)	131	19.452			

TABLE 28

RESULTS OF INDIVIDUAL ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR SIMPLE REACTION TIME

Subj.	Mean Sq. Variance Sessions	Error	F Ratio	Session Means PD	D_1	D_2	D_3	D_4	D_5
10	.0508	.0458	1.11	-1.575	-1.189	-1.176	-1.339	-1.167	-1.368
11	.0200	.0055	3.64	-1.425	-1.269	-1.27[illegible]	-1.330	-1.370	-1.533
12	.0152	.0067	2.27	-1.577	-1.398	-1.417	-1.427	-1.535	-1.594
13	.0650	.0052	12.50**	-1.401	-1.029**	-1.324	-1.372	-1.491	-1.542
14	.8620	.0350	24.63**	-1.901	-.838**	+.046**	-1.461	-1.563	-1.604
17	.0378	.0622	.61	-1.556	-1.351	-1.288	-1.558	-1.581	-1.312
18	.0304	.0243	1.25	-1.402	-1.332	-1.219	-1.450	-1.454	-1.582
19	.0092	.0017	5.41*	-1.400	-1.242**	-1.249*	-1.291*	-1.368	-1.370
20	.6426	.0348	18.47**	-1.558	-.332**	-.663**	-1.520	-1.578	-1.640
21	.9514	.0628	15.15**	-1.459	-.541*	+.134**	-.456**	-1.461	-1.498
23	.0916	.0083	11.04**	-1.717	-1.325**	-1.173**	-1.500	-1.674	-1.619
Per Cent Men Affected (< .01 level) =					45.5	36.4	9.1	0.0	0.0

**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD

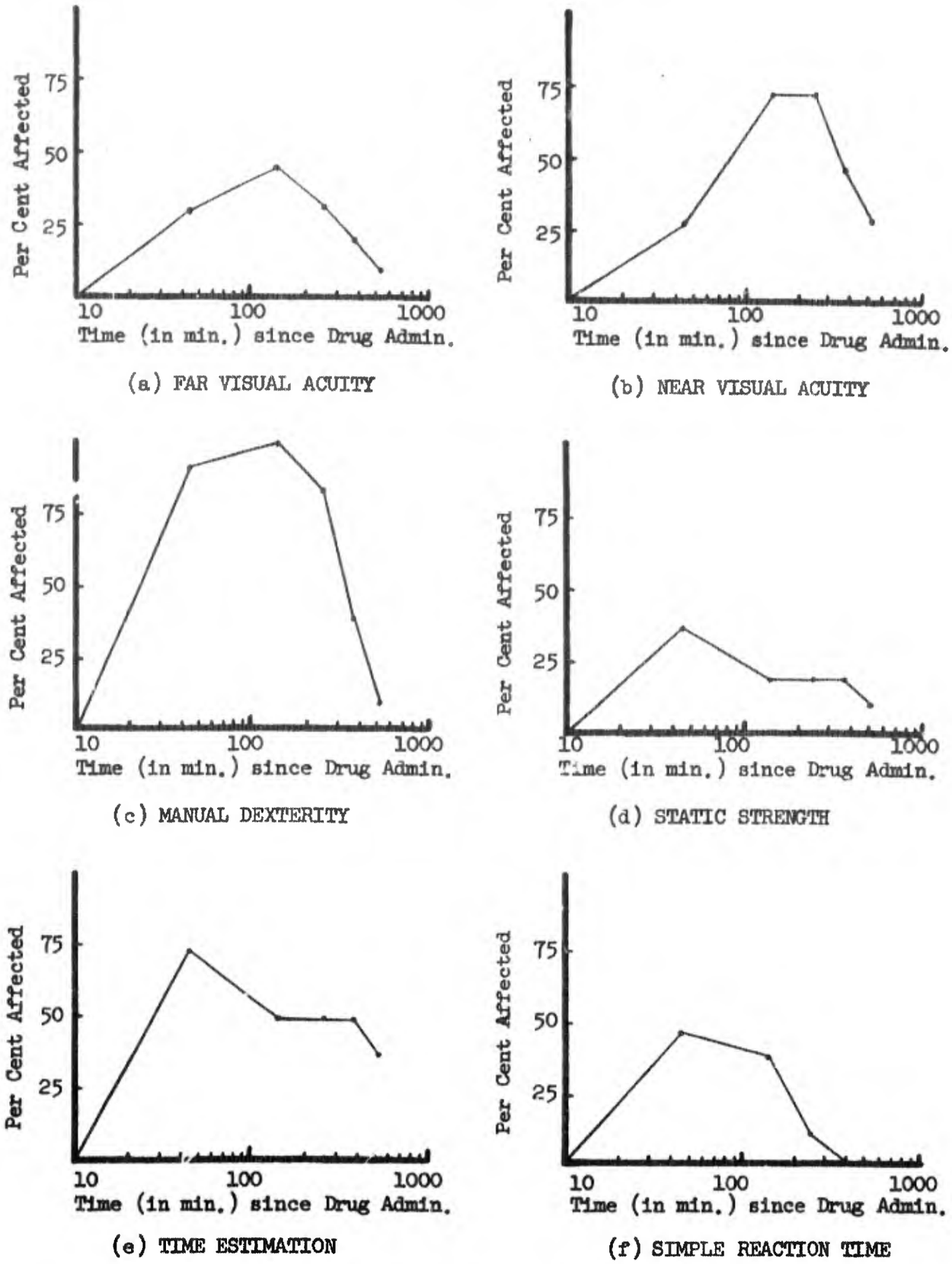
along with the manner of recording responses, might be in order to determine if the test apparatus or procedure is contributing to the low correlations obtained.

1.10 General Assessment of Study I

While the major concern of this study was with the development of tests and procedures for inclusion in the AIR battery, a general view of the data may be used to examine the effects of a 12 μg/kg IM injection of scopolamine on the basic abilities measured. Of the nine response variables examined in Study I, six provided reasonably clear and comparable results. These six: far visual acuity, near visual acuity, manual dexterity, static strength, time estimation, and simple reaction time, are included in the graphs of Figure 2 in terms of the per cent of men affected as a function of the logarithm of the time since the administration of the drug. Of the other three variables, gross body equilibrium and number facility could not be analyzed in a manner similar to that used for the other variables. Because the basis for classifying a particular response as affected or unaffected was unrelated between these two variables or to any of the others, plotting their results along with the others would be misleading. The data for short term memory are obvious without plotting.

The plotting of the data in Figure 2 as a function of the log of the time since drug administration suggests that a simple quadratic equation might be used to describe the relationship presented for far and near acuity, manual dexterity, and reaction time. A more complex form might be required for static strength and time estimation. However, the small amount of data available would not warrant such an attempt at this time, nor would it be worthwhile without cross-validation of the results obtained here.

It should be clear from the analyses presented earlier, that the figures do not imply that the same man or men are affected across the time points or on the several variables at any point in time. Each data point plotted simply represents a count of the number of men affected on that variable at that



(a) FAR VISUAL ACUITY

(b) NEAR VISUAL ACUITY

(c) MANUAL DEXTERITY

(d) STATIC STRENGTH

(e) TIME ESTIMATION

(f) SIMPLE REACTION TIME

Figure 2. Per cent of men affected as a function of time since drug administration in Study I.

point in time. The relationships among variables and among subjects should be examined as a function of time. From the essentially univariate analyses which have been discussed in this study, the next logical step would be to a multivariate analysis. Were sufficient data available, it would be of interest to intercorrelate the tests at each point in time and to examine the number of factors which might be identifiable at the various stages of drug effect. Parallel with this should go analysis of the intercorrelations between subjects at the various time points to determine how many types of subjects might be identified. The consistent appearance of subject by time point interactions has already demonstrated that more than one type is present. A simple comparison of Subject 21 with Subject 11 across the various tests can demonstrate this.

It is these types of analyses which must eventually be performed in order to determine the number of variables which must be assessed to clearly specify the effect of an agent on "human behavior", and to clarify the subject variables which cause one subject to react quite differently from another under equal doses of the same agent. While it may be practically impossible to gather or handle a sufficiently large number of subjects for these types of analyses (a minimum of 50) in any one study, a gradual accumulation of information from a sufficient number of adequately controlled small studies would serve the same purpose.

2.0 Study II

The second study employed tests of arm-hand steadiness, explosive strength, manual dexterity, finger dexterity, multi-limb coordination, dynamic flexibility, and dynamic strength. The procedure followed in the administration of the specific tests used to measure these abilities is covered in Appendix A, and a description of the apparatus used may be found in Fleishman (1964).

The general structure of the experimental design for this study was identical to that used in Study I, although the specific times for the various

test sessions on the second day were somewhat different. The specific times of the five test sessions were 30, 90, 210, 330 and 510 minutes after the administration of the drug. The agent administered was, again, a 12 µg/kg IM injection of scopolamine.

2.1 Arm-Hand Steadiness

The track-tracing test used to measure this ability provided scores in two different forms, total time in error (or in contact with the edge of the track) and total number of errors (or number of times the stylus contacted the edge of the track). The basic data obtained at each trial for each subject are contained in Tables B-2.1a and B-2.1b in Appendix B. The time scores are presented in decimal parts of a minute.

2.1.1 Analysis of Control Group Data (Time-in-Error Scores). The analysis of the time-in-error scores for the control group is presented in Table 29. The analysis indicated that there were no differences among the sessions, related to interaction, or between replications. However, the specific tests comparing the PD mean to the subsequent test session means did indicate that a gradual learning curve was developing such that the D_5 mean differed at the .05 level from the PD mean. The descending sequence of the means from the PD to the D_5 session were: .017, .014, .014, .013 and .012 minutes.

2.1.2 Analysis of Experimental Group Data (Time-in-Error Scores). Because of the possible learning trend in the control group, the data points from the experimental group were first expressed as deviations from the control group mean for each session, and the data were then analyzed in that form. The results of the general analysis are presented in Table 30, and they clearly show a session, interaction and replication effect. This last effect demonstrated that the second trial was generally better than the first.

In view of the significant interaction, the basis for the session effect is more clearly seen in the analyses of the individual subjects as presented in Table 31. Since the scores reported are in terms of time in error,

TABLE 29

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR ARM-HAND STEADINESS (TIME-IN-ERROR SCORES)

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	4	.000731	.000183		
Sessions (B)	5	.000165	.000033	1.37	>.05
Interaction (AB)	20	.000480	.000024	.77	>.05
Replications (R)	1	.000031	.000031	2.21	>.05
Error (E)	29	.000413	.000014		
Total (T)	59	.001820			

Test-Retest Reliability: Across all Sessions = .835

For an Average Session = .805

TABLE 30

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR ARM-HAND STEADINESS (TIME-IN-ERROR SCORES)

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	13	.104	.0080		
Sessions (B)	5	.137	.0274	19.75	<.01
Interaction (AB)	65	.091	.0014	7.00	<.01
Replications (R)	1	.003	.0030	15.00	<.01
Error (E)	83	.015	.0002		
Total (T)	167	.350			

TABLE 31

ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR ARM-HAND-STEADINESS (TIME-IN-ERROR SCORES)

Subj.	Mean Sq. Variance		F Ratio	Session Means					
	Sessions	Error		PD	D_1	D_2	D_3	D_4	D_5
47	.000584	.000055	10.62*	-.011	.013*	.037**	.010*	.002	-.004
49	.001183	.000229	5.17*	.008	.030	.074**	.047*	.020	.016
50	.003887	.000072	53.99**	-.005	.055**	.112**	.013	.021*	.001
51	.000426	.000047	9.06*	-.005	.022*	.031**	.020*	.001	.000
52	.000494	.000184	2.68	.009	.043	.031	.009	.009	.005
53	.001232	.000039	31.59**	.011	.067**	.039**	.051**	.014	.003
55	.000993	.000226	4.39	-.006	.041*	.046*	.018	.002	.001
56	.003505	.000370	9.47*	.012	.102**	.095**	.046	.019	.010
57	.002416	.000014	172.57**	.008	.045**	.095**	.039**	.008	.006
59	.001531	.000097	15.78**	-.005	.040**	.068**	.031*	.009	.002
60	.002977	.000115	25.89**	.003	.053**	.108**	.041*	.069**	.012
61	.001932	.000113	16.37**	-.009	.024*	.076**	.015	.005	-.003
63	.022770	.000656	34.71**	.002	.139**	.299**	.138**	.048	.046
64	.001599	.000142	11.26**	-.005	.027*	.057**	.061	.012	.001
Per Cent of Men Affected (< .01 level) =					50.0	85.7	28.6	7.1	0.0

**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD

the negative values represent performance which is better than the control group mean at that point while positive values represent poorer performance.

The pattern of results in Table 31, while showing obvious differences among subjects, would suggest that the agent had a strong effect on this ability, but that recovery from this was fairly rapid, being almost complete at D_4 (330 minutes after drug administration).

2.1.3 Analysis of Control Group Data (Number-of-Error Scores). None of the effects tested in the analysis of variance of the control group data for the number of errors committed was significant. The main result of this analysis, presented in Table 32, was the low reliability estimates of .336 and .202. These were generally indicative of the failure of this measure to differentiate, in a consistent manner, between the subjects from one test session to another. The ability to clearly identify the various subjects in terms of their overall performance was lacking; in fact, three of the five subjects had identical overall means of 7.5 errors.

2.1.4 Analysis of Experimental Group Data (Number-of-Error Scores). Ordinarily, the further analysis of data which has been shown to be essentially unreliable might not be warranted, but the experimental group data may be used to illustrate why reliability, in itself and based on an undrugged sample, may not be an appropriate criterion for the use or elimination of a test or measure from the general test battery. The analysis presented in Table 33 clearly shows a session, interaction, and replication effect. This indicated that the effect of the agent across sessions was quite strong, that subjects reacted differently to it, and that the second trial was generally better than the first.

Table 34 shows that drug effects were detectable on a subject by subject basis as well as for the group. The general impression from these data, however, would be of a less strong drug effect on this ability than that which was suggested by the time-in-error scores. If the only difference between these two modes of measuring arm-hand steadiness is the greater

TABLE 32

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR ARM-HAND STEADINESS (NUMBER-OF-ERROR SCORES)

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	4	109	27.2		
Sessions (B)	5	67	13.4	1.25	>.05
Interaction (AB)	20	215	10.7	1.72	>.05
Replications (R)	1	18	18.0	2.88	>.05
Error (E)	29	181	6.2		
Total (T)	59	590			

Test-Retest Reliability: Across all Sessions = .336

For an Average Session = .202

TABLE 33

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR ARM-HAND STEADINESS (NUMBER-OF-ERROR SCORES)

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	13	8406	647		
Sessions (B)	5	15106	3021	56.00	<.01
Interaction (AB)	65	3485	54	2.25	<.01
Replications (R)	1	257	257	10.71	<.01
Error (E)	83	1991	24		
Total (T)	167	29245			

TABLE 34

ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR ARM-HAND STEADINESS (NUMBER-OF-ERROR SCORES)

Subj.	Mean Sq. Variance Sessions	Mean Sq. Variance Error	F Ratio	Session Means PD	D_1	D_2	D_3	D_4	D_5
47	131.9	19.4	6.80*	6.0	13.5	27.0**	14.5	8.5	5.0
49	98.9	13.3	7.44*	20.0	21.5	34.0*	32.0*	19.0	18.0
50	613.1	18.7	32.79**	10.5	39.0**	51.5**	15.0	17.0	8.5
51	57.8	3.3	17.52**	8.0	14.5*	19.5**	18.0*	12.0	6.0
52	100.3	30.1	3.33	18.5	32.5*	26.0	16.0	18.5	13.5
53	188.3	16.2	11.62**	16.0	38.5**	23.0	28.0*	22.5	10.5
55	195.4	31.1	6.28*	10.5	29.5*	32.0*	19.0	11.5	10.0
56	201.0	13.8	14.57**	13.5	37.5**	31.5**	22.0	17.5	13.0
57	317.4	13.7	23.17**	20.0	30.5*	47.5**	26.5	14.0	14.5
59	183.8	34.5	5.33*	9.5	26.0*	34.0**	25.0*	15.5	11.5
60	305.3	24.1	12.67**	18.0	39.5**	49.0**	26.0	38.5**	20.0
61	376.3	14.7	25.60**	7.0	22.0*	44.0**	18.5*	12.5	8.0
63	896.0	76.7	11.86**	13.0	48.5**	70.5**	51.5**	25.5	25.5
64	194.1	28.8	6.74*	7.0	17.5	31.5**	25.5*	11.5	8.5
Per Cent Men Affected (< .01 level) =					35.7	71.4	7.1	7.1	0.0

**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD

reliability of the time-in-error scores, then the time scores should be more sensitive (imply a greater effect) than the number-of-error scores. Computation of correlations between the two scores for each of the control subjects provided data which support the contention that these are measures of the same ability.

The fact that drug effects can be clearly shown with the number-of-error measures illustrates how concern with test reliability can be diminished as the relative size of session differences increases. The effect of scopolamine on arm-hand steadiness measured in terms of the number of errors is so severe that even a rather gross measuring device can reflect the changes it produces. This would mean that the test could be useful, in its present form, for demonstrating some of the effect of this agent. If no other method of assessing this ability were available, this type of measurement could be usefully retained in the test battery.

If a test is sufficiently sensitive so as to consistently differentiate between drugged and undrugged subjects, as this measure did at the D_2 session (a question of the concurrent or diagnostic validity of the test) and to detect changes of practical significance, i.e., changes which would reflect impairment on some militarily relevant task (a question of the power of the test), the variable could be a useful part of the test battery in spite of low reliability. These points, though, cannot be adequately studied without a quantitative definition of "changes of practical significance," and this depends upon the establishment of the relationship between this test score and related military tasks. In other words, a final decision on the use or discarding of a specific test should not be made on the basis of reliability alone but, rather, on the basis of the adequacy with which a test does what it is intended to do. The point is made here, with this measure, where an alternative measure is available, so that the basic effect of retaining a measure with low reliability can be seen in terms of the consequent loss of sensitivity for experimental investigations.

2.1.5 Critique of the Arm-Hand Steadiness Test. The two methods used to measure this ability (time in error and number of errors) are, presumably, measures of the same thing. They appeared to be highly correlated within each subject in the control group. If both are equally related to military tasks of interest, only one should be retained for routine use with the test battery. On the basis of the small sample of data available, it would appear that time-in-error scores were more reliable and, other things being equal, this is the measure that should be retained. Pilot studies currently being conducted will provide data to verify the appropriateness of this choice in terms of reliability.

Generally, it is the greater reliability of the time-in-error scores which accounts for the greater sensitivity (greater per cent of men affected) in the results of Table 31 as compared to Table 34. Since it might be premature to make a definite choice at this stage of test development and, since the apparatus routinely provides both types of information, it is recommended that both types of data continue to be gathered. Eventually, in conjunction with military criterion task data, sufficient information should be available to support a clear choice between these two measures.

2.2 Explosive Strength

This ability was assessed by means of measuring the distance of a standing broad jump by each subject. The basic data, recorded to the nearest quarter of a foot, are presented in Appendix Table B-2.2.

2.2.1 Analysis of Control Group Data. The appearance, in the analysis of the control group data shown in Table 35, of a statistically significant interaction demonstrated that changes in performance were occurring within the control group, but that these were not consistent across subjects. The reliability of the data was reasonably high, but the specific subject related changes could not be taken into account in the analysis of the experimental group data.

TABLE 35

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR

EXPLOSIVE STRENGTH

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	4	42.41	10.60		
Sessions (B)	5	1.05	.21	.78	>.05
Interaction (AB)	20	5.35	.27	5.40	<.01
Replications (R)	1	.15	.15	3.00	>.05
Error (E)	29	1.41	.05		
Total (T)	59	50.37			

Test-Retest Reliability: Across all Sessions = .887

For an Average Session = .864

TABLE 36

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR

EXPLOSIVE STRENGTH

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	13	85.79	6.599		
Sessions (B)	5	40.52	8.104	26.14	<.01
Interaction (AB)	65	20.14	.310	2.79	<.01
Replications (R)	1	.48	.480	4.32	<.05
Error (E)	83	9.18	.111		
Total (T)	167	156.11			

TABLE 37

INDIVIDUAL ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR EXPLOSIVE STRENGTH

Subj.	Mean Sq. Variance		F Ratio	Session Means					
	Sessions	Error		PD	D_1	D_2	D_3	D_4	D_5
47	.146	.008	18.25**	6.00	5.50**	5.75*	5.87	6.12	6.25*
49	.284	.020	14.20**	4.50	3.75**	4.00*	4.50	4.50	4.75
50	1.058	.034	31.12**	7.25	5.75**	6.25**	7.37	7.12	7.37
51	1.322	.014	94.43**	5.25	3.37**	3.25**	4.25**	4.62**	4.87*
52	.244	.242	1.01	6.37	6.00	5.37	5.62	5.75	6.00
53	.208	.084	2.48	6.25	5.62	6.00	5.75	6.12	7.50
55	1.008	.184	5.48*	6.37	4.50**	5.50	6.00	6.00	6.37
56	1.434	.152	9.43*	4.75	3.50*	3.87	4.62	5.50	5.62
57	.670	.116	5.78*	5.25	4.00*	4.37*	4.50	5.00	5.25
59	.282	.064	4.41	6.00	5.25*	5.25*	5.87	6.12	5.75
60	.258	.070	3.69	6.00	5.12*	5.25*	5.37	5.75	5.87
61	.928	.032	29.00**	6.37	4.87**	5.37**	6.37	6.37	6.50
63	4.020	.038	105.79**	6.00	2.50**	4.75**	5.50*	5.87	6.37
64	.516	.016	32.25**	5.62	4.62**	4.50**	5.00**	5.50	5.62
Per Cent of Men Affected (< .01 level) =					57.1	35.7	14.3	7.1	0.0

**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD

2.2.2 Analysis of Experimental Group Data. Because of the absence of a clearly definable trend in the data of the control group, the experimental group data were analyzed in their original form. This, of course, indicates that some of the changes identified in the data of this group might be related to these "normal" changes of this variable rather than to changes induced by the administration of the drug. The analysis of the group data, presented in Table 36, showed a strong session and interaction effect, and also indicated a possible effect related to replication of the test. In terms of replication, the second trial appeared to be slightly better than the first.

The analysis of each subject's data, presented in Table 37, indicated that the drug affected performance on this variable rather quickly. Any subject who showed an effect was affected at the D_1 session, but recovery was generally complete by the D_3 session.

2.2.3 Critique of the Explosive Strength Test. The use of the broad jump to assess this ability provided sufficiently reliable data, but the appearance of an interaction in the control group data would indicate that more specific instructions to the subjects might be in order. It might be possible, through the use of precise demonstrations of one given method in the orientation session, to eliminate some of the variation in the manner in which subjects attempt the broad jump. This might serve to limit the session to session changes noted for the control subjects with this variable.

2.3 Manual Dexterity

In Study I this ability was measured by use of the Minnesota Manipulation Test. In this study, use was made of a block-turning test developed by Fleishman. A fairly clear idea of the nature of the task imposed may be gained from the procedures used for testing as presented in Appendix A. The basic data recorded for each trial is included here in Table B-2.3 of Appendix B.

2.3.1 Analysis of Control Group Data. The analysis, shown in Table 38, indicated a slight interaction and a strong replication effect.

TABLE 38

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR MANUAL DEXTERITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	4	480.10	120.02		
Sessions (B)	5	28.68	5.74	1.99	>.05
Interaction (AB)	20	57.90	2.89	2.37	<.05
Replications (R)	1	12.15	12.15	9.96	<.01
Error (E)	29	35.35	1.22		
Total (T)	59	614.18			

Test-Retest Reliability: Across all Sessions = .893

For an Average Session = .871

TABLE 39

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR MANUAL DEXTERITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	9	1021.50	113.50		
Sessions (B)	5	2214.17	442.83	72.60	< .01
Interaction (AB)	45	274.40	6.10	2.93	< .01
Replications (R)	1	136.54	136.54	65.64	< .01
Error (E)	59	122.46	2.08		
Total (T)	119	3769.07			

For the latter, the second trial produced generally better performance than the first. This is similar to the "massed practice" effect noted with this ability in Study I (cf. p. 31). The slight control group interaction is also similar. In Study I, which showed a significant session effect as well, the interaction would suggest differential rates of learning among the subjects.

Here in Study II the session effect was not significant. However, the more sensitive test of comparing the PD session mean with each subsequent session mean demonstrated that differences were occurring in a relatively consistent manner for the group. The six session means were: 27.4, 28.5, 28.7, 29.0, 28.4 and 29.7, corresponding to PD through D_5 respectively. The PD mean differed at the .05 level from the D_3 mean, and at the .01 level from the D_5 mean. Again, it may be noted that the data depart from a simple learning trend because of the regression between D_3 and D_4. This corresponds to the lunch break and the "proactive inhibition" phenomenon noted in Study I between the D_2 and D_3 sessions of that study (cf. p. 33).

2.3.2 Analysis of Experimental Group Data. This analysis, as can be seen in Table 39, showed a clear session, interaction, and replication effect. The analysis was performed on the data after the basic scores were adjusted for the trend noted in the control group means. The magnitude of the effects shown in the table, large as they are, are actually underestimated since the group analysis omitted the four most seriously affected subjects. Subjects 56, 57, 63 and 64 were omitted from the group analysis because they were completely untestable on this variable at the D_2 session (90 minutes after administration of the drug).

The individual analyses in Table 40 give a clear impression of the strength and variation of the effect of scopolamine on this ability. Comparison of the per cent of men affected here with the data in Table 11 (p. 31) indicated a general similarity in results.

One additional point might be noted in Table 40. Subject 64, since he was untestable at the D_2 session, should obviously be considered to have

TABLE 40

INDIVIDUAL ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR MANUAL DEXTERITY

Subj.	Mean Sq. Variance Sessions	Error	F Ratio	Session Means PD	D_1	D_2	D_3	D_4	D_5
47	68.32	4.08	16.74**	+6.1	-4.0**	-8.2**	-2.5**	+4.6	+4.8
49	38.96	1.35	28.86**	-2.4	-10.5**	-12.2**	-8.5**	-3.4	-2.2
50	27.35	4.13	6.62*	-2.9	-10.0*	-10.7*	-9.5*	-4.9	-2.7
51	36.40	.68	53.53**	+3.6	-6.0**	-8.7**	-3.5**	-1.4**	-1.2**
52	41.19	.60	68.65**	+4.1	-4.5**	-3.7**	+.5**	+4.6	+6.3*
53	23.55	.53	44.43**	-1.9	-7.5**	-9.7**	-7.0*	-3.4	-1.2
55	62.67	2.00	31.33**	+3.1	-9.0**	-6.7**	-4.0**	+1.1	+4.8
56	37.58	.40	93.95**	+.1	-8.0**	***	-2.5*	+1.1	+3.3**
57	90.68	4.25	21.34**	+5.6	-10.0**	***	-7.5**	+.6	+2.8
59	61.59	1.00	61.59**	+8.6	-1.0**	-6.2**	+1.0**	+4.6**	+6.3
60	69.36	4.55	15.24**	+6.6	-5.0**	-10.7**	-5.5**	-3.9**	+.3
61	68.35	2.93	23.33**	+5.6	-7.0**	-8.7**	-.5*	+3.6	+2.3
63	110.93	.51	217.51**	+4.6	-14.0**	***	-10.5**	-3.4**	-1.0**
64	33.38	4.08	8.18*	+2.1	-6.5*	***	-8.0**	-3.4*	-1.0
Per Cent of Men Affected (< .01 level) =					85.7	92.8	71.4	28.6	14.3

***Untestable at this session

**Signif. at .01 level for F or difference between D_j and PD

*Signif. at .05 level for F or difference between D_j and PD

been seriously affected on this variable, even though the F ratio reported for him indicated only a mild reaction. This F ratio, significant only at the .05 level, pertains just to the data obtained from the testable sessions. Even omitting his poorest performance, and with a rather large error term, some reaction was still evident.

2.3.3 Critique of the Manual Dexterity Test. The block-turning test used in this study provided reasonably reliable data and information on the effect of the drug on this ability comparable to that obtained from the Minnesota Manipulation Test used in Study I. Since only one of these measures need be retained in the AIR battery, the Minnesota Test should probably be preferred. A number of considerations enter into this choice.

First of all, the Minnesota Manipulation Test provided somewhat more reliable data. Secondly, the block-turning test involves a simple learning of which numbers on the backs of the blocks used correspond to the numbers on the faces of the cubes; this aspect, which facilitates performance in the undrugged group, could be disrupted by the effect of a drug on short term memory. Thirdly, there is a possibility, based on the test procedure, that data from the block-turning test may be confounded with drug effects on visual acuity or accommodation more easily than data from the Minnesota test. This follows from the need, in the block-turning test, for the subjects to perceive the numerals on the faces of the blocks which they are manipulating. Finally, the control group interaction is proportionately smaller in the control group data with the Minnesota Test so that a clearer concept of changes over time is provided by that test. This, of course, provides a clearer frame of reference for the analysis of the experimental group data.

2.4 Finger Dexterity

The Purdue Pegboard Test, which was used to measure this ability, was scored in terms of the number of units (pins, collars, and washers) assembled in a fixed period of time as outlined in the test procedures in Appendix A. Table B-2.4 contains the basic data for each subject.

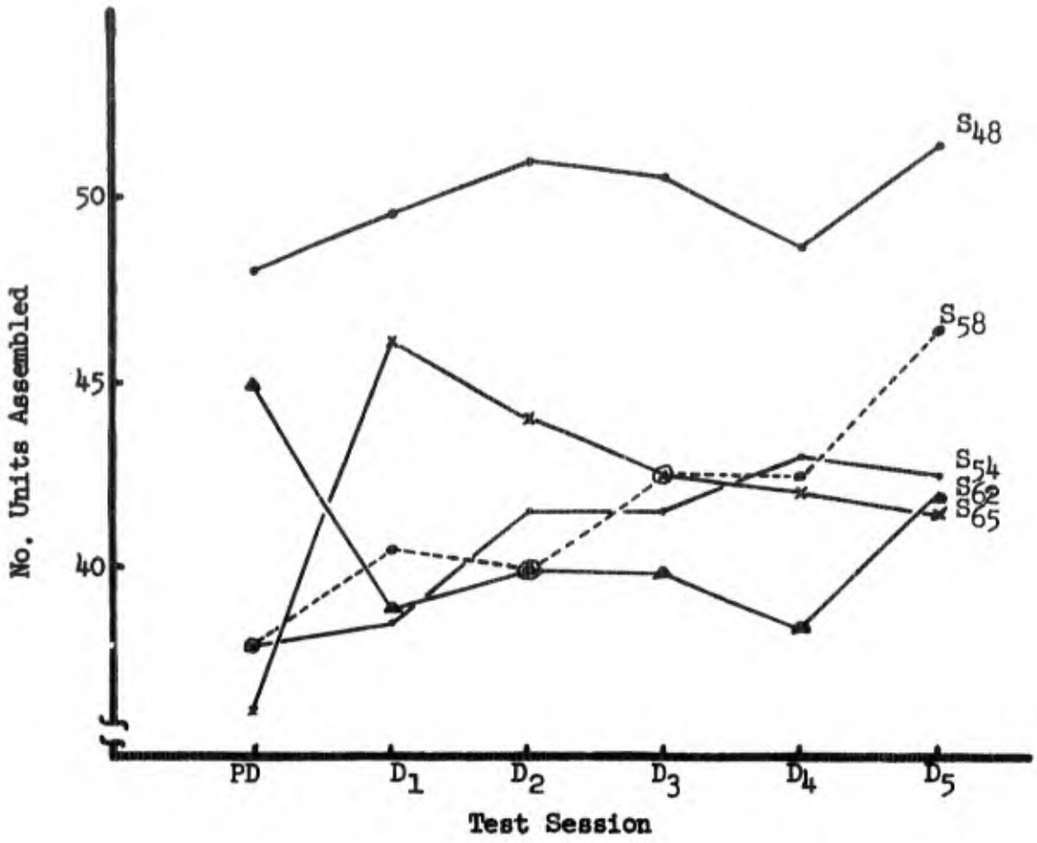


Figure 3. Data on Control Group performance showing pattern of interaction on Purdue Assembly Test of Finger Dexterity.

2.4.1 Analysis of Control Group Data. The results in Table 41 demonstrate a slight session, but a strong interaction and replication effect in the control group data. For the last effect, the second trial was generally better (more units assembled) than the first.

The basis for the interaction in the control group data is depicted in Figure 3 which shows how differently the subjects were responding over time on this test. Subjects 58 and 54 appeared to show general learning trends while subjects 62 and 65 showed patterns which might be characteristic of placebo responses. This latter impression, however, is difficult to maintain since the same impression is not borne out by the performance of these subjects on the other variables tested. Subject 48 shows an unusually high level of performance, running into the 50's, and the data trend is some odd combination of increase and decrease. In plotting the data for Figure 3 it also became appar-

TABLE 41

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR

FINGER DEXTERITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	4	729	182.25		
Sessions (B)	5	71	14.20	1.19	>.05
Interaction (AB)	20	238	11.90	4.67	<.01
Replications (R)	1	21	21.00	8.24	<.01
Error (E)	29	74	2.55		
Total (T)	59	1133			

Test-Retest Reliability: Across all Sessions = .754

For an Average Session = .705

TABLE 42

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR

FINGER DEXTERITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	12	3933	327.75		
Sessions (B)	5	8431	1686.20	56.68	<.01
Interaction (AB)	60	1785	29.75	4.14	<.01
Replications (R)	1	272	272.00	37.88	<.01
Error (E)	77	553	7.18		
Total (T)	155	14974			

ent that the reliability estimates of .754 and .705 were primarily a function of the atypical performance of Subject 48. When his data were removed from the control group analysis, the session effect disappeared but the interaction and replication effects remained. The reliability estimates dropped to essentially zero (.076 and -.109).

On the basis of data reported from large sample studies, the reliability of this test should be in the .70's (cf., e.g., Fleishman, 1954, where a reliability of .74 was found for the assembly version of this test). The sudden rise in performance by Subject 65 and the sudden drop for Subject 62 between the PD and the D_1 session represent the oddest aspect of the control group performance and contribute most to the creation of an interaction effect and very poor reliability. Their data make it almost impossible to say exactly what the changes noted in the experimental group represent. If, for example, the drug subjects were to behave in a manner similar to Subject 62 with no effect due to the drug, the initial drop of over 7 score units would represent a decrease in performance significant at the .05 level for nine of the 14 experimental subjects, and at the .01 level for four of them. These changes, confounded with actual drug related changes, would make the effect of the agent appear to be much stronger than it should be. If the experimental group, or individuals in it, were to normally behave like Subject 65, then decreases in performance from the PD level would represent far stronger effects than the data, in itself, would suggest. The unadjusted data would not fully reflect the influence of the drug.

2.4.2 Analysis of Experimental Group Data. Lacking any frame of reference for estimating what the experimental group would do had the drug not been administered, the analysis of the obtained data becomes extremely tenuous. It was carried out only because the effect of the drug, in combination with non-drug related changes, was sufficiently strong so as to force performance levels completely below any of the data observed with the experimental group.

TABLE 43

INDIVIDUAL ANALYSES FOR EACH SUBJECT OF THE EXPERIMENTAL GROUP FOR FINGER DEXTERITY

Subj.	Mean Sq. Variance Sessions	Error	F Ratio	Session Means PD	D_1	D_2	D_3	D_4	D_5
47	193	5.8	33.28**	46.0	28.5**	24.5**	38.0*	44.0	48.5
49	133	15.0	8.87*	42.0	38.5	21.5**	28.5*	33.5	43.0
50	206	6.6	31.21**	41.5	19.5**	19.0**	25.5**	21.5**	39.5
51	84	6.6	12.73**	44.5	35.0*	28.5**	31.5**	41.0	42.5
52	50	10.2	4.90	46.5	39.5	33.5**	37.5*	43.0	45.5
53	83	.6	138.33**	33.5	19.5**	18.5**	18.5**	23.5**	30.0**
55	134	6.8	19.71**	43.0	24.5**	23.5**	33.0**	36.0*	41.0
56	290	15.7	18.47**	35.0	19.0*	***	18.0*	34.5	46.5*
57	338	7.4	45.68**	47.0	26.5**	16.5**	23.5**	41.0	46.5
59	144	.6	240.00**	48.0	36.0**	24.0**	37.0**	43.0**	44.0**
60	125	9.8	12.76**	44.5	36.0*	24.5**	29.5**	29.0**	42.0
61	109	13.2	8.26*	41.5	28.5*	23.5*	35.0	40.5	40.0
63	312	1.0	312.00**	38.5	17.5**	10.0**	13.5**	36.0**	33.0**
64	133	6.6	20.15**	40.5	28.0**	20.5**	24.5**	31.5*	40.0
Per Cent of Men Affected (< .01 level) =					57.1	92.9	64.3	35.7	21.4

**Signif. at .01 level for F or difference between D_j and PD
*Signif. at .05 level for F or difference between D_j and PD
***Untestable

The general analysis, reported in Table 42, omitted the data for Subject 56 since he was untestable at the D_2 session. All effects tested were clearly significant. The analysis for each subject, contained in Table 43, showed the action of the agent (and all other sources of variation) on this ability. It should be evident that the data on the per cent of men affected cannot be construed as referring solely to the effect of the drug administered. This confounding of extraneous causes of significant changes and drug related changes stems from the interaction noted in the control group. This differs from the case where the control group data indicated only generally low reliability, as for the number-of-error scores for arm-hand steadiness, which primarily decreases the sensitivity of the test in detecting changes.

2.4.3 Critique of the Finger Dexterity Test. On the basis of a great deal of data available from many large scale studies which have employed the Purdue Pegboard Test, it is evident that the results obtained from the control group in this study should not be taken as typical of performance on this test. Some uncontrolled source of variation led to extremely poor results. The manner in which the test was administered and the manner in which test administrators functioned in the measurement of this ability should be closely examined. It is recommended that this test be included in the next drug study to be conducted and that some pilot testing be carried out on undrugged subjects to try to determine why the changes observed would occur.

2.5 Multi-Limb Coordination

The two-hand coordination test was used to measure the multi-limb coordination ability. The basic data, presented in Table B-2.5 of Appendix B, is expressed in decimal parts of a minute. The score also indicates the proportion of the time in a one-minute trial that the subject remained on the test target.

2.5.1 Analysis of Control Group Data. Only four of the five control subjects could be utilized in this analysis because, as Table B-2.5 indicates,

TABLE 44

ANALYSIS OF CONTROL GROUP DATA FOR MULTI-LIMB COORDINATION

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	3	1.32	.44		
Sessions (B)	5	.71	.14	4.67	<.01
Interaction (AB)	15	.47	.03	10.00	<.01
Replications (R)	1	.01	.003	1.00	>.05
Error (E)	69	.21	.003		
Total (T)	95	2.72			

Test-Retest Reliability: For All Sessions = .746

For an Average Session = .695

the first subject (S_{48}) in this group as well as three from the experimental group (S_{47}, S_{49} and S_{50}) were given only three replications per trial while the remainder of the subjects were given four per trial. Since a strong learning factor became evident, this increasing difference in number of trials across sessions would preclude treating these four subjects as comparable to the remainder of the sample in either group.

The analysis of the data on the four control subjects, reported in Table 44, showed a clear session effect, but the predominant characteristic was the very strong interaction effect. Figure 4 is a graph of this interaction pattern showing four fairly different control subject trends. Subject 65 essentially mastered the control task during the first day of training and then maintained a near perfect performance during the second day. Subjects 62 and 54 learned different amounts during the first day, but both reached almost perfect performance by D_5 so that they displayed different rates of

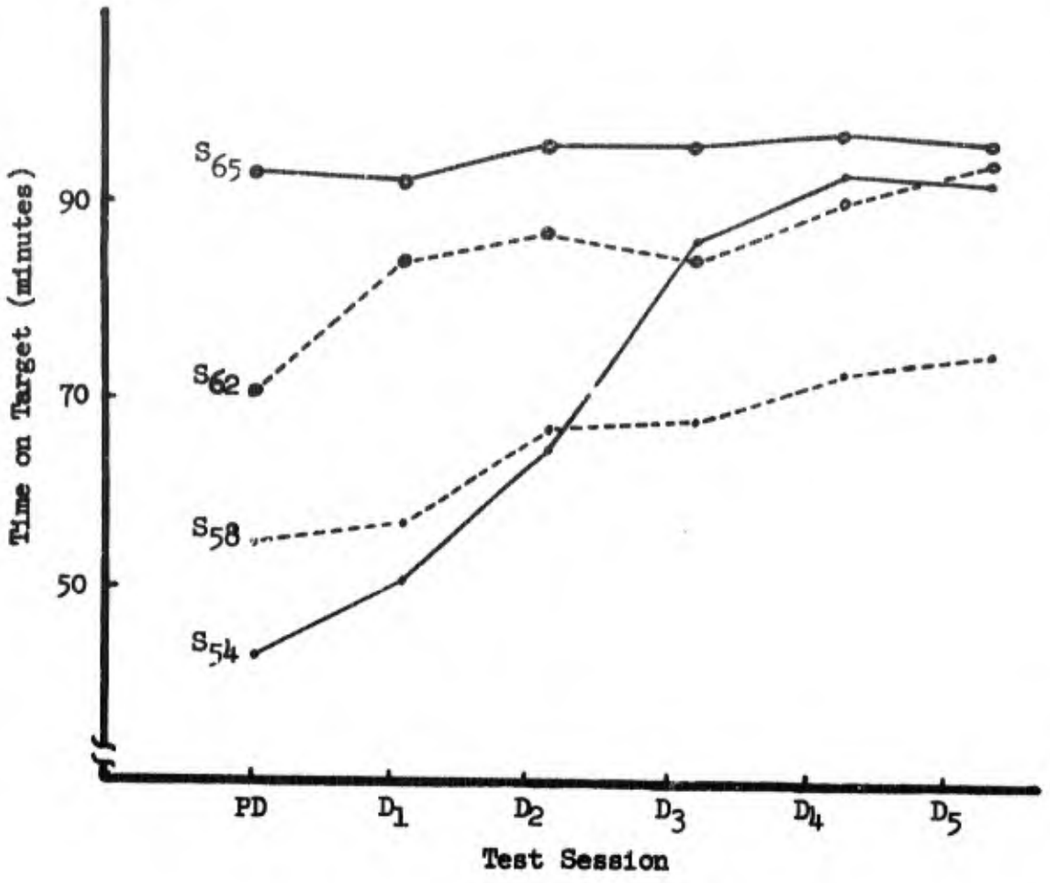


Figure 4. Pattern of Control Group Interaction on the Two-Hand Coordination Test of Multi-limb Coordination

learning during the second day of testing. Subject 58 did not acquire too much skill on this task during the first day, nor did he progress much throughout the second day.

The control group interaction, then, is a function of the differential rates of learning exhibited by the control subjects, and it is strengthened by the fact that the test, as administered here, had an achievable upper limit of performance which restricted the level of the better subjects. These differences are so pronounced that a meaningful analysis of the experimental group data was considered to be difficult. Of course, it can be argued on the basis of the data available that any drop in performance below the PD level is probably related to the drug, but it would be impossible to estimate the significance of the size of any particular decrease since its appropriate level without the drug is unknown.

2.5.2 Experimental Group Data. In general, all of the experimental group subjects did show a decrease in performance from their PD levels, especially from the D_1 to D_3 sessions. Therefore, the drug did affect this ability. Any further analysis would be speculative. It might be observed, though, that if some fixed criterion, related to military performance, were available such that levels below something like .50 represented incapacitation for certain skills, further analysis could be carried out and per cent of men affected determined on that basis.

2.5.3 Critique of the Multi-Limb Coordination Test. In its present form, and within the time restriction of a two-day assessment schedule, this test would have to be classified as unusable for this type of research. If it were to be used, sufficient time would have to be allowed prior to the introduction of experimental conditions of interest to allow each subject to achieve a level of performance which would remain relatively stable throughout a full day of testing. This, of course, would vary from subject to subject and would probably be far too time consuming for inclusion in a general test battery. On this basis, it is recommended that the test be dropped from the general battery.

2.6 Dynamic Flexibility

This ability, in the general category of physical proficiency, was measured by what Fleishman (1964, p. 79) calls the "Bend, Twist, and Touch Test." The data, in terms of the number of times the subject could complete the cycle of physical movements outlined in the test procedure, are contained in Table B-2.6.

2.6.1 Analysis of Control Group Data. This test, as implemented for Study II, did not include provision for replication within a given test session. In view of this, the analysis, shown in Table 45, could only establish that there was no significant change in performance over time. Test reliability was estimated as being quite high (.961 and .953).

TABLE 45

ANALYSIS OF VARIANCE OF CONTROL GROUP DATA FOR DYNAMIC FLEXIBILITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	4	907	226.75		
Sessions (B)	5	7	1.40	.76	>.05
Error (AB)	20	37	1.85		
Total (T)	29	951			

Test-Retest Reliability: Across all Sessions = .961

For an Average Session = .953

TABLE 46

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUP DATA FOR DYNAMIC FLEXIBILITY

Source	Degrees of Freedom	Sums of Squares	Mean Squares	F	P
Subjects (A)	13	1912	147.08		
Sessions (B)	5	1157	321.40	36.50	<.01
Error (AB)	65	412	6.34		
Total (T)	83	3481			

2.6.2 Analysis of Experimental Group Data. The analysis of the experimental group data, reported in Table 46, indicated that the drug had a strong effect on this ability, but a test of the interaction or an analysis of the individual subjects could not be carried out since there was no replication of the test. In general, a change in score units of 10.5 would be considered as significant at the .05 level for the group means, and a change of 12.6 at the .01 level. An examination of the data would suggest that the subjects did differ in the patterns of their reactions, but a detailed estimate of per cent of men affected cannot be adequately computed.

2.6.3 Critique of the Dynamic Flexibility Test. A full evaluation of the utility of this test in the general test battery would require that some form of replication be included with its use. It is recommended that this be implemented as soon as possible, and that decisions regarding the use of the test be delayed until such data become available.

2.7 Dynamic Strength

It was intended that this ability be measured by having the subjects complete as many "pull-ups" as possible. Unfortunately, with the men tested for this study, three of the five control subjects in all test sessions, and six of the fourteen in the experimental group at the PD session could only complete one or two. Obviously, measures of change from these levels would be rather pointless. On this basis, the test must be classified as too difficult for use in the general battery, and some simpler test of this ability should be developed. One possible recommendation could be Fleishman's (1964, p. 50) "Bent Arm Hang Test" in which the subject goes into a partially chinned position and holds himself there as long as possible.

2.8 General Assessment of Study II

The data analyses from this study have shown that two of the tests, track-tracing for arm-hand steadiness, and block-turning for manual dexterity, are

usable in their present form. With some modification, three of the remaining five (broad jump, Purdue Pegboard, and the bend, twist and touch test) should also become usable. The two-hand coordination test should probably be dropped because of the extensive learning period which would be required to stabilize performances. The "pull-ups" test should be dropped since it is apparently too difficult to provide useful data.

The major goal of both Study I and II was the identification of useful tests for evaluating human performance so as to determine those abilities which are and which are not affected by chemical agents. The utility of each test was largely determined by the characteristics of the data which it provided. However, it should be remembered that these data characteristics are determined by many factors in the experimental situation including the test administrator, his instructions, the test apparatus, and the scoring system. Modification in one or more of these factors may make the difference between useful and useless data. It is, therefore, a matter of some importance to determine as precisely as possible just which factors are most critical in creating a particular data distribution. The analyses conducted so far are the first steps. Perhaps their greatest value has been to provide guidance as to where to look further. Suggestions for this "further look" constitute the content of the concluding chapter of this report.

CHAPTER IV

SUMMARY AND IMPLICATIONS FOR FUTURE STUDIES

The primary concern of the preceding analyses was to examine each of the tests used to determine the adequacy of the data obtained from them and to recommend changes where these appeared necessary. These recommendations are summarized in Tables 47 and 48 for the two studies.

In accordance with the basic purpose of these studies, there was as much concern with the behavior of the control group as with the effect of the drug on the experimental group. The intent, in administering scopolamine to the experimental group, was not so much to study the effect of this agent on the modes of behavior measured, but to make use of an agent already known to be effective in modifying human performance so that any problems in administering the tests to drugged subjects would be identified. Based on the extensive experience of the Edgewood Arsenal personnel in this area, many obvious modifications in standard procedures and apparatus were made before these studies were carried out. Primarily as a result of this, little difficulty in test procedure with the drugged subjects was encountered.

However, these necessary changes in test administration also imply that data on reliability and validity drawn from the standardized forms of the tests may not be directly applicable in these studies. Through continued use, sufficient control group data must eventually be acquired so as to determine the effect of test modification on both reliability and validity for each test retained in the final version of the test battery. The data gathered and reported on here, of course, being based on very small samples, should not be construed as proving or disproving test reliabilities. The results are merely suggestive, and further data must be gathered.

One of the most important implications of these studies for future work is in the area of the structure of the experimental design which has been employed. Ideally, in keeping with the ultimate use to which the test battery is

TABLE 47

Recommendations for Modifications of Tests Employed in Study I Based on Detailed Analyses of Performance Scores

Ability Measured	Test Name	Estimated Reliability[1]	Major Problems Noted	Recommended Changes
1.1 Far Vis. Acuity	ORTHORATER	.92	Inadequate ceiling	Develop different test of acuity to adequately test "better than average" subjects.
1.2 Near Vis. Acuity	ORTHORATER	.95	" "	
1.3 Manual Dexterity	MINN. MANIPULATION	.96	None	None
1.4 Static Strength	HAND DYNOMOMETER	.98	Control Group Interaction	Consider different test.
1.5 Gross Body Equilib.	BALANCE A	----	Inadequate ceiling	Extend trial time to 1 minute and/or require subjects to keep hands on hips.
1.6 Number Facility	ADDITION	.8?	a. No replication within trials b. Untestable cases related to loss of visual acuity	a. Score odd vs. even items for number correct on each form. b. Modify test or treat Subjects to offset visual acuity losses observed.
1.7 Short Term Memory	AUDITORY NO. SPAN	.75	Scoring system may be too gross	a. Separate test items to form two series for each trial. b. Extend items to an 11 unit group; drop 3 unit group.
1.8 Time Estimation	EMPTY INTERVAL JUDGMENT	.72	Strong Control Group Interaction	Improve training by continuing to provide knowledge of results in all sessions of first day's testing.
1.9 Reaction Time	SIMPLE VISUAL REACTION TIME	.71	None	None

[1] Average single session estimated reliability based on intra-class correlation coefficients as computed from control subject data.

TABLE 48

Recommendations for Modifications of Tests Employed in Study II Based on Detailed Analyses of Performance Scores

Ability Measured	Test Name	Estimated Reliability[1]	Major Problems Noted	Recommended Changes
2.1a Arm-Hand Steadiness	TRACK TRACING-Time in error	.80	None	None
2.1b Arm-Hand Steadiness	TRACK TRACING-Number of errors	.20	Lacks reliability	Eliminate from battery if confirmed in next study.
2.2 Explosive Strength	BROAD JUMP	.86	Control Group Interaction	Give more explicit instructions to subjects.
2.3 Manual Dexterity	BLOCK TURNING	.86	None	Drop in favor of Minn. Manip. for better reliability.
2.4 Finger Dexterity	PURDUE PEGBOARD	.70	Poor Control Group Data	Use same form in next study.
2.5 Multilimb Coordination	TWO-HAND COORDINATION	.69	Strong Control Interaction	Eliminate from battery; would require extensive pre-training.
2.6 Dynamic Flexibility	BEND, TWIST & TOUCH	.95	No replication	Replicate with two 20-second trials at each session.
2.7 Dynamic Strength	PULL-UPS	----	Test too difficult for some undrugged subjects	Drop from battery; substitute Bent-Arm Hang Test.

[1] Average single session estimated reliability based on intra-class correlation coefficients as computed from control subject data.

to be put, use of these tests should permit the military to make unqualified statements about the effect of a chemical agent on a broad range of human abilities. The consistent appearance in the experimental group data, with every variable tested, of a subject-by-test-session interaction clearly makes such unqualified statements impossible. As has been noted, the presence of the interaction demonstrates that the drug effect cannot be stated independently of the subjects who were tested. In an ideal case, the interaction effect should be essentially zero, i.e., the mean square variance for the interaction in the analyses should be approximately equal to that of the sampling error.

A general subject-by-treatment interaction, where "treatment" can refer to a time or a dosage variable, is quite common in psychopharmacology (cf., e.g., Kornetsky, Humphries, & Evarts, 1957). In its simplest form, it indicates that an additive model such as $X_{ij} = A_i + B_j$ cannot be used to "explain" the score obtained, that some significant, non-additive effect must be tacked on to each unique combination of the A_i's (subject effects) and B_j's (treatment effects) as in the form of the model used in these analyses: $X_{ij} = A_i + B_j + AB_{ij}$. This, in turn, can mean that some significant, uncontrolled source of variation may be present which, if accounted for either in the experimental design as a third factor or as a covariate adjustment on the data obtained, could reduce the interaction term to negligible proportions.

If the variance which is unaccounted for in the simple additive model is related to organismic factors such as personality, biochemical individuality, variations in organ size or functioning, etc., then the variance encountered is properly a part of the overall effect of the drug when it is applied to a randomly selected population of subjects. However, it then becomes somewhat deceptive to report data in terms of the mean level of performance of the group. Significant differences are occurring among subjects so that more than one kind of population is being sampled at a given point in time. A single mean does not reflect this. For the sake of simplicity, if only two "types" of subjects were

being sampled, affected and unaffected, the single mean might fall between them and be representative of neither. Properly, the drug effect as such would be the mean of the affected group only. This is why, from a tactical standpoint, it appears more relevant to indicate the percentage of men who show an effect at each point in time rather than to deal with the overall means of the experimental groups. These should, almost invariably, be underestimates of the means of the drug as such. The problem, of course, is much more complex than the simple dichotomy used above since there are obviously varying degrees of affectedness.

If, on the other hand, the variance which is unaccounted for is related to stimulus or environmental factors such as minute but important variations in the effective amounts of the agent administered, or minor variations in test procedures or behavior of test administrators, these must be identified and better controlled. Their influence must be eliminated if a "pure" measure of drug effect is to be obtained.

Whatever the basis of the interaction is, it should be evident that the major sources of variation not accounted for in these studies must be identified and quantified if future studies are to produce more satisfactory results, if clear statements regarding the effect of the agent are to be made. It is the failure to appreciate the meaning of an interaction effect which is one of the basic causes of confusion in results reported in psychopharmacological literature. If the results for the experimental groups in these studies had been reported simply in terms of group means at each point in time, a second study with exactly the same tests but different subjects could produce significantly different results. This follows from the fact that the effects obtained are uniquely tied to the specific subjects tested as a function of the time of the testing. For this reason, two apparently similar studies can even produce contradictory results.

One step toward discovering the nature of the interactions encountered in these studies has already been suggested in terms of examining the inter-correlations among subjects at the various time points to see if consistent types of subjects can be identified (cf. p. 56). If they can be, then related aspects of personality, biochemical individuality, etc., might be associated with these interactions.

The question of whether or not "equally effective doses" of an agent are really administered as a simple function of body weight has already been the subject of several discussions between EARL and AIR personnel. If this is too simple a schema for psychoactive chemicals, then a more precise concept should be developed. Alternatively, and possibly more usefully from the military standpoint, it might be better to administer a fixed amount of an agent (which is what any weapon system would deliver) specifically to study how subject performance as a function of weight is related to fixed doses. This would also permit the examination of a great many other subject-related variables at the same time, and may therefore provide a much more direct route to the nature of the subject-by-treatment interaction.

In another area, there is a general question, as these studies progress, concerning just how many abilities must be assessed to be able to specify the effect of a chemical agent on "human performance." Of the five broad categories of human performance (physical proficiency, psychomotor, sensory-perceptual, cognitive, and social), how many ability tests must be included under each to assess, from a practical standpoint, the full range of the effect of an agent? The answer to this obviously lies in the degree of relatedness among the many abilities which psychologists have already identified. While most of these may be more or less unrelated or independent in an undrugged population, a broad range of them can be similarly affected by a given agent. If this should hold true across a large number of agents of interest, then only a few differentially affected abilities would require measurement. This,

however, is something which is difficult to prejudge. An answer can be determined only after the full range of abilities have been tested.

The graphs on p. 55 show that, to some extent, different abilities were differentially affected by scopolamine. Knowing the effect, for example, on near visual acuity for Subject 10 (p. 27) provides almost no information about the effect on his manual dexterity (p. 31). By the same token, as was noted in Study I, knowledge of the effect of the agent on near visual acuity does provide some knowledge of performance of subjects on the number facility test if the eye condition is untreated.[5]

As a general illustration, if the magnitude of reaction to the drug were subject-related and non-specific with respect to the variables measured, then the magnitude of the general F ratios reported for each of the experimental subjects should reflect this common type of reaction pattern. The F ratios provided by seven of the tests administered in Study I were rank ordered within each variable, and the "number of failures" as defined for gross body equilibrium and number facility were also used to rank order the subjects within those tests. The resulting series of ranks were intercorrelated and provided the data reported in Table 49.

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5. Thus, what was shown in Study I was the effect of scopolamine on the ability of the subject to perceive typewritten numbers and then to add the numbers perceived. Number facility as such, e.g., in terms of mental addition, may have been only slightly affected or even totally unaffected.

TABLE 49

INTERCORRELATIONS AMONG TESTS FROM STUDY I BASED ON RANK-ORDERINGS OF MAGNITUDES OF SUBJECT'S REACTIONS

Ability	2	3	4	5	6	7	8	9
1. Far Acuity	.28	.63*	-.39	-.21	.24	.51	.21	.10
2. Near Acuity		.02	-.25	.04	.66*	-.15	-.05	.58*
3. Manual Dexterity			-.36	.27	.02	.35	-.14	-.09
4. Static Strength				.39	-.38	.01	.10	.00
5. Gross Body Equilibrium					-.03	-.06	-.28	.05
6. Number Facility						-.24	-.14	.48
7. Short Term Memory							.75**	.40
8. Time Estimation								.51
9. Reaction Time								

*Signif. at or beyond the .05 level
**Signif. at or beyond the .01 level

If subjects generally reacted in a similar manner across all tests, Table 49 would reflect a high degree of intercorrelation among all of the tests. It is evident in the Table that such is not the case. There are only four correlations which differ significantly from zero at or beyond the .05 level. These are the correlations between far acuity and manual dexterity, near acuity and number facility, near acuity and reaction time, and short term memory and time estimation. The common basis between visual acuity and tasks requiring vision is obvious enough; that between short term memory and time estimation would also appear reasonable, but establishing its theoretical basis could lead to some intriguing speculation.

Table 49 illustrates how correlational analyses can begin to bring together the many separate findings reported in these studies. There are many types of correlational and multivariate analyses which must eventually be performed before most of the questions in this area can be fully resolved.

Further examinations of the intercorrelations among the data reported on here will be continued, and reported on in the future. This is being done not so much because the amount of data really warrants this type of treatment, but to develop suggestions for the adequate planning of future studies and the development of basic methods for coping with the gradually increasing amounts of data that will be accumulated.

In summary, then, it is recommended that future efforts be directed toward:

1. identifying the factor or factors which produced the experimental group interactions, especially as to whether these are organismic or stimulus parameters, and developing means of controlling for them.
2. studying the intercorrelations among subjects within the tests to determine if types of subjects can be identified.
3. studying the relationship among tests within subjects to determine if common reaction patterns can be identified, permitting the elimination of redundancy in the test battery.

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APPENDIX A

INSTRUCTIONS FOR ADMINISTERING TESTS USED IN STUDY II

ARM-HAND STEADINESS TRACK TRACING

ORIENTATION: Instructions for the subject

THIS IS A TEST TO SEE HOW STEADILY YOU CAN MOVE YOUR ARM IN A PATTERN. YOUR TASK IS TO CAREFULLY MOVE THIS STYLUS THROUGH THIS SLOT. YOU ARE TO DO THIS AS RAPIDLY AS YOU CAN WITHOUT ALLOWING THE STYLUS TO TOUCH THE TIP, BOTTOM OR SIDES OF THE SLOT (demonstrate).

EVERY TIME THE STYLUS TOUCHES ANY PART OF THE METAL PLATE AROUND THE SLOT ERRORS WILL AUTOMATICALLY BE COUNTED AGAINST YOU, SO TRACE THE PATTERN AS CAREFULLY AS YOU CAN.

WHEN YOU GET TO THE END OF THE SLOT, PUSH IN ON THE BUTTON WITH THE STYLUS (demonstrate). AND THEN RETRACE THE PATTERN WITHOUT REMOVING THE STYLUS FROM THE SLOT TILL YOU GET BACK TO THE STARTING POSITION.

REMEMBER, IT IS IMPORTANT THAT YOU MOVE STEADILY ENOUGH SO THAT YOU DON'T TOUCH ANY PART OF THE SLOT.

WHEN I SAY "READY", PLACE THE STYLUS IN THE HOLE, WHEN I SAY "GO", TRACE THE PATTERN TO THE OTHER END AND THEN RETURN TO THE STARTING POINT. DO NOT REMOVE THE STYLUS FROM THE SLOT UNTIL YOU HAVE RETURNED TO THE STARTING POINT.

DO YOU HAVE ANY QUESTIONS?

LET'S TRY A PRACTICE TRIAL.

-------PRACTICE------- (one trace)

(Correct any errors in the S's procedure.)

OK, WE'LL NOW HAVE A SHORT REST PERIOD.

-------REST------- (30 seconds)

ARM-HAND STEADINESS TRACK TRACING

ORIENTATION: Instructions for the subject (cont.)

OK, LET'S START THE FIRST TRIAL. READY? GO!

-------TEST------- (one trace)

WE'LL HAVE ANOTHER REST PERIOD.

-------REST------- (30 seconds)

OK, LET'S START THE SECOND TRIAL. READY? GO!

-------TEST------- (one trace)

THAT IS THE END OF THIS TEST.

ARM-HAND STEADINESS TRACK TRACING

BASELINE: Instructions for the subject

DO YOU REMEMBER THIS TEST?

DON'T FORGET TO MOVE STEADILY AND CAREFULLY THROUGH THE SLOT, NEVER TOUCHING THE SIDES OR BOTTOM. BE SURE TO PUSH THE BUTTON AT THE END OF THE SLOT AND THEN RETRACE WITHOUT REMOVING THE STYLUS TILL YOU RETURN TO THE STARTING POSITION.

OK, LET'S START THE FIRST TRIAL.

READY?

GO!

-------TEST------- (one trace)

OK, WE'LL NOW HAVE A REST PERIOD.

-------REST------- (30 seconds)

OK, LET'S START THE SECOND TRIAL.

READY?

GO!

-------TEST------- (one trace)

THAT IS THE END OF THE TEST.

ARM-HAND STEADINESS — TRACK TRACING

EXPERIMENTAL: Instructions for the subject

DO YOU REMEMBER THIS TEST?

DON'T FORGET TO MOVE STEADILY AND CAREFULLY THROUGH THE SLOT, NEVER TOUCHING THE SIDES OR BOTTOM. BE SURE TO PUSH THE BUTTON AT THE END OF THE SLOT AND THEN RETRACE THE PATTERN WITHOUT REMOVING THE STYLUS TILL YOU RETURN TO THE STARTING POSITION.

OK, LET'S START THE FIRST TRIAL.

READY?

(If the S does not get ready, repeat three times: HOLD THE STYLUS or PUT THE STYLUS IN THE SLOT. If necessary, place the stylus in his hand.)

GO!

(If the S does not respond when you say "GO", repeat a maximum of three times: START TRACING THE PATTERN WITH THE STYLUS. If the S still does not respond, he will be considered untestable).

-------TEST------- (one trace)

OK, WE'LL HAVE A REST PERIOD NOW.

-------REST------- (30 seconds)

OK, LET'S START THE SECOND TRIAL.

READY? (same instructions)

GO! (same instructions)

-------TEST------- (one trace)

THAT IS THE END OF THE TEST.

EXPLOSIVE STRENGTH — STANDING BROAD JUMP

ORIENTATION: Instructions for the subject

THIS IS A TEST TO MEASURE HOW FAR YOU CAN JUMP. YOU WILL STAND WITH YOUR TOES DIRECTLY BEHIND THIS LINE (point). WHEN I SAY "READY", PLACE YOUR FEET ABOUT ONE FOOT APART, BEND YOUR KNEES AND GET SET TO JUMP LIKE THIS (demonstrate correct starting position).

WHEN I SAY "GO", JUMP AS FAR AS YOU CAN, KEEPING BOTH LEGS TOGETHER AND LANDING ON BOTH FEET (demonstrate).

YOUR SCORE WILL BE HOW FAR YOU CAN JUMP. THE DISTANCE YOU JUMP WILL BE MEASURED FROM THE POINT YOUR HEELS TOUCH CLOSEST TO THE STARTING LINE. TRY NOT TO FALL, IF YOU ARE GOING TO FALL, FALL FORWARDS.

DO YOU HAVE ANY QUESTIONS?

OK, LET'S TRY A PRACTICE TRIAL.

READY?

GO!

-------PRACTICE------- (one jump)

(Watch the subject to be sure he is responding properly. Give any help needed on the practice trial.)

(Tell the S his score. Record all scores to the nearest 3 inches.)

(If S does not jump within the area outlined for the broad jump, repeat that particular trial one more time. If S jumps a distance less than one foot, record NT for his score; likewise, if he does not jump within the area outlined twice on a particular trial, record an NT.)

OK, LET'S START THE FIRST TRIAL

EXPLOSIVE STRENGTH — STANDING BROAD JUMP

ORIENTATION: Instructions for the subject (cont.)

READY?

GO!

-------TEST------- (one jump)

WE'LL NOW HAVE A SHORT REST PERIOD.

-------REST------- (30 seconds)

OK, LET'S START THE SECOND TRIAL.

READY?

GO!

-------TEST------- (one jump)

THAT IS THE END OF THE TEST.

EXPLOSIVE STRENGTH — STANDING BROAD JUMP

BASELINE: Instructions for the subject

DO YOU REMEMBER THIS TEST?

DON'T FORGET, KEEP BOTH LEGS TOGETHER AND TRY TO LAND ON BOTH FEET.

REMEMBER, TRY NOT TO FALL.

OK, LET'S START THE FIRST TRIAL.

READY?

GO!

-------TEST ------ (one jump)

WE'LL HAVE A SHORT REST PERIOD.

-------REST------- (30 seconds)

OK, LET'S START THE SECOND TRIAL.

READY?

GO!

-------TEST------- (one jump)

THAT IS THE END OF THE TEST.

EXPLOSIVE STRENGTH — STANDING BROAD JUMP

EXPERIMENTAL: Instructions for the subject

DO YOU REMEMBER THIS TEST?

DON'T FORGET, KEEP BOTH LEGS TOGETHER AND TRY TO LAND ON BOTH FEET.

REMEMBER, TRY NOT TO FALL.

OK, LET'S START THE FIRST TEST.

READY?

(If the S does not get set, repeat a maximum of three times: COME ON NOW, GET READY TO JUMP. If necessary, place S in the starting position.

GO!

(If the S does not respond when you say "GO", repeat a maximum of three times: LET'S GO, JUMP FORWARD AS FAR AS YOU CAN. If he still does not respond, he will be considered untestable.)

-------TEST------- (one jump)

WE'LL NOW HAVE A SHORT REST PERIOD.

-------REST------- (30 seconds)

OK, LET'S START THE SECOND TEST

READY? (same instructions)

GO! (same instructions)

-------TEST------- (one jump)

THAT IS THE END OF THE TEST.

MANUAL DEXTERITY — BLOCK MANIPULATION

ORIENTATION: Instructions for the subject

(To start the test, the blocks should be in the lower right hand corner with the orange side up.)

THIS IS A TEST OF YOUR MANUAL DEXTERITY. YOUR TASK WILL BE TO MOVE THESE SIX BLOCKS FROM ONE COMPARTMENT TO THE NEXT AS RAPIDLY AS POSSIBLE, MOVING CLOCKWISE AROUND THE BOX (indicate direction). YOU WILL HAVE ONE MINUTE TO MOVE THE BLOCKS AS FAR AS YOU CAN.

AS YOU CAN SEE, EACH BLOCK HAS A NUMBER AND AN ORANGE STRIP ACROSS THE TOP. ON THE REVERSE SIDE THERE IS A BLACK NUMBER AND A BLACK STRIP. THE COLOR ON THE SIDE OF THE BLOCK FACING UP MUST ALWAYS CORRESPOND TO THE COLOR OF THE COMPARTMENT IN WHICH IT IS PLACED. THE BLOCK WITH THE NUMBER 1 ON THE ORANGE SIDE DOES NOT NECESSARILY HAVE NUMBER 1 ON THE REVERSE OR BLACK SIDE.

YOU MUST ALWAYS MOVE THE BLOCKS ONE AT A TIME, IN ORDER, STARTING WITH #1 AND THEN #2. FOR EXAMPLE, BEFORE YOU GO BACK FOR #2, THE FIRST BLOCK MUST HAVE BEEN PLACED IN THE CORRECT POSITION CORRESPONDING TO THE NUMBER ON THE RESIDE (pick up blocks one and two and demonstrate the procedure). IF YOU MAKE A MISTAKE, BE SURE TO CORRECT IT BEFORE CONTINUING FURTHER.

REMEMBER THAT THE BLOCKS SHOULD READ 1-2-3 ACROSS THE TOP AND 4-5-6 ACROSS THE BOTTOM IN EACH COMPARTMENT. THE STRIPES SHOULD ALWAYS BE AT THE TOP OF EACH BLOCK AND THE COLOR OF THE BLOCK FACING UP MUST CORRESPOND TO THE COLOR OF THE COMPARTMENT (indicate).

MANUAL DEXTERITY — BLOCK MANIPULATION

ORIENTATION: Instructions for the subject (cont.)

WHEN YOU HAVE FILLED UP ONE COMPARTMENT, GO RIGHT ON TO THE NEXT AS RAPIDLY AS POSSIBLE. PROCEED FROM ONE COMPARTMENT TO THE NEXT (indicate direction) UNTIL I SAY "STOP".

ALL THE BLOCKS MUST BE MANIPULATED IN THE AIR BEFORE THEY ARE PLACED IN THE COMPARTMENT (demonstrate). YOU MAY USE EITHER HAND, BUT YOU MUST USE THE SAME HAND THROUGHOUT THE TESTS. WHICH HAND WOULD YOU PREFER? (record this on the data sheet.)

DO YOU HAVE ANY QUESTIONS? BE CERTAIN THAT EACH BLOCK IS IN ITS PROPER POSITION BEFORE REACHING FOR THE NEXT ONE. REMEMBER, WORK AS RAPIDLY AS POSSIBLE, SINCE SPEED IS IMPORTANT IN GETTING A GOOD SCORE.

OK, LET'S TRY A PRACTICE TRIAL.

READY?

(Subject should place hand on first block, but should not lift it.)

GO!

-------PRACTICE------- (one minute)

STOP!

PLEASE REPLACE THE BLOCKS IN THEIR ORIGINAL POSITION WHILE WE HAVE A REST PERIOD.

-------REST------- (30 seconds)

OK, LET'S START THE FIRST TRIAL.

MANUAL DEXTERITY — BLOCK MANIPULATION

ORIENTATION: Instructions for the subject (cont.)

READY?

GO!

-------TEST------- (one minute)

STOP!

(Record the total number of blocks moved.)

PLEASE REPLACE THE BLOCKS.

-------REST------- (30 seconds)

OK, LET'S START THE SECOND TRIAL.

READY?

GO!

-------TEST------- (one minute)

STOP!

(Record the total number of blocks moved.)

PLEASE REPLACE THE BLOCKS.

THAT IS THE END OF THIS TEST.

MANUAL DEXTERITY — BLOCK MANIPULATION

BASELINE: Instructions for the subject

REMEMBER TO USE YOUR (RIGHT) (LEFT) HAND ONLY AND GO AS FAST AS YOU CAN. BE SURE TO PLACE THE BLOCKS CORRECTLY BEFORE REACHING FOR THE NEXT BLOCK, AND ALWAYS TAKE THE BLOCKS IN ORDER.

DO YOU HAVE ANY QUESTIONS?

OK, LET'S START THE FIRST TRIAL.

READY?

GO!

-------TEST------- (one minute)

STOP!

PLEASE REPLACE THE BLOCKS

-------REST------- (30 seconds)

OK, LET'S START THE SECOND TRIAL.

READY?

GO!

-------TEST------- (one minute)

STOP!

PLEASE REPLACE THE BLOCKS.

THAT IS THE END OF THE TEST.

MANUAL DEXTERITY — BLOCK MANIPULATION

EXPERIMENTAL: Instructions for the subject

REMEMBER TO USE YOUR (RIGHT) (LEFT) HAND ONLY AND GO AS FAST AS YOU CAN. BE SURE TO PLACE THE BLOCKS CORRECTLY BEFORE REACHING FOR THE NEXT BLOCK AND ALWAYS TAKE THE BLOCKS IN ORDER.

ANY QUESTIONS?

READY?

(If the S does not get ready by placing the correct hand on the first block, repeat a maximum of three times: PUT YOUR HAND ON THE BLOCK. If necessary, place his hand on the first block.)

GO!

(If the S does not begin, repeat a maximum of three times: START MOVING THE BLOCKS TO THE NEXT COMPARTMENT. If he still does not respond correctly, consider him untestable.)

-------TEST------- (one minute)

STOP! PLEASE REPLACE THE BLOCKS.

-------REST------- (30 seconds)

OK, LET'S START THE SECOND TEST.

READY?

GO!

-------TEST------- (one minute)

STOP! PLEASE REPLACE THE BLOCKS.

THAT IS THE END OF THIS TEST.

FINGER DEXTERITY — PURDUE ASSEMBLY

ORIENTATION: Instructions for the subject

THIS IS A TEST TO SEE HOW QUICKLY AND ACCURATELY YOU CAN WORK WITH YOUR HANDS.

(Left-handed subjects will receive the same instructions, except that the opposite hand, e.g., the word in parentheses, will be used.)

(Demonstrate as you say the following:)

IF YOU ARE RIGHT (LEFT) HANDED, YOU WILL PICK UP A PIN FROM THE RIGHT (LEFT) HAND CUP WITH YOUR RIGHT (LEFT) HAND AND PLACE IT IN THE TOP HOLE OF THE RIGHT (LEFT) COLUMN.

AS YOU ARE DOING THIS, PICK UP A WASHER WITH YOUR LEFT (RIGHT) HAND. AS SOON AS THE PIN HAS BEEN PLACED IN THE HOLE, DROP THE WASHER OVER THE PIN.

WHILE THE WASHER IS BEING PLACED WITH THE LEFT (RIGHT) HAND, PICK UP A COLLAR WITH YOUR RIGHT (LEFT) HAND. PLACE THIS OVER THE PIN AND WASHER.

AS YOU ARE DOING THIS, PICK UP ANOTHER WASHER WITH YOUR LEFT (RIGHT) HAND AND DROP IT OVER THE PIN AND COLLAR. THIS COMPLETES A SINGLE ASSEMBLY.

AS SOON AS THE FINAL WASHER IS BEING PLACED WITH THE LEFT (RIGHT) HAND, START THE SECOND ASSEMBLY BY TAKING ANOTHER PIN WITH YOUR RIGHT (LEFT) HAND AND PLACING IT IN THE NEXT LOWER HOLE.

AS YOU ARE DOING THIS, TAKE ANOTHER WASHER WITH YOUR LEFT (RIGHT) HAND AND PLACE IT ON THE PIN. WHILE DOING THIS, TAKE ANOTHER COLLAR WITH YOUR RIGHT (LEFT) HAND AND PLACE IT

FINGER DEXTERITY PURDUE ASSEMBLY

ORIENTATION: Instructions for the subject (cont.)

OVER THE PIN. AS THIS IS BEING DONE, PICK UP ANOTHER WASHER WITH YOUR LEFT (RIGHT) HAND AND PLACE IT OVER THE PIN WITH THE COLLAR TO COMPLETE THE SECOND ASSEMBLY.

REMEMBER, DON'T STOP AFTER PUTTING ON THE FINAL WASHER, BUT IMMEDIATELY PICK UP ANOTHER PIN WITH YOUR RIGHT (LEFT) HAND AND PLACE IT IN THE NEXT EMPTY HOLE.

ALSO, ONLY PICK UP ONE PIECE AT A TIME. DON'T PICK UP SEVERAL WASHERS AND KEEP SLIPPING THEM ON. PICK UP ONE PIECE AT A TIME.

OK, NOW MAKE A FEW ASSEMBLIES FOR PRACTICE.

-------PRACTICE------- (3 assemblies)

(Watch the S to be sure that he is responding properly. Give any help needed on this practice.)

STOP! NOW PUT EVERYTHING BACK INTO THE CUPS.

WHEN I SAY "READY", PUT YOUR HANDS OVER THE CUPS YOU ARE FIRST GOING TO USE. WHEN I SAY "GO", MAKE AS MANY ASSEMBLIES AS YOU CAN BEGINNING WITH THE TOP RIGHT (LEFT) HOLE. KEEP WORKING AS RAPIDLY AS YOU CAN UNTIL I SAY "STOP".

IF YOU DROP ANY PIECE, FORGET ABOUT IT AND IMMEDIATELY START ON A NEW ASSEMBLY BY PICKING UP A PIN WITH YOUR RIGHT (LEFT) HAND AND PLACING IT IN THE NEXT EMPTY HOLE.

ARE THERE ANY QUESTIONS?

FINGER DEXTERITY — PURDUE ASSEMBLY

ORIENTATION: Instructions for the subject (cont.)

OK, LET'S START THE FIRST TRIAL.

READY?

GO!

-------TEST------- (one minute)

STOP!

(Record the score. Each piece is given one point. The score for 6 complete assemblies is 24. The score for 6 complete assemblies plus a pin and washer is 26.)

PLEASE PUT EVERYTHING BACK INTO THE CUPS.

LET'S START THE SECOND TRIAL.

READY?

GO!

-------TEST------- (one minute)

STOP!

PLEASE PUT EVERYTHING BACK INTO THE CUPS.

THAT IS THE END OF THIS TEST.

FINGER DEXTERITY — PURDUE ASSEMBLY

BASELINE: Instructions for the subject

DO YOU REMEMBER THIS TEST? DON'T FORGET TO PICK UP THE PINS AND COLLARS WITH YOUR RIGHT (LEFT) HAND AND ONLY USE YOUR LEFT (RIGHT) HAND TO PICK UP THE WASHERS.

WORK AS FAST AS YOU CAN AND IF YOU DROP ANYTHING, FORGET ABOUT IT AND IMMEDIATELY BEGIN ANOTHER ASSEMBLY BY PICKING UP A PIN WITH YOUR RIGHT (LEFT) HAND AND PUTTING IT IN THE NEXT EMPTY HOLE.

OK, LET'S START THE FIRST TRIAL.

READY?

GO!

-------TEST------- (one minute)

STOP! PLEASE PUT EVERYTHING BACK IN THE CUPS.

OK, LET'S START THE SECOND TRIAL.

READY?

GO!

-------TEST------- (one minute)

STOP! PLEASE PUT EVERYTHING BACK INTO THE CUPS.

THAT IS THE END OF THIS TEST.

FINGER DEXTERITY — PURDUE ASSEMBLY

EXPERIMENTAL: Instructions for the subject

DO YOU REMEMBER THIS TEST? DON'T FORGET TO PICK UP THE PINS AND COLLARS WITH YOUR RIGHT (LEFT) HAND AND ONLY USE YOUR LEFT (RIGHT) HAND TO PICK UP THE WASHERS.

WORK AS FAST AS YOU CAN AND IF YOU DROP ANYTHING, FORGET ABOUT IT AND IMMEDIATELY BEGIN ANOTHER ASSEMBLY BY PICKING UP A PIN WITH YOUR RIGHT (LEFT) HAND AND PUTTING IT IN THE NEXT EMPTY HOLE.

OK, LET'S START THE FIRST TRIAL.

(If the S does not get ready, repeat a maximum of three times: PUT YOUR HANDS OVER THE CUPS. If necessary, position the S over the board.)

GO!

(If the S does not start, repeat a maximum of three times: PICK UP THE PIN AND PUT IT IN THE HOLE. If necessary, place the S's hand in the pin tray. If he still does not respond, he will be considered untestable. Go to the next apparatus.)

-------TEST------- (one minute)

STOP! PLEASE PUT EVERYTHING BACK IN THE CUPS.

OK, LET'S START THE SECOND TRIAL.

READY? (Same instructions as above)

GO! (Same instructions as above)

-------TEST------- (one minute)

STOP! PLEASE PUT EVERYTHING BACK IN THE CUPS.

THAT IS THE END OF THIS TEST.

MULTI-LIMB COORDINATION TWO HAND COORDINATION

ORIENTATION: Instructions for the subject

THIS IS A TEST OF COORDINATION. THE BLACK DISC WILL ROTATE VERY SLOWLY IN A CLOCKWISE DIRECTION (indicate). THIS SMALL BRASS TARGET (indicate) WILL MOVE WITH THE DISC, AND IT WILL ALSO MOVE IN AN IRREGULAR MANNER IN THE CURVED SLOT (illustrate).

YOUR TASK WILL BE TO KEEP THE POINTER (indicate) ANYWHERE ON TOP OF THIS BRASS TARGET. YOUR SCORE IS THE TOTAL AMOUNT OF TIME YOU STAY ON THE TARGET, SO WHEN YOU GET OFF THE TARGET, GET BACK ON IT AS QUICKLY AS YOU CAN.

YOU ARE TO MOVE THE POINTER BY TURNING THESE TWO HANDLES. THE UPPER LEFT HANDLE MOVES THE POINTER SIDEWAYS AND THE HANDLE IN FRONT MOVES THE POINTER TOWARD AND AWAY FROM YOU. NOW PLACE YOUR HANDS ON THE HANDLES AND GET AN IDEA OF HOW THE POINTER MOVES.

WHEN I SAY "READY", PUT YOUR HANDS ON THE HANDLES AND MOVE THE POINTER SO THAT YOU START WITH IT DIRECTLY ON THE BRASS TARGET.

DO YOU HAVE ANY QUESTIONS?

WE'LL HAVE A PRACTICE TRIAL FIRST.

READY?

(Make sure the S starts each trial with the pointer on the target.)

GO!

(In order to start the trial press the button marked Unit 1 and simultaneously say "GO".)

MULTI-LIMB COORDINATION — TWO HAND COORDINATION

ORIENTATION: Instructions for the subject (cont.)

-------PRACTICE------- (one cycle)

(Watch the S to be sure that he is responding properly. Give any help needed on the practice trial.)

OK, LET'S START THE FIRST TRIAL.

READY?

GO!

-------TEST------- (one cycle)

STOP!

(After each cycle there is a 15-second rest to allow the S to get the pointer back on the target.)

OK, LET'S START THE SECOND (THIRD) (FOURTH) TRIAL.

(This test is given four times. After each trial record the cumulative time in the appropriate column on the data collection sheet. The score is the final time after the fourth trial divided by 4.)

READY?

GO!

-------TEST------- (one cycle)

STOP!

THAT IS THE END OF THIS TEST.

MULTI-LIMB COORDINATION TWO HAND COORDINATION

BASELINE: Instructions for the subject

DO YOU REMEMBER THIS TEST? DON'T FORGET, IF YOU GO OFF THE TARGET, GET BACK ON IT AS QUICKLY AS YOU CAN; DON'T WAIT FOR THE TARGET TO COME TO YOU.

OK, LET'S START THE FIRST TRIAL.

READY?

(Make sure the S starts each trial with the pointer on the target.)

GO!

-------TEST------- (one cycle)

(This test is given 4 times each session with a 15-second rest between trials.)

STOP!

THAT IS THE END OF THE TEST.

MULTI-LIMB COORDINATION — TWO HAND COORDINATION

EXPERIMENTAL: Instructions for the subject

DO YOU REMEMBER THIS TEST? DON'T FORGET, IF YOU GET OFF THE TARGET, GET BACK ON AS QUICKLY AS YOU CAN; DON'T WAIT FOR THE TARGET TO COME TO YOU.

OK, LET'S START THE FIRST TRIAL.

READY?

(If the S does not get ready, repeat a maximum of three times: PLACE YOUR HANDS ON THE HANDLES or MOVE THE POINTER ONTO THE TARGET. If necessary, place the subjects hands on the handles.)

GO!

(If the S does not respond, repeat a maximum of three times: GET ON THE TARGET. If the S does then respond, allow him to finish the cycle and then give him another complete trial. If the S does not respond, he will be considered untestable.)

-------TEST------- (one cycle)

(The test is given 4 times during each session with a 15-second rest between each trial.)

STOP!

THAT IS THE END OF THIS TEST.

DYNAMIC FLEXIBILITY DYNAMIC FLEXIBILITY TEST

ORIENTATION: Instructions for the subject

(To start the practice and/or the test, place the power switch in the "On" position. The white power light should now be on. From then on, the counters will be activated. Reset the counters after practice.)

WE'RE NOW GOING TO TEST HOW FLEXIBLE YOUR MUSCLES ARE. WE'D LIKE YOU TO STAND BEHIND THIS LINE (point), WITH YOUR FEET APART AND AGAINST THE SIDE RAILS.

WHEN I TELL YOU TO GET READY, STAND ERECT BEHIND THE LINE WITH YOUR HANDS LOCKED TOGETHER LIKE THIS (show subject grip with thumbs interlocked, fingers extended and joined). WHEN I SAY "GO", BEND AND TOUCH THE "X" IN FRONT OF YOU WITH BOTH HANDS, KEEPING YOUR KNEES STRAIGHT (demonstrate); THEN STRAIGHTEN UP, TWIST TO YOUR LEFT AND TOUCH THE "X" ON THE FLOOR AGAIN AND THEN STRAIGHTEN UP AND THIS TIME TWIST TO THE RIGHT AND TOUCH THE "X" BEHIND YOU (demonstrate 3 complete cycles).

YOUR SCORE WILL BE THE NUMBER OF TIMES YOU CAN DO THIS, SO MOVE AS FAST AS YOU CAN. REMEMBER TO KEEP YOUR LEGS STIFF. YOU WILL NOT RECEIVE CREDIT IF YOU DO NOT TOUCH THE "X's" OR IF YOU BEND YOUR KNEES TOO FAR. YOU'LL HAVE 20 SECONDS IN WHICH TO DO AS MANY REPETITIONS AS POSSIBLE.

DO YOU HAVE ANY QUESTIONS?

OK, LET'S TRY A FEW PRACTICE TRIALS.

READY?

GO!

-------PRACTICE------- (three repetitions)

DYNAMIC FLEXIBILITY DYNAMIC FLEXIBILITY TEST

ORIENTATION: Instructions for the subject (cont.)

(Watch the subject to be sure that he is responding properly. Give him any help needed on the practice trials. Reset the counters after the practice.)

WE'LL NOW HAVE A SHORT REST PERIOD.

-------REST------- (15 seconds)

OK, LET'S START THE FIRST TRIAL. REMEMBER TO GO AS FAST AS YOU CAN.

READY?

GO!

-------TEST------- (20 seconds)

STOP!

THAT IS THE END OF THIS TEST.

DYNAMIC FLEXIBILITY

DYNAMIC FLEXIBILITY TEST

BASELINE: Instructions for the subject

DO YOU REMEMBER THIS TEST? REMEMBER TO TOUCH THE "X" MARKS AND TO KEEP YOUR LEGS STRAIGHT. GO AS FAST AS YOU CAN.

READY?

GO!

-------TEST-------

(20 seconds)

STOP!

THAT IS THE END OF THIS TEST.

DYNAMIC FLEXIBILITY DYNAMIC FLEXIBILITY TEST

EXPERIMENTAL: Instructions for the subject

DO YOU REMEMBER THIS TEST? DON'T FORGET, TOUCH THE "X" MARKS AND KEEP YOUR LEGS STRAIGHT. GO AS FAST AS YOU CAN.

READY?

(If the S does not get ready, repeat a maximum of three times: GET READY TO TOUCH THE "X" ON THE FLOOR. If necessary, place the S is position. If he still does not get ready, he will be considered untestable.)

GO!

(If the S does not respond, repeat a maximum of three times: COME ON, BEND OVER AND TOUCH THE "X" ON THE FLOOR. If he does respond but does not straighten and twist, remind him three times to TOUCH THE "X" ON THE WALL. If he still does not respond, consider him untestable.)

-------TEST------- (20 seconds)

STOP!

THAT IS THE END OF THE TEST.

DYNAMIC STRENGTH — PULLUPS

ORIENTATION: Instructions for the subject

THIS IS A TEST TO MEASURE HOW MANY PULLUPS YOU CAN DO. YOU WILL STAND ON THIS PLATFORM (indicate) AND GRASP THE BAR IN THIS MANNER (demonstrate), UNDERHANDED, NOT OVER THE BAR.

WHEN I TELL YOU TO GET READY, LOWER YOURSELF UNTIL YOU ARE HANGING WITH YOUR ARMS STRAIGHT (demonstrate). WHEN I SAY "GO", PULL YOURSELF UP UNTIL YOUR CHIN IS OVER THE BAR AND LOWER YOURSELF TO THE STARTING POSITION (demonstrate).

DO AS MANY PULLUPS AS YOU CAN AND DO NOT STOP UNTIL YOU ARE NO LONGER ABLE TO PULL YOURSELF UP. DO NOT PAUSE MORE THAN TWO SECONDS EITHER AT THE TOP OR BOTTOM OF THE CYCLE OR YOU WILL BE TOLD TO STOP.

REMEMBER, YOU MUST GO ALL THE WAY UP UNTIL YOUR CHIN IS OVER THE BAR AND ALL THE WAY DOWN UNTIL YOUR ARMS ARE STRAIGHT, OR YOU WILL ONLY GET HALF CREDIT. DON'T KICK OR TWIST YOUR LEGS AND DON'T STOP UNTIL YOU ARE NOT ABLE TO DO ANY MORE AT ALL.

ANY QUESTIONS?

OK, LET'S BEGIN; I'LL COUNT EACH PULLUP OUT LOUD AS YOU DO IT.

READY?

GO!

(The interval between ready and go should be very short. As soon as the S lowers himself to the starting position tell him to begin.)

(Count each pullup out loud. If the subject begins to sway or kick, tell him to steady himself.)

DYNAMIC STRENGTH PULLUPS

ORIENTATION: Instructions for the subject (cont.)

-------TEST-------

(When the subject has stopped or has paused for rest longer than two seconds, the trial is over. Record the number of pullups he has correctly executed.)

THAT IS THE END OF THIS TEST.

DYNAMIC STRENGTH PULLUPS

BASELINE: Instructions for the subject

DON'T FORGET, DO AS MANY PULLUPS AS YOU CAN AND DO NOT PAUSE FOR MORE THAN TWO SECONDS AT ANY TIME OR YOU WILL BE TOLD TO STOP.

GO ALL THE WAY UP UNTIL YOUR CHIN IS OVER THE BAR AND ALL THE WAY DOWN UNTIL YOUR ARMS ARE STRAIGHT OR YOU WILL ONLY GET HALF CREDIT.

OK, LET'S BEGIN.

READY?

GO!

-------TEST-------

THAT IS THE END OF THIS TEST.

DYNAMIC STRENGTH

PULLUPS

EXPERIMENTAL: Instructions for the subject

DON'T FORGET, DO AS MANY PULLUPS AS YOU CAN AND DO NOT PAUSE FOR MORE THAN TWO SECONDS AT ANY TIME, OR YOU WILL BE TOLD TO STOP.

GO ALL THE WAY UP UNTIL YOUR CHIN IS OVER THE BAR, AND ALL THE WAY DOWN UNTIL YOUR ARMS ARE STRAIGHT OR YOU WILL ONLY GET HALF CREDIT.

OK, LET'S BEGIN.

READY?

(If the S does not get ready, repeat a maximum of three times: GRASP THE BAR AND GET READY TO START. If necessary, place the S's hands correctly on the chinning bar. If he still does not get ready, he will be considered untestable.)

GO!

(If the S does not respond when you say "GO", repeat a maximum of three times: PULL YOURSELF UP UNTIL YOUR CHIN IS OVER THE BAR. If he still does not respond, he will be considered untestable.)

-------TEST-------

THAT IS THE END OF THE TEST.

APPENDIX B

BASIC DATA TABLES FOR STUDIES I AND II

Table B - 1.1

Far Visual Acuity (Orthorater)
Data from Study I

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_9	12	12	12	12	12	12
		12	12	12	12	12	12
	S_{15}	11	12	12	12	12	12
		12	12	12	12	12	11
	S_{16}	11	11	11	10	11	11
		11	11	11	11	11	11
	S_{22}	10	10	10	11	10	11
		10	11	11	10	11	11
Experimental Group	S_{10}	11	4	4	3	4	10
		10	7	5	3	4	8
	S_{11}	12	10	9	10	11	12
		11	11	10	12	11	11
	S_{12}	6	6	5	5	5	5
		7	5	4	6	6	5
	S_{13}	10	8	7	8	8	9
		10	8	7	9	8	8
	S_{14}	10	9	5	5	8	9
		11	11	3	6	7	8
	S_{17}	7	5	5	6	6	7
		7	3	5	7	7	7
	S_{18}	11	8	7	12	11	12
		12	9	9	10	9	10
	S_{19}	12	11	9	11	11	10
		11	11	10	11	11	12
	S_{20}	12	10	6	10	10	9
		12	9	9	10	9	10
	S_{21}	8	5	5	5	8	8
		8	6	2	2	8	9
	S_{23}	10	9	10	10	11	10
		10	10	10	10	11	10

Table B - 1.2

Near Visual Acuity (Orthorater)
Data from Study I

		Test Session =	PD	D_1	D_2	D_3	D_4	D_5
		Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_9		12	12	12	12	12	12
			12	12	12	12	12	12
	S_{15}		12	12	12	12	12	12
			12	12	12	12	12	12
	S_{16}		11	10	10	11	11	11
			11	10	10	11	11	10
	S_{22}		10	9	9	10	10	9
			9	10	9	9	10	9
Experimental Group	S_{10}		11	3	1	2	1	2
			10	3	3	1	1	3
	S_{11}		11	10	10	9	12	12
			10	12	11	9	12	11
	S_{12}		7	7	3	2	4	4
			7	6	3	2	3	5
	S_{13}		11	8	6	7	6	5
			11	8	6	8	6	7
	S_{14}		12	8	1	1	3	3
			12	9	1	1	3	3
	S_{17}		12	12	10	10	12	10
			12	10	12	10	8	9
	S_{18}		12	11	9	11	9	12
			10	12	8	11	12	12
	S_{19}		12	10	2	4	6	10
			12	12	4	5	7	12
	S_{20}		12	8	3	3	4	9
			12	8	2	5	3	8
	S_{21}		11	8	2	4	7	9
			11	9	3	2	9	9
	S_{23}		11	9	3	2	7	7
			11	10	8	3	8	9

Table B - 1.3

Manual Dexterity Data (No. Blocks Moved)

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Durg Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_9	39	41	45	36	39	43
		42	48	44	42	41	47
	S_{15}	36	39	40	37	38	40
		38	40	40	38	42	45
	S_{16}	39	41	40	42	43	44
		40	42	40	43	41	45
	S_{22}	53	57	59	52	58	63
		52	60	63	55	58	63
Experimental Group	S_{10}	44	15	28	28	39	42
		53	28	32	38	49	51
	S_{11}	36	28	29	30	35	33
		38	35	27	32	35	39
	S_{12}	35	25	21	22	27	33
		35	25	25	25	31	38
	S_{13}	47	25	27	36	40	40
		49	31	32	36	44	45
	S_{14}	35	21	20	27	32	37
		44	25	19	30	38	37
	S_{17}	39	27	31	33	40	44
		42	28	36	37	43	44
	S_{18}	47	24	27	36	44	43
		48	33	32	42	46	50
	S_{19}	47	27	32	39	47	47
		52	33	38	40	42	48
	S_{20}	44	28	23	34	39	47
		50	31	30	38	41	46
	S_{21}	44	21	9	28	41	46
		49	20	11	27	47	56
	S_{23}	40	20	21	27	32	39
		43	30	28	34	37	31

Table B - 1.4

Static Strength Data (in Kilos)
Data from Study I

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_9	40	37	35	32	32	31
		34	32	32	32	33	33
	S_{15}	62	64	63	61	72	56
		61	64	63	63	74	57
	S_{16}	69	68	73	72	70	71
		64	71	76	74	68	72
	S_{22}	62	61	59	56	61	58
		59	60	55	57	57	54
Experimental Group	S_{10}	48	50	49	47	52	64
		49	50	51	44	48	52
	S_{11}	44	41	45	47	53	55
		44	39	43	42	50	54
	S_{12}	34	28	27	30	29	34
		34	25	25	25	30	36
	S_{13}	53	53	50	51	59	58
		56	49	53	55	53	52
	S_{14}	51	49	49	39	55	52
		56	45	54	49	57	60
	S_{17}	52	44	52	51	50	57
		50	43	52	50	53	54
	S_{18}	59	55	55	58	57	62
		53	54	55	60	60	60
	S_{19}	66	59	50	61	61	61
		63	60	63	63	63	60
	S_{20}	66	54	59	56	61	64
		62	56	58	61	55	63
	S_{21}	51	40	41	45	46	50
		51	37	40	42	45	45
	S_{23}	64	55	56	59	60	60
		67	55	53	58	58	59

Table B - 1.5

Gross Body Equilibrium Data (in Seconds)

Data from Study I

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_9	20	20	20	20	20	20
		20	20	20	20	20	20
	S_{15}	20	18	20	20	20	20
		16	20	20	20	20	20
	S_{16}	20	20	20	20	20	20
		20	20	20	20	20	20
	S_{22}	--	20	20	10	20	20
		--	11	19	18	20	20
Experimental Group	S_{10}	12	6	8	20	20	20
		13	0	7	9	17	20
	S_{11}	20	8	15	20	20	20
		20	6	18	20	20	20
	S_{12}	13	7	5	11	7	9
		20	7	7	10	12	20
	S_{13}	20	0	0	17	17	6
		16	10	0	10	20	20
	S_{14}	20	0	10	12	20	11
		20	0	7	20	20	20
	S_{17}	20	0	11	9	12	11
		20	6	16	19	20	20
	S_{18}	20	0	8	20	20	20
		20	8	7	9	20	20
	S_{19}	20	11	20	8	20	20
		20	16	12	20	17	20
	S_{20}	20	5	7	16	20	20
		20	10	5	20	17	20
	S_{21}	20	6	0	8	11	20
		20	0	10	8	11	20
	S_{23}	20	0	11	20	20	20
		20	6	6	17	20	20

Table B - 1.6

Number Facility Data (No. Items Correct)

Data from Study I

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_9	37	43	37	30	35	36
	S_{15}	29	33	26	34	30	29
	S_{16}	40	40	39	40	39	39
	S_{22}	41	45	42	44	44	44
Experimental Group	S_{10}	30	1	0	0	0	6
	S_{11}	38	25	17	15	26	37
	S_{12}	38	30	1	0	6	4
	S_{13}	69	38	6	18	32	46
	S_{14}	33	0	0	0	0	0
	S_{17}	35	25	20	24	25	32
	S_{18}	49	34	6	32	45	46
	S_{19}	47	29	0	0	0	23
	S_{20}	33	0	0	0	0	16
	S_{21}	39	11	1	7	27	38
	S_{23}	42	21	0	0	29	35

Table B - 1.7

Short Term Memory Data (Longest Span Completed)
Data from Study I

		Test Session =	PD	D_1	D_2	D_3	D_4	D_5
		Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_9		6	6	6	6	6	6
			5	5	4	5	6	7
	S_{15}		4	3	4	4	3	3
			3	4	2	6	2	2
	S_{16}		5	5	5	6	6	6
			4	4	4	5	5	5
	S_{22}		2	3	3	2	3	3
Experimental Group	S_{10}		4	3	2	3	4	2
			4	3	2	3	2	3
	S_{11}		5	3	3	3	3	3
			4	3	4	3	4	3
	S_{12}		4	4	4	4	4	4
			4	4	2	2	4	2
	S_{13}		6	2	3	5	5	5
			6	4	4	5	5	5
	S_{14}		4	4	1	2	3	3
			5	3	2	2	2	3
	S_{17}		5	3	3	4	4	2
			4	4	3	4	5	4
	S_{18}		4	4	2	2	4	5
			4	3	2	2	3	4
	S_{19}		5	4	2	4	4	4
			5	2	4	3	4	4
	S_{20}		5	3	2	1	5	2
			5	1	1	2	5	4
	S_{21}		5	2	1	2	4	5
			4	1	2	2	4	4
	S_{23}		7	5	4	5	6	5
			6	3	4	3	6	4

Table B - 1.8

Time Estimation Data (in Seconds)
Data from Study I

		Test Session =	PD	D_1	D_2	D_3	D_4	D_5
		Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_9		9.88	10.22	8.80	9.41	10.60	9.06
			11.52	11.03	10.89	10.19	10.25	9.98
			11.01	10.07	11.26	10.33	10.71	10.09
			10.98	11.13	11.25	10.28	10.29	9.61
			10.92	10.87	11.91	10.21	10.13	9.56
	S_{15}		10.48	12.48	11.52	10.11	12.72	9.79
			9.67	12.32	12.29	10.41	12.71	10.50
			11.00	13.38	12.13	10.78	14.25	12.31
			11.78	14.39	12.66	10.68	15.35	12.10
			13.01	15.29	12.22	10.87	14.05	12.44
	S_{16}		11.31	9.63	10.21	8.44	9.69	8.85
			9.40	10.45	10.17	10.09	8.92	10.01
			10.15	9.12	10.35	10.67	10.32	9.75
			10.62	9.57	10.28	10.12	9.63	10.35
			9.83	9.37	10.27	10.20	9.54	10.40
	S_{22}		9.78	8.66	8.94	8.10	10.27	8.88
			10.26	8.83	9.49	8.50	10.32	8.77
			10.36	8.78	8.68	8.59	10.98	8.19
			9.40	9.45	9.85	7.94	10.57	8.08
			10.00	8.72	9.48	9.42	10.13	8.04
Experimental Group	S_{10}		9.12	8.47	6.80	9.33	8.30	10.85
			9.98	9.08	9.33	9.52	9.25	10.12
			10.29	11.48	8.71*	9.85	9.38	8.65
			12.15	9.53	9.82	9.40	10.61	9.20
			9.71	7.71	8.88	14.38	11.19	9.63
	S_{11}		8.84	7.63	7.68	8.03	6.75	7.66
			9.77	7.21	8.89	8.74	7.78	6.88
			10.13	8.02	6.89	8.94	7.87	8.37
			9.44	6.68	9.05	6.71	7.27	9.21
			8.31	6.21	7.32	7.78	7.87	7.74
	S_{12}		8.29	9.04	8.54	6.00	8.82	8.81
			8.75	8.38	8.87	6.60	7.64	8.92
			8.86	9.29	9.10	8.07	8.51	8.49
			8.64	8.55	12.44	8.48	7.86	9.22
			8.68	6.94	8.66	9.65	7.63	8.86

*Original data replaced by cell mean, subj. fell asleep.

Table B - 1.8

(Cont'd.)

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Experimental Group	S_{13}	11.95	12.62	9.40	11.88	9.79	10.28
		14.59	9.60	11.53	11.39	11 23	10.96
		13.42	10.72	11.86	12.25	12.05	11.52
		14.62	9.83	16.12	13.17	12.05	11.19
		15.41	10.04	12.70	11.94	11.86	11.36
	S_{14}	9.41	6.71		6.17	9.51	8.75
		10.18	7.35*		8.59	8.70	8.39
		9.80	7.61		8.89	9.04	7.96
		9.71	7.51		8.95	8.66	8.29
		9.76	7.56		9.68	8.34	8.50
	S_{17}	9.27	8.59	8.42	9.53	8.93	9.25
		9.48	9.72	10.52	9.84	9.65	9.47
		9.27	10.43	7.86	10.68	10.15	9.03
		8.26	9.14	10.36	10.07	9.64	8.87
		9.40	10.68	9.36	9.09	10.50	11.48
	S_{18}	9.21	7.26	9.46	7.75	8.37	9.25
		10.01	8.46	7.52	8.73	9.26	10.49
		10.25	8.43	7.30	8.29	9.75	10.62
		10.60	9.69	8.80	9.49	9.75	11.21
	S_{19}	10.50	7.76	9.38	8.97	9.54	8.46
		10.85	8.70	8.62	8.99	9.82	9.44
		10.74	9.70	10.28	8.89	10.20	10.10
		11.25	9.91	8.80	9.36	9.79	9.32
		10.43	10.34	10.22	9.73	10.85	9.91
	S_{20}	9.29	9.22	9.17	8.50	7.73	8.55
		10.14	9.11	10.33	11.26	9.54	9.56
		11.87	8.16	9.33	11.71	8.91	9.03
		11.83	9.14	9.82	9.56	9.06	9.58
		11.11	8.56	10.01	9.05	12.80	10.23
	S_{21}	8.61	7.16*			9.77	7.80
		8.89	6.60			11.77	10.13
		9.66	7.15			11.68	10.68
		10.88	7.21			11.30	11.25
		10.32	7.69			11.72	11.21

*Original data replaced by cell mean, subj. fell asleep.

Table B - 1.8

Cont'd.)

Test Session =			D_1	D_2	D_3	D_4	D_5
Time Since Drug Admin. =			+0:45	+2:15	+4:15	+6:15	+8:45
Experimental Group	S_{23}		1 .23	9.62	9.46	8.16	10.06	9.45
			11.41	9.23	9.92	10.46	10.11	9.16
			11.29	9.39	10.26	9.71	10.31	9.26
			11.17	9.48	9.91	9.91	10.03	10.02
			10.70	9.23	10.31	9.49	10.49	9.84

Table B 1.9

Reaction Time Data (Loge of Seconds)
Data from Study I

		Test Session =	PD	D_1	D_2	D_3	D_4	D_5
		Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_9		-1.528	-1.336	-1.427	-1.374	-1.374	-1.470
			-1.313	-1.419	-1.394	-1.332	-1.374	-1.537
	S_{15}		-1.427	-1.505	-1.492	-1.073	-1.474	-1.470
			-1.298	-1.483	-1.456	-1.415	-1.532	-1.465
	S_{16}		-1.826	-1.551	-1.619	-1.677	-1.640	-1.650
			-1.585	-1.650	-1.546	-1.423	-1.585	-1.661
	S_{22}		-1.528	-1.565	-1.575	-1.661	-1.556	-1.661
			-1.435	-1.528	-1.561	-1.528	-1.519	-1.595
Experimental Group	S_{10}		-1.556	-1.191	-1.406	-1.423	-1.262	-1.630
			-1.595	-1.187	- .947	-1.255	-1.073	-1.106
	S_{11}		-1.419	-1.370	-1.328	-1.328	-1.317	-1.505
			-1.413	-1.168	-1.224	-1.332	-1.423	-1.561
	S_{12}		-1.570	-1.374	-1.382	-1.556	-1.505	-1.609
			-1.585	-1.423	-1.452	-1.298	-1.565	-1.580
	S_{13}		-1.324	-1.076	-1.328	-1.457	-1.487	-1.542
			-1.478	- .983	-1.321	-1.287	-1.496	-1.542
	S_{14}		-1.532	- .709	- .247	-1.487	-1.537	-1.590
			-1.551	- .968	+ .340	-1.435	-1.590	-1.619
	S_{17}		-1.528	-1.313	-1.411	-1.546	-1.630	-1.720
			-1.585	-1.390	-1.165	-1.570	-1.532	- .904
	S_{18}		-1.431	-1.313	- .968	-1.431	-1.546	-1.595
			-1.374	-1.351	-1.470	-1.470	-1.363	-1.595
	S_{19}		-1.394	-1.298	-1.287	-1.295	-1.378	-1.390
			-1.406	-1.187	-1.211	-1.287	-1.359	-1.351
	S_{20}		-1.542	- .233	- .957	-1.431	-1.595	-1.655
			-1.575	- .432	- .370	-1.431	-1.561	-1.625
	S_{21}		-1.532	- .812	+ .419	- .294	-1.492	-1.465
			-1.386	- .270	- .151	- .618	-1.431	-1.532
	S_{22}		-1.726	-1.224	-1.211	-1.386	-1.687	-1.604
			-1.709	-1.427	-1.136	-1.614	-1.661	-1.635

Table B - 2.1a

Arm-Hand Steadiness Data (Time in Error)

Data from Study II

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_{48}	.015	.011	.010	.012	.021	.009
		.011	.011	.017	.010	.006	.005
	S_{54}	.020	.015	.016	.015	.020	.012
		.008	.014	.026	.013	.016	.020
	S_{58}	.023	.020	.014	.007	.006	.007
		.018	.011	.013	.015	.007	.009
	S_{62}	.025	.023	.019	.019	.021	.015
		.023	.019	.015	.018	.019	.017
	S_{65}	.015	.008	.006	.013	.007	.014
		.015	.005	.006	.009	.006	.013
Experimental Group	S_{47}	.012	.029	.068	.026	.023	.008
		.002	.026	.039	.021	.007	.007
	S_{49}	.029	.048	.070	.071	.025	.026
		.024	.040	.110	.050	.041	.030
	S_{50}	.011	.063	.139	.027	.040	.016
		.015	.075	.117	.026	.023	.010
	S_{51}	.014	.047	.055	.033	.016	.013
		.012	.025	.039	.034	.013	.011
	S_{52}	.032	.048	.066	.023	.024	.012
		.023	.067	.029	.021	.020	.022
	S_{53}	.028	.082	.058	.069	.040	.010
		.030	.080	.053	.059	.025	.020
	S_{55}	.013	.076	.060	.021	.017	.012
		.011	.035	.065	.042	.013	.014
	S_{56}	.045	.150	.142	.070	.034	.024
		.015	.083	.080	.048	.030	.020
	S_{57}	.020	.056	.110	.052	.023	.017
		.032	.062	.113	.053	.019	.020
	S_{59}	.017	.054	.101	.052	.020	.015
		.008	.054	.068	.036	.025	.014

Table B - 2.1a
(Cont'd.)

Test Session =		PD	D_1	D_2	D_3	D_4	D_5
Time Since Drug Admin. =		-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Experimental Group	S_{60}	.023	.069	.133	.075	.096	.030
		.020	.066	.116	.033	.069	.019
	S_{61}	.010	.039	.113	.037	.021	.011
		.020	.066	.116	.033	.069	.019
	S_{63}	.023	.184	.321	.176	.042	.058
		.018	.123	.310	.127	.081	.059
	S_{64}	.010	.050	.068	.089	.021	.020
		.015	.032	.078	.059	.031	.006

Table B - 2.1b

Arm-Hand Steadiness Data (No. of Errors)

Data from Study II

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_{48}	9	7	7	9	13	5
		7	8	12	7	3	3
	S_{54}	14	9	15	6	14	7
		6	8	17	7	11	13
	S_{58}	14	11	9	5	4	4
		10	8	6	9	4	6
	S_{62}	11	14	11	10	11	7
		11	10	6	9	10	9
	S_{65}	8	7	10	11	6	7
		10	4	6	8	5	9
Experimental Group	S_{47}	11	15	35	15	13	5
		1	12	19	14	4	5
	S_{49}	21	20	29	33	15	18
		19	23	39	31	23	18
	S_{50}	10	33	50	16	20	9
		11	45	53	14	14	8
	S_{51}	8	16	19	21	13	6
		8	13	20	15	11	6
	S_{52}	20	26	30	17	20	10
		17	39	22	15	17	17
	S_{53}	14	42	23	29	27	8
		18	35	23	27	18	13
	S_{55}	11	38	33	16	14	9
		10	21	31	22	9	11
	S_{56}	18	42	36	25	16	13
		9	33	27	19	19	13
	S_{57}	20	28	44	25	18	14
		20	33	51	28	10	15
	S_{59}	13	27	30	33	14	12
		6	25	38	17	17	11

Table B - 2.1b
(Cont'd.)

		Test Session =	PD	D_1	D_2	D_3	D_4	D_5
		Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Experimental Group	S_{60}		19	40	51	33	38	27
			17	39	47	19	39	13
	S_{61}		7	23	51	21	12	10
			7	21	37	16	13	6
	S_{63}		15	59	73	61	19	29
			11	38	68	42	22	22
	S_{64}		6	19	32	32	9	13
			8	16	31	19	14	4

Table B - 2.2

Explosive Strength Data (Distance Jumped in Feet)

Data from Study II

		Test Session =	PD	D_1	D_2	D_3	D_4	D_5
		Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_{48}		4.75	5.00	4.75	4.75	4.50	4.50
			5.00	4.75	4.75	4.75	4.75	5.00
	S_{54}		5.50	5.75	5.75	6.00	6.00	6.00
			6.00	6.00	5.50	5.75	6.25	6.00
	S_{58}		6.00	6.00	6.50	7.00	6.50	7.00
			6.00	6.25	6.50	6.50	7.00	7.25
	S_{62}		4.75	4.00	4.00	3.75	4.25	4.00
			4.25	4.50	3.75	4.50	4.25	4.50
	S_{65}		6.00	6.00	6.25	6.25	6.00	6.25
			6.00	6.25	6.25	6.25	6.00	6.25
Experimental Group	S_{47}		6.00	5.50	5.75	5.75	6.00	6.25
			6.00	5.50	5.75	6.00	6.25	6.25
	S_{49}		4.50	3.50	4.00	4.50	4.50	4.75
			4.50	4.00	4.00	4.50	4.50	4.75
	S_{50}		7.00	5.50	5.50	7.25	7.00	7.50
			7.50	6.00	7.00	7.50	7.25	7.25
	S_{51}		5.00	3.25	3.25	4.25	4.50	4.75
			5.50	3.50	3.25	4.25	4.75	5.00
	S_{52}		6.25	6.50	5.50	5.25	5.25	6.00
			6.50	5.50	5.25	6.00	6.25	6.00
	S_{53}		6.50	5.50	5.75	5.75	6.25	6.75
			6.00	5.75	6.25	5.75	6.00	6.25
	S_{55}		6.75	4.00	5.25	6.00	6.00	6.50
			6.00	5.00	5.75	6.00	6.00	6.25
	S_{56}		4.75	3.00	3.25	4.50	5.50	5.50
			4.75	4.00	4.50	4.75	5.50	5.75
	S_{57}		5.00	4.25	4.25	5.00	5.00	4.75
			5.50	3.75	4.50	4.00	5.00	5.75
	S_{59}		6.00	5.00	5.25	5.75	6.25	6.00
			6.00	5.50	5.25	6.00	6.00	5.50

Table B - 2.2
(Cont'd.)

		Test Session =	PD	D_1	D_2	D_3	D_4	D_5
		Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Experimental Group	S_{60}		6.00	5.00	5.00	5.25	6.00	6.00
			6.00	5.25	5.50	5.50	5.50	5.75
	S_{61}		6.25	4.75	5.75	6.50	6.50	6.50
			6.50	5.00	5.75	6.50	6.50	6.50
	S_{63}		6.00	2.75	4.75	5.50	5.75	6.25
			6.00	2.25	4.75	5.50	6.00	6.50
	S_{64}		5.50	4.50	4.50	5.00	5.50	5.45
			5.75	4.75	4.50	5.00	5.50	5.50

Table B - 2.3

Manual Dexterity Data (No. Blocks Turned)

Data from Study II

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_{48}	24	25	26	26	24	26
		25	25	23	25	25	28
	S_{54}	25	29	28	28	30	30
		27	29	30	31	31	31
	S_{58}	25	24	26	26	28	29
		25	27	27	28	26	31
	S_{62}	27	28	29	27	26	28
		31	29	30	30	27	29
	S_{65}	32	34	35	34	33	33
		33	35	33	35	34	32
Experimental Group	S_{47}	31	23	17	27	32	34
		36	26	24	26	34	35
	S_{49}	25	16	15	20	23	26
		25	20	18	21	27	29
	S_{50}	23	18	20	19	22	25
		26	19	16	20	25	29
	S_{51}	31	21	20	25	26	28
		31	24	20	26	28	29
	S_{52}	31	22	24	29	32	35
		32	26	26	30	34	37
	S_{53}	24	20	17	21	24	28
		27	22	21	23	26	29
	S_{55}	31	19	20	23	28	34
		30	20	24	27	31	35
	S_{56}	27	20	--	25	29	32
		28	21	--	28	30	34
	S_{57}	33	17	--	25	27	30
		33	20	--	18	31	35
	S_{59}	36	27	21	26	32	35
		36	28	24	30	34	37

Table B - 2.3
(Cont'd.)

Test Session =		PD	D_1	D_2	D_3	D_4	D_5
Time Since Drug Admin. =		0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Experimental Group	S_{60}	30	21	17	22	25	28
		38	25	19	25	24	32
	S_{61}	33	19	18	29	32	31
		33	24	22	28	32	33
	S_{63}	31	14	--	18	25	27
		33	15	--	19	25	30
	S_{64}	31	21	--	20	23	27
		29	23	--	22	27	30

Table B - 2.4

Finger Dexterity Data (No. Items Assembled)

Data from Study II

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_{48}	48	47	49	49	49	51
		48	52	53	52	48	52
	S_{54}	38	40	40	41	41	41
		38	37	42	41	44	43
	S_{58}	38	40	40	41	41	46
		38	41	40	44	44	47
	S_{62}	46	39	40	38	38	40
		44	39	40	42	39	44
	S_{65}	36	44	45	41	44	40
		36	48	43	44	40	43
Experimental Group	S_{47}	45	27	20	36	44	49
		47	30	29	40	44	48
	S_{49}	44	32	17	25	34	45
		40	35	26	32	33	41
	S_{50}	41	16	16	24	23	37
		42	23	22	27	20	42
	S_{51}	44	36	25	28	38	42
		45	34	32	35	44	43
	S_{52}	44	35	33	40	41	44
		49	44	34	35	45	47
	S_{53}	34	19	18	19	23	29
		33	20	19	18	24	31
	S_{55}	41	20	22	30	30	40
		45	29	25	36	42	42
	S_{56}	37	17	--	15	29	46
		33	21	--	21	40	47
	S_{57}	49	24	16	21	38	47
		45	29	17	26	44	46
	S_{59}	48	35	23	37	41	44
		48	37	25	37	45	44

Table B - 2.4
(Cont'd.)

Test Session =		PD	D_1	D_2	D_3	D_4	D_5
Time Since Drug Admin. =		-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Experimental Group	S_{60}	46	35	21	26	26	43
		43	37	28	33	32	41
	S_{61}	45	25	21	34	37	38
		38	32	26	36	44	42
	S_{63}	38	18	10	12	35	32
		39	17	10	15	37	34
	S_{64}	41	28	17	21	28	39
		40	28	24	28	35	41

Table B - 2.5
Multilimb Coordination Data (Time on Target)
Data from Study II

	Test Session	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_{48}	.33	.64	.50	.71	.51	.71
		.46	.50	.67	.57	.54	.60
		.42	.55	.70	.61	.68	.52
	S_{54}	.70	.81	.85	.86	.85	.90
		.70	.82	.84	.86	.92	.89
		.72	.84	.89	.83	.95	.94
		.68	.89	.88	.79	.87	.98
	S_{58}	.51	.54	.74	.80	.93	.90
		.41	.47	.70	.82	.91	.95
		.57	.50	.61	.87	.92	.92
		.35	.47	.52	.95	.96	.94
	S_{62}	.45	.61	.62	.70	.74	.72
		.50	.58	.77	.62	.60	.66
		.68	.47	.61	.63	.74	.78
		.54	.57	.64	.73	.79	.78
	S_{65}	.88	.95	.97	.99	.99	.98
		.98	.88	.97	.99	.99	.92
		.91	.91	.99	.96	.96	.94
		.93	.95	.96	.95	.99	.94
Experimental Group	S_{47}	.85	.46	.51	.48	.59	.82
		.75	.62	.31	.50	.76	.77
		.80	.59	.37	.42	.72	.85
	S_{49}	.47	.46	.19	.29	.42	.40
		.54	.42	.22	.18	.27	.40
		.41	.32	.27	.22	.36	.46
	S_{50}	.72	.49	.19	.38	.41	.61
		.74	.49	.44	.48	.55	.72
		.66	.59	.47	.51	.36	.91
	S_{51}	.54	.34	.28	.40	.41	.70
		.59	.33	.29	.46	.70	.65
		.75	.55	.23	.31	.39	.59
		.73	.42	.35	.44	.60	.61
	S_{52}	.48	.31	.17	.42	.57	.62
		.53	.19	.14	.53	.55	.50
		.54	.16	.28	.51	.54	.70
		.39	.16	.13	.49	.63	.63
	S_{53}	.57	.53	.50	.41	.53	.80
		.77	.54	.22	.32	.55	.93
		.64	.34	.27	.42	.81	.96
		.80	.50	.44	.28	.56	.95

Table B - 2.5
(Cont'd.)

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Experimental Group	S_{55}	.64	.27	.23	.48	.70	.73
		.75	.52	.30	.56	.82	.83
		.76	.63	.35	.71	.83	.87
		.61	.38	.35	.68	.74	.87
	S_{56}	.61	.16	-	.10	.41	.49
		.69	.15	-	.18	.54	.64
		.62	.17	-	.08	.40	.70
		.65	.17	-	.21	.61	.83
	S_{57}	.36	.34	-	.08	.43	.68
		.46	.16	-	.17	.28	.58
		.45	.15	-	.18	.49	.62
		.61	.11	-	.06	.37	.57
	S_{59}	.62	.56	.22	.46	.64	.80
		.74	.52	.20	.58	.79	.82
		.88	.39	.22	.62	.87	.90
		.67	.50	.15	.71	.80	.87
	S_{60}	.63	.45	.24	.20	.43	.61
		.73	.44	.27	.39	.47	.71
		.72	.54	.24	.42	.40	.59
		.78	.48	.28	.42	.32	.66
	S_{61}	.91	.67	.51	.69	.91	.97
		.86	.64	.45	.85	.90	.90
		.91	.70	.67	.88	.92	.94
		.88	.63	.59	.78	.93	.92
	S_{63}	.48	.06	.02	-	.25	.55
		.69	.05	.01	-	.27	.57
		.64	.07	.03	-	.26	.52
		.60	.06	.05	-	.44	.55
	S_{64}	.60	.39	.12	.20	.37	.57
		.50	.40	.10	.16	.19	.59
		.66	.27	.15	.22	.30	.53
		.53	.45	.16	.11	.17	.63

Table B - 2.6

Dynamic Flexibility Data (No. of Cycles)

Data from Study II

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_{48}	22	22	20	20	21	21
	S_{54}	31	35	34	33	33	33
	S_{58}	24	24	23	27	26	27
	S_{62}	35	37	34	35	33	33
	S_{65}	34	34	34	34	33	36
Experimental Group	S_{47}	36	27	30	32	29	36
	S_{49}	24	16	21	22	21	26
	S_{50}	34	16	28	30	30	32
	S_{51}	32	19	21	30	30	32
	S_{52}	19	18	17	20	20	21
	S_{53}	28	21	21	22	25	28
	S_{55}	23	14	18	20	21	22
	S_{56}	20	12	15	14	21	23
	S_{57}	23	13	19	21	24	26
	S_{59}	29	25	28	30	31	32
	S_{60}	35	27	27	30	33	36
	S_{61}	36	22	29	32	36	36
	S_{63}	31	12	14	24	32	28
	S_{64}	29	18	18	22	22	28

Table B - 2.7

Dynamic Strength (No. Pull-Ups)

Data from Study II

	Test Session =	PD	D_1	D_2	D_3	D_4	D_5
	Time Since Drug Admin. =	-0:45	+0:45	+2:15	+4:15	+6:15	+8:45
Control Group	S_{48}	1.50	1.00	1.00	1.00	1.50	1.50
	S_{54}	2.00	1.50	1.00	1.00	1.50	2.00
	S_{58}	9.00	9.00	10.00	10.00	10.00	10.50
	S_{62}	0.50	0.50	0.50	0.50	0.50	0.50
	S_{65}	12.00	10.00	10.00	12.00	12.00	12.00
Experimental Group	S_{47}	12.00	9.00	9.00	12.00	9.00	10.00
	S_{49}	2.00	.50	1.50	1.00	1.75	1.50
	S_{50}	10.00	4.00	9.00	6.00	9.00	10.00
	S_{51}	3.50	--	0.50	3.00	3.00	3.50
	S_{52}	1.50	0.50	1.00	3.00	3.00	3.50
	S_{53}	2.00	1.00	1.50	2.50	3.00	3.00
	S_{55}	7.00	3.0	3.50	4.00	5.00	3.00
	S_{56}	9.00	4.00	9.00	8.00	10.00	8.50
	S_{57}	4.00	2.00	--	.75	--	3.00
	S_{59}	2.50	0.50	0.50	1.50	1.50	2.50
	S_{60}	4.00	0.50	2.00	2.00	3.00	2.50
	S_{61}	5.00	3.50	4.00	5.00	5.00	5.00
	S_{63}	1.50	--	--	--	--	.50
	S_{64}	1.50	1.00	1.00	--	0.50	1.50

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13. ABSTRACT

This report describes further development in a series of studies being conducted to assess the effects of drugs on human performance. It covers the development of detailed statistical methods for the analysis of data gathered in two studies covering tests of 17 different basic abilities. In both studies, experimental subjects were given a 12 µg/kg IM injection of scopolamine, and performance following injection was compared with that of control subjects.

Critical attention was given to the adequacy of the data obtained from each test and a series of recommendations for test improvements was given. An analysis of subject-by-treatment interactions in experimental group data led to recommendations to identify the factors which produce them in terms of stimulus or organismic parameters, and to develop means of controlling for them. Further recommendations were made regarding the study of intercorrelations among subjects and among tests to attempt to identify types of subjects and possible redundancy among tests in the batteries being used.

KEY WORDS: DRUGS, SCOPOLAMINE, VOLUNTEERS, SKILL, HUMAN PERFORMANCE, ABILITIES, STATISTICS, EXPERIMENTAL DESIGN, METHODOLOGY, RELIABILITY

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