1939

Studies in Gross Bodily Movement.

Alan Douglas Grinsted

Louisiana State University and Agricultural & Mechanical College

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STUDIES IN CROSS BODILY MOVEMENT

A Thesis

Submitted to the Graduate Faculty of the
Louisiana State University and
Agricultural and Mechanical College
in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy

in

The Department of Psychology

By

Alan Douglas Grinsted
B. A., Bucknell University, 1931
M. A., University of Florida, 1933
1939
The complete list of all those to whom belongs credit for assistance in the carrying out of this study is too long to mention here in its entirety. However, it gives me great pleasure to name here some to whom I am particularly indebted.

First, I should like to mention my appreciation of the cooperation shown me by the students in the psychology classes of Louisiana State University who acted as subjects in Experiments I and III and by Mr. John R. Sheptuga, Principal, and his teachers in the University Elementary and High Schools, without whose assistance the data could never have been gathered for Experiment II.

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I am greatly indebted also to Miss Beverly W. Phillips and to my wife, Mary Elizabeth Chitty Grinstead, both of whom worked carefully and untiringly at the tasks of reading records and assisting with the working out of statistical results. To my wife I am also grateful for inspiration and encouragement throughout the study.
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ABSTRACT

A review of the literature on the question of gross bodily movement, especially that of Smyansky, Johnson, Irwin, and Garvey, suggests that such movement is primarily a function of internal bodily states. The work of Clark on bodily movement during problem solving suggests that each individual, when attacking a mental problem, assumes a characteristic pose for the better concentration of attention. Such evidence seems to be opposed to the idea that a person assumes "muscle sets" appropriate to his mental processes. This paper reports studies which attack the problem of whether gross bodily movement is a function of the "person" or of an individual's mental activity at a given moment.

In the first experiment is described a new type of stabilimeter chair which was designed by the author and in which measurements of the movements of the occupant can be recorded without his being aware that such measurements are being made. With the aid of this apparatus, polygraph records were obtained of the movements of 53 subjects while they sat solving mental problems. Results disclosed that the amount of movement was significantly greater at the end of a given mental effort than at the start; that is, movement was found to be greatest when the subject decided that a given attack upon the problem was futile or when he definitely solved the problem than it was at the time of presentation of the problem or at times when a given attack upon the problem was in progress. These results bear out the idea that the assuming of muscle sets appropriate to the attack to be made on the problem do not account
for the gross bodily movements accompanying thought processes and raise
the further question as to what factors are related to such activity in
making individuals above the infant level. (Most of the work reported
in the literature has been on infants or on motility during sleep.)

Experiment II, reported in this paper, attempts to discover some of these
factors. An observational study was made upon children in their class-
rooms, the time-sampling technique being employed. The number of move-
ments per observation for each child was calculated, and where feasible
the results were treated statistically. The average amount of movement
between two different days for the same forty subjects showed a product-
moment correlation of .40 ± .09, revealing a consistency between indivi-
duals from day to day. Data on 123 children showed correlations of
-.78 ± .025 between average amount of movement and age with I.Q. partialled
out, and -.81 ± .081 between average amount of movement and I.Q. with age
partialled out. No sex differences were found, and habitual amount of
sleep proved to have no relation when age was held constant. Diurnal
curves obtained after three days continuous observation of ten children
in the first grade and ten in the seventh showed a high degree of simi-
larity between the two grades in the shape of the curves, although the
average amount of movement in the first grade was much the greater from
point to point in the curves. A peak in amount of movement was revealed
in each case between 9:00 and 9:30 A.M. with a falling off until just
before noon when a lesser rise occurred. Another smaller peak was found
in each case between one-thirty and two o'clock in the afternoon.

A third experiment was attempted to find the relation between
movement as revealed in the stabilimeter chair and scores made on the
Bemreuter Personality Inventory. The results suggest that movement increases with lack of self-confidence and with degree of sociability as measured by the Flanagan scales for the Inventory. However, the limited number of subjects used (14) and the conditions of the experiment were such as to permit the results to be considered only as tentative.

The author of this paper feels that his results disprove the idea of a "muscle set" in problem solving and indicate that gross bodily movement is related to the individual's personality rather than to his mental activity at any given moment.
INTRODUCTION

The term motility has had a variety of uses in the field of psychology. In the present paper it will be understood to refer only to gross bodily movement such as is involved in change of bodily position. Eye-movements, tics, and similar "minor" movements, then, are not included. Similarly, we are interested primarily in the sort of motility that is often thought of as involuntary and "automatic," rather than in the more definitely "willed" behavior such as has concerned Downey (42), Allport and Vernon (3), and others (46, 76). It is not uncommon to hear people talk about the sort of gross bodily movement with which we are here concerned. Expressions such as "ill at ease," "can't sit still any longer," "on needles and pins," and "fidgety" all attest to popular interest in motility on the part of the average man. However, most of the work that has been done by way of scientific investigation of the phenomenon has dealt with either infants or sleeping persons.

The first studies of spontaneous activity seem to have been made with small animals by means of the revolving drum cage (38, p. 101). This is a "squirrel cage" type of inclosure that revolves when the animal "runs" it, the number of revolutions being automatically recorded by a counter. The actual measurement of motility dates back at least as far as 1914, when Szymansky (205, 206, 207, 208, 209, 210) developed nine different forms of Aktographen apparatus of the kymograph recording type, and used them with both animals and humans. In
general, his method consisted of putting his subject in the cage, chair, or bed, which is so mounted as to yield to his movements. Such movements displace a stylus which rides upon a strip of moving paper and which, as long as it is not disturbed, will record a straight line upon the paper. Another stylus, actuated at convenient intervals by a clock, provides a time-record. With such apparatus he secured many long-time records of the activity of many kinds of creatures, ranging from snails to children. His method of registering bodily movement was later applied, in principle, by several other inventors, many of whom were not acquainted with Szymansky's work at the time they designed their own instruments (108, p. 252).

Speaking of such methods of recording movements kymographically, Johnson and Swan say,

"The results, of course, are strictly relative to the method, and are meaningless unless the principles and the limitations of the latter are considered along with them .... In general, the displacement of the recording pen is not proportional to the work performed in effecting the change of bodily position. This assertion is based on the fact that all the mountings were subject to periods of their own, .... and on the further fact that at least one component of the reaction of the mounting is left unrecorded. Such being the case, an attempt to convert the magnitude of the recording pen into an expression of the energy expended by the subject in performing the change of bodily posture, is absurd." (111, pp. 18-19.)

Szymansky's results were inadequately stated, in many cases, and his records on humans were obtained for only single days (110, p. 38). Each of his subjects remained in bed for twenty-four hours, with instructions to remain as quiet and passive as possible. "Indifferent" but not "exciting" material was permitted to be read, and eight of his
subjects obeyed the instructions. On the basis of simple inspection of amplitude and frequency of movement of the record line he assigned each hour period to a category of relative activity.

The work of Johnson and Swan which was done later (110, p. 121) showed that large variations in the manner of resting of an individual subject from day to day are the rule, not the exception; and consequently, a single day's record affords no reliable representation of the subject's habitual way of resting. However, Szymansky's results are interesting in view of their being the first of the kind to be obtained. The main periods of motility came in the morning and late in the afternoon. Szymansky noticed the resemblance between these findings and those of W. Stern, reported in Ueber Psychologie der individuellen Differenzen in 1900. At different times of the day, Stern had made his subjects tap out a rhythm in waltz time, and had plotted the tempo against the time of day. The curve resembled a wide Latin "M" in that it declined until one or two hours after the midday meal, rose to a second maximum late in the afternoon, and again declined toward evening.

Whereas Szymansky had been interested in activity over a full twenty-four hour day, Gerber (62) seems to have been the first to record activity during sleep alone. Kreidl and Herz (125) made non-statistical studies on deaf and blind subjects finding them to differ but little from normals, and Karger (116) the sleep of hospitalized children as affected by drugs, fever, etc.
In connection with the Simmons Investigation of Sleep at the Melloa Institute, H. M. Johnson and his assistants (103, 104, 105, 106, 107, 108, 109, 111, 112) used activity recording beds to study the nocturnal activity of 150 persons of both sexes, of different occupations, and in various states of health. Each of these persons was studied for periods ranging between a few weeks and two years, with more than two million measurements being made and treated statistically (103, p. 6). Johnson's method of reading the kymograph records was based upon the classification of each five-minute interval into "active" or "passive" according to whether the sleeper changed his gross bodily position within it or not. His chief findings are reported as follows:

"On the whole, young children change from one position to another more often than adolescents. In fact, the quietest sleepers who are represented in this exhibit are the men of college age. However, more recent experiments, made by a different method (photographic) indicate that college women rest more quietly than men of the same age, although their sleep is still far from being 'log-like.' Middle-aged sleepers are satisfied with one position for much shorter times than college youths and maidens — the men as a class being considerably more restless than their wives. This difference may be laid in part to occupation rather than sex, for a good part of the group of men was made up of intellectual workers, who composed, or wrote, or planned, or drafted or calculated, at night, while their wives lounged, read, or reviewed the social activities of the afternoon. Such men, without exception, retire late, go to sleep promptly, stir frequently, and awake early. Only two subjects were older than 64, and only one past 70. The latter does not appear in the table, being a recent addition, and studied for only a few weeks. Both these elderly men are quiet sleepers for their class. The former is a skilled mechanic, the latter a wealthy business man who is afraid he may be insomnic." (103, p. 9.)

Johnson's work also indicated that motility during sleep is correlated with health rather than with illness, as has often been popularly supposed (103, p. 9). His curves on sleepers point to the further
fact that the disposition toward immobility, as Szymansky has called it, follows a characteristic rhythm (108, pp. 264-270; 110, p. 15).

This rhythm in motility has been found by Garvey to be much more pronounced in the case of children than was found by Johnson in the case of adults. He also found a high degree of similarity between analogous curves for two children taken at random. So great was this similarity, in fact, that he was able to combine the records of all 13 children in a single curve and show that these rhythmic tendencies (dated from the interval in which the child goes to sleep) still persist quite clearly, although, as pointed out by Johnson (108, p. 267 f.), each point plotted to the curve represents 1300 observations.

Garvey's study also revealed that quietness was not related to length of time habitually spent in sleeping, that temperature had little effect except that activity increased with extremes of temperature, and that (as Johnson's studies had shown) restlessness increased toward morning. There was some indication that quietness of sleep and lower humidities were associated, but the evidence was not clear. Sex differences were also small and uncertain. Three-year-old children were more active than either two-year-olds or four-year-olds, with the four-year-olds the least active in the group. The data also suggest that constipation may produce longer sleep and looseness of the bowels more restless sleep (55, pp. 55-63, 79-80).

Most of the investigations of motility not so far mentioned have dealt with the movements of either animals or children, and have largely centered around the relation of motility to hunger (55, pp. 55-63, 79-80).
Especially notable in this connection is the work of Wada (219), who reported that stomach contractions and bodily movements coincided to a remarkable degree. She also verified the earlier work of Carlson and Ginsburg (86) and of Ginsburg, Tumpowsky, and Carlson (68) to the effect that contractions of the stomach are coincident with the sensation of hunger.

Irwin (88, 89, 90, 91, 92, 93, 94, 95, 96, 97, 98, 99, 100, 101) has made the most complete studies of the activities of the newborn baby. Using a stabilimeter in connection with a crib placed inside an experimental cabinet, he found that bodily activity increased relatively in proportion to the time since feeding (90). Unlike Garvey and Johnson, Irwin read his records in terms of the percentage of frequency of movements per unit time of the mechanical pens. However, the problem of amplitude and intensity was disregarded, and the frequency of oscillations was taken as a measure of the automatic activity (88). He states his conclusions as follows:

"The fact that activity usually is greatest toward the end of the observation period, that is, just before nursing, and usually is least at the beginning of the period, just after nursing, suggests that the activity is stimulated probably by hunger contractions. This inference is best illustrated by the exceptional rise of the curve during the long night period when the nursings were eight hours apart. Since the external stimuli are constant, the conclusion seems justified that the activity of these infants is due largely to internal factors, presumably of the alimentary canal."

Irwin has also found in his work with Weiss (101) that activity is greater under conditions of either darkness or bright light than under dim light. These workers think the sensitivity of the dark adapted eye may be related to this phenomenon.
In addition to his work described above, Irwin has been concerned as well with the question of the development of activity in the newborn (88). Measurements made of the percentage of frequency of observed movements of the various body parts and according to the frequency of observed movements per hour showed that the activity in the anterior segments of the body are greater than that in the posterior segments on every day except the first, and that throughout the entire ten days gains in dominance over the extremities. He further distinguishes between two general types of bodily movement: specific movement, i.e. involving a single segment and at a rate slow enough to be discriminated by a trained observer, and mass movement, which includes movements too rapid to be described by experimenters, and involving the whole body. He says,

"The most striking characteristic exhibited by infants during the first ten days is the large amount of energy expended while mass activity is in progress. It appears during hunger, previous to belching, during regurgitation, intestinal disturbances, defecation, and occasionally during micturition. The infant maintains continuous body movement with such speed and excessiveness that the experimenter, even when using a specially devised code, cannot keep up with the infant .... Mass activity is present usually just before nursing, and almost invariably toward the middle or end of the long night period from which two nursings have been omitted. It continues in an aperiodic manner and may last from a few minutes to five hours." (88, p. 102.)

Irwin reasons that the stimulation is internal, since activity is greatest during hunger, regurgitation, etc. — processes of the smooth muscle and involving one part or another of the alimentary canal — from which it follows that "the activity is due to stimuli arising internally while these processes go on." (88, p. 104.) Shirley (190, p. 238) calls
attention to the fact that these findings were anticipated by Preyer (161) in his characterization of the impulsive movements of infants as those "caused without previous peripheral excitement exclusively by the nutritive and other organic processes."

All of these findings indicate that bodily activity in the infant or in the sleeping person is directed toward the alleviation of bodily needs. There is nothing in the literature so far as the present writer knows that suggests that the situation is any different in the case of the waking, more mature person who is not immediately occupied with the job of responding to some particular stimulus from the outside. That is to say, when one is adjusting to some outside stimulus situation, such as when he hoed a garden or catches a ball that is thrown to him, he is obviously moving in accordance with needs other than or in addition to his immediate bodily needs; but when he sits daydreaming, or wondering how he will pay next month's bills, there is no evidence that his movements are in any wise motivated differently than when he is asleep. Nevertheless, one continually encounters the notion that a person "gets set" to think about a problem in the same sort of way that he "gets set" to throw a ball, or that a bird-dog points when he finds a covey of quail. Confusion seems to have arisen out of the use of the word set. Many textbooks draw analogies between the situation when a runner "gets set" at the start of a race and the sort of "mental set" that accompanies efficient performance in a reaction-time or a word-association experiment (38, p. 314f). The important question is raised here as to whether the phenomenon which we call mental set is truly a muscle
It is true that various experimenters have found what they called bodily tension accompanying thought (69, 102), and on the basis of such work Washburn formulated a motor psychology of thinking (224). Nevertheless, H. M. Johnson decided that "as late as June, 1931, no practicable method of measuring general muscular relaxation and tension in any intact organism had been brought into existence, although a variety of other forms of measurement — such as those of the resistance of the body to an external electromotive force — had been unwarrantably called muscular tension," (108, p. 247), and the present writer knows of no such method's having been discovered since the date Johnson mentions.

Dashiell says, "When a person assumes a set that will facilitate his response to some particular stimulus or stimuli, that set goes by the name of attending or attention" (38, p. 322). But when a person is "attending" to the solving of a mental problem, are we justified in saying that he has a set in the sense of the above definition?

Very little seems to have been done by way of investigation of this question. In 1908, Starring (199) reported in his study of thought processes that eye-movements sometimes occur in connection with visual imagery and in the perceiving of position of the various terms. He also reported that arm movements seem to represent relationships of right and left. Other studies in the early part of the century, such as those of Bousher (17), Courten (35), Curtis (36), Gamble (53), and Pintner (153), and others somewhat more recently (24, 154, 183) have been concerned with the relation of mental activity to circulation of the blood, to tongue movements, to movements of the larynx, to breathing,
The nature and extent of movement of subjects who were trying to answer such questions as: "How would you plan a trip to Iceland?" and "How would you entertain a party of blind children?" Miss Clark reports, on the basis of her observations, that "... the gross bodily behavior of each individual represented a characteristic mode of adaptation which he adopted toward all his problems .... This gross bodily behavior was so invariably adopted by each individual that it seemed to have no bearing on the solving of the various problems but was simply a bodily adjustment for the better concentration of attention" (Clark, p. 29). Obviously this is counter to the idea that the subject is assuming a postural set.

Clark thinks that minor movements are more closely related to problem solving, and she finds some relation for some individuals between imagery and eye-movements, articulatory movements, and incipient hand movements. In addition to her written observations, this investigator used a concealed telegraph key to record on a kymograph when the problem was presented; then she pressed the key every time she saw the subject move, and the subject pressed another key to show when the problem was solved. Thus she attempted to obtain a record of the amount of movement of any kind. Whereas she says on one occasion that "movement increased as thinking progressed" (Clark, p. 29), she later says,
"the number of movements tended to decrease as the thinking was pro-
longed" (32, p. 46). The seeming contradiction of these two statements
is not clarified. Clark also reports that "perceptible movements
accompany thinking during not more than one-third of its total time.
They come and go at irregular intervals with the exception of pauses
immediately after the presentation of the problem and just prior to
the subject's final signal" (32, p. 55). She states elsewhere (32, p.
29) that the pauses after the problem averaged 2.7 sec. and those just
before the solution, 3.2 sec. The pauses occurring immediately after
presentation of the problem again suggest that the person is not "getting
set" to think out a solution. Clark concludes (32, p. 86) that "the
arousal of movement by imagery requires a strength of stimulus adequate
to pass from the sensory areas out through the efferent neurones," and
she suggests that "thinking may be other than a mere series of sensori-
motor arcs."

Obviously this study by Ruth Clark is more in line with what one
might expect from the result of such studies as have been mentioned in
the earlier part of this paper. While she made no distinction between
gross bodily movement and minor movements, such as those of the eyes,
her results suggest that a person's motility while problem solving is
much more an expression of the person himself than of the problem which
he is attempting to solve.

This raises the further question, then, as to how much an individu-
ual's motility in a given situation is affected by the kind of person
he is, in addition to the question as to the relation of his motility to mental activity. These are the questions upon which it was hoped that the present study might throw some light.
EXPERIMENT I

The Problem

In accordance with the preceding discussion it is the purpose of this experiment to study the relation of motility to problem solving. Presumably, if a person assumes postural sets appropriate to his thought processes, we may expect his "getting set" movements to occur in an objective record of his motility. When one attack upon a problem does not bring about a solution and a new method of attack becomes necessary, we should expect that the subject will show a shift of position appropriate to his new postural set.

The problem of this experiment, then, may be stated as follows: Do a person's movements during a problem-solving activity reveal any evidence of postural sets appropriate to the activity of searching for a solution?

Apparatus

In this experiment there was used a stabilimeter chair of a type somewhat different from those mentioned in the literature by Szymansky (116, p. 19) and Renshaw (166). It is more complicated than those generally used, but has the advantage of being so constructed that the person sitting in it is unaware of the fact that measurements of his movements are being made. It is, to all appearances, simply a comfortable morris chair with a foot rest. However, the arms, back, seat,
and foot rest are all mounted on coil springs, and from each of them there leads off a string to a polygraph that is mounted behind the chair. Each string is attached to a steel rod that carries the point of an Inkograph pen on the polygraph. On the opposite end of the rod from where the string is attached is fastened a rubber band, which exerts its pull in opposition to any pull on the string. Consequently, therefore, any release upon the tension on the string, such as occurs when a part of the chair gives under pressure exerted upon it by the subject, permits the rubber band to contract, pulling the steel rod through the groove in which it is set and moving the pen point. When the pressure applied by the subject to the part of the chair in question is released, the springs under that part return it to its normal position, this in turn pulling on the string and causing the rod to carry the pen point back once more to its former position or as near to it as the subject's partial release of pressure may permit. The pen itself is supported in an upright position by a rack with holes in which the pens ride. The pens themselves are so placed that their points are in line with one another and record on a sheet of white paper which is driven by an electric clock motor at the constant speed of 4-5/8 in. per minute. The apparatus has been sufficiently enclosed to render it almost noiseless even to one who listens for its sound, and no subject has given any sign of being aware of its existence.

In the latter part of the experiment a pencil maze was used in conjunction with an original device to make the situation such that the subject is unable to see where he is going. This apparatus consists
merely of a light piece of veneer wood 8-3/4 inches square and 1/16 inch thick, in the center of which is a circular hole 3/4 inch in diameter. By means of a beveled block of wood 1/8 inch thick adjoining the hole, it is possible to staple a pencil in such a position that its point protrudes through the hole until it is in line with the center of the opening. By this means a person can run a pencil maze by watching his progress through the hole and still not be able to see ahead far enough to avoid blind alleys or to see openings before he gets to them. It is felt that a subject working a maze with this device is more nearly in the situation that an animal finds himself in when placed in a "wall" maze than is generally true in the case of pencil mazes.

Subjects

All of the subjects used in this experiment were members of the psychology classes in Louisiana State University. There were fifty-three in all, two of whom were graduate students, the rest all being undergraduates. In the procedure to be described, all of the subjects were used, except as otherwise stated. So far as the experimenter could know, the subjects were all in good health, and each appeared for the experiment alone in accordance with a previously arranged appointment. If any selection occurred, it was merely as a result of the fact that some did not keep their appointments or avoided making one. The few thus left out probably would not have influenced the results greatly had they been included. Of those asked to perform as subjects, about 85 per cent made appointments and appeared. There is little reason to
believe that any were under any unusual emotional strain, but it was the first time that most of them had ever acted as subjects in a psychological experiment.

Procedure

Each $S$ was experimented on alone. When he appeared he was seated in the stabilometer chair facing $E$, who told him that the purpose of the experiment was to see how he would go about solving some problems, and that consequently he was to "think aloud" in so far as possible.

The problems were given by $E$, one at a time. Those used were of the kind that involve figuring out how to measure out two pints of water, using only a 7-pint pail and a 5-pint pail. All but one of the problems were taken from the average adult level of the latest revision of the Stanford-Binet Scale (211). $E$ read each problem as instructed by the authors of the scale, with the exception that he always omitted telling $S$ with which pail to begin until and in the event that it became evident that $S$ could not solve the problem without that help. If none of the standard problems proved sufficiently difficult to baffle the subject for at least a full minute, an original and more difficult problem was added.**

$E$ also, by use of a concealed telegraph key, made code signal marks that were electrically recorded on the polygraph paper to show the times

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*The usual order of giving the problems was to give all of those in Form $N$, average adult 2, p. 177; and then one or two of those in Form $L$, average adult 6, the choice depending upon the experimenter's judgment as to which would best call forth the subject's thought processes and yet not be too discouraging.

**To get 4 pints of water, measuring with only a 5-pint pail and a 9-pint pail.
at which (1) the problem was given, (2) the subject showed verbally that he knew the answer, and (3) the subject gave verbal evidence that he was abandoning one mode of attack to undertake another, the criteria for this last being such remarks on the part of $S$ as: "No, that won't work," or "I don't think I can get it." The signal for (1) was given at a point about midway in the reading of the problem on the assumption that $S$ would be starting to search for the solution as soon as he had heard all the necessary details the first time. The signals for (2) and (3) were given immediately after $E$ recognized the criteria calling for them, i.e., evidence of a shift in attack for (2) and evidence of a correct solution for (3). The signal for (3) was also given when $S$ gave definite signs of giving up and $E$ felt that further encouragement was useless. Encouragement was given $S$ to go on, however, when $S$ tended to give up too easily, and in such cases the signal for (2) was given. Aside from brief words of encouragement from $E$ in such cases, however, he avoided any conversation with $S$ while $S$ was trying to solve the problem. The problems were given in succession until at least four had been attempted and until at least one had consumed a full minute. In three cases $S$ solved all of the problems given, including the added, more difficult one in less than a minute each. In the case of 45 subjects, the last problem was announced as the last at least twice, with such remarks as: "We'll try one more," followed by, "This is the last problem."

In the case of 38 subjects a maze-solving technique was tried after the problem solving part of the experiment was concluded; however, the
subjects were given no advance notice that anything else was to be required of them when the problem solving was ended. Each S was asked whether he knew what a maze was, after which a simple pencil maze was shown him with an explanation of its use. Then there was placed across the arms of the stabilimeter chair a wide board on which had been fastened with thumb tacks a Porteous pencil maze for the twelve-year level. The maze running device described under "Apparatus" was already in place with the pencil on the starting point, and S was asked to trace his way out without crossing any lines and without removing his pencil from the paper. E stood where he could see the progress of S in his effort to work his way through the maze and where he could also reach the concealed telegraph key to record by the code signals as used in the problem solving part of the experiment the moment when S started to search for his way out of the maze and the moment when he finally reached the outside. When the experiment was finally concluded, E asked each S not to discuss the experiment with anyone who had not already acted as a subject.

In the problem solving part of the experiment fifty-three records were obtained, covering 201 readable problem records in all. (Some individual problems had to be left out of consideration because of one or another difficulty, such as failure of one of the pens to record on the entire record, failure of the experimenter to use the correct code signals, etc.) The entire record of each problem solving attempt was then divided into five-second intervals, starting at a point ten seconds before the signal marking the point where the problem was given,
and continuing ten seconds past the signal showing that the solution had been reached. Each segment was then examined for any movement whatever, and the "active" intervals (those in which any movement occurred) were marked in similar fashion to the method used in motility studies by Johnson (108, p. 255f) and by Garvey (55, p. 9). Analogous intervals in the several problems were then compared and percentages of active intervals among all those of a given type were worked out. Thus the percentages of active intervals out of the total of 201 problems considered were worked out for the two intervals just before and the two just following the presentation of the problem, the reaching of the solution, and the points where signals indicated that shifts occurred. Also, to obtain records not so closely related to these points, measurements were made of the four intervals following the two immediately after the presentation of the problem, and the last four immediately preceding the two just before the solution and not otherwise included.

By the use of this method it was possible to construct a somewhat fictitious curve of a probably typical problem solving attempt, showing the average motility for all the subjects for the ten seconds just before and the ten just after the presentation of the problem, for the following twenty seconds (where no shift or solution signals occurred), for the ten seconds just before and the ten just after the first two shifts, for the last twenty seconds not included in either shift or solution, and for the ten seconds just before and the ten just after the signal showing that the problem had been solved. Inasmuch as the time taken to solve the various problems varied from problem to problem
and from person to person, there was no better way of bringing the data together so as to show the effect of the different stages in the problem solving attempt.

A curve was also constructed to show the relative amount of activity occurring in each quarter of the several problem solving attempts, not including the ten seconds just before or the ten just after the presentation of the problem or the point where the solution was reached. This curve, then, disregards the effect of the shifts of attack on the problem and is concerned merely with the comparative amount of motility in successive periods of the problem solving attempt.

In order to more easily deal with the data statistically the problem solving records were also read in another way. Ten second intervals were marked off immediately before and immediately following the point where each problem was first given, immediately before and immediately after each signal indicating a change in attack, and immediately before and immediately after each point where a signal indicated that a solution had been reached. Then those areas of the record which were not thus included in a ten-second segment were also divided off into ten second intervals. Next the total number of pen oscillations within each of these intervals was counted, after the manner of Irwin's treatment of motility records on infants (66, p. 95). Averages were obtained for each subject, one average for each of the types of interval marked. From these averages were obtained for all subjects showing the number of pen oscillations in each of the types of interval for an average subject.
The maze records were given less elaborate treatment. Each was divided into 5-second intervals, and these intervals were each marked as to whether or not they were "active," that is, contained any movement whatever. Then each maze was divided into quarters, except that the interval in which the solution occurred was omitted in this division, and included with the two following it in a separate section altogether. Thus each maze was divided into five sections, the last including only the solution interval and the two following, and the other four being made up of a quarter of the remaining record, starting where the action on the maze began. The percentage of active intervals in each of these sections was then calculated, and the data combined to obtain averages for all thirty-eight subjects. In this way it was possible to obtain a curve of the same sort as the second one mentioned above in connection with the problem solving part of the experiment.

Results

Figure 1 shows the relative amount of motility that occurs in different stages of the problem solving process. This curve shows the percentage of "active" five-second intervals in each period and indicates that the least motility occurs in the ten-second interval just before the signal marking the start of the problem solving attempt. Since the reading of the problem actually began in this interval, one might expect movement to increase if the subject were "getting set" to solve the problem. But even the next ten seconds following the giving
Figure 1. Motility Curve in Problem Solving, Showing Effect of Shifts.
of the problem are nearly as low in percentage of active intervals. In the next twenty seconds we find increasing movement. This is a period in which the person is presumably pushing toward a solution without any shifts in his mode of attack; however, since shifts were recorded only when E signified verbally that he was making a shift, it is reasonable to suppose that actually shifts did occur in such intervals and that such shifts may have accompanied the increased motility found here. The peaks of the curve are found at points just before shifts were known to have occurred and where the solution was reached. This last is highest of all.

The data presented here suggest that while movement does occur along with problem solving, it occurs the most after the problem is solved or just before a verbal sign is given that the subject is abandoning an old attack to undertake a new one and can hardly, therefore, be thought of as being due to the assuming of postural sets. Two questions arise at this point, however: (1) do the movements at the point of solution mean, perhaps, that the subject is getting "set" for a new problem? and (2) are these differences between intervals reliable?

In order to clear up the point raised in the first question the last forty-five subjects used in the experiment were told definitely when the last problem was presented that it was to be the final one. Presumably an increase in movement following the last problem, then, could not be due to an assuming of a new postural set. Measurements of the motility following the solution of the last problem by these subjects, as well as new readings of the records as described above under
Table I

Means, Differences between the Means, Standard Errors, and Critical Ratios for the Various Stages in the Problem Solving Process

<table>
<thead>
<tr>
<th>Stages with Means</th>
<th>After Problem</th>
<th>Neutral Period</th>
<th>Solution Period</th>
<th>Before Shift</th>
<th>After Shift</th>
<th>Last Solution</th>
<th>Last Solution</th>
</tr>
</thead>
<tbody>
<tr>
<td>2.50±.285</td>
<td>.52±.385</td>
<td>.34±.05</td>
<td>2.99±.485</td>
<td>1.57±.400</td>
<td>.82±.720</td>
<td>1.44±.691</td>
<td>5.56±.804</td>
</tr>
<tr>
<td>Critical Ratio</td>
<td>.14</td>
<td>.54</td>
<td>4.25</td>
<td>5.45</td>
<td>1.14</td>
<td>3.11</td>
<td>4.42</td>
</tr>
<tr>
<td>2.82±.257</td>
<td>.16±.307</td>
<td>1.97±.446</td>
<td>.85±.380</td>
<td>.30±.709</td>
<td>.94±.680</td>
<td>3.04±.796</td>
<td></td>
</tr>
<tr>
<td>Critical Ratio</td>
<td>.52</td>
<td>4.01</td>
<td>2.24</td>
<td>.42</td>
<td>1.32</td>
<td>3.82</td>
<td></td>
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<tr>
<td>2.66±.155</td>
<td>2.03±.423</td>
<td>1.01±.326</td>
<td>.46±.661</td>
<td>1.10±.651</td>
<td>3.20±.771</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Critical Ratio</td>
<td>4.90</td>
<td>3.10</td>
<td>.69</td>
<td>1.69</td>
<td>4.15</td>
<td></td>
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<tr>
<td>4.69±.388</td>
<td>1.02±.479</td>
<td>1.57±.766</td>
<td>.93±.740</td>
<td>1.17±.847</td>
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<tr>
<td>Critical Ratio</td>
<td>2.15</td>
<td>2.05</td>
<td>1.26</td>
<td>1.38</td>
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<tr>
<td>5.57±.280</td>
<td>.55±.717</td>
<td>0.09±.689</td>
<td>2.19±.803</td>
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<tr>
<td>Critical Ratio</td>
<td>.78</td>
<td>.13</td>
<td>2.75</td>
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<tr>
<td>5.12±.660</td>
<td>.64±.912</td>
<td>2.74±.1002</td>
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<tr>
<td>Critical Ratio</td>
<td>.70</td>
<td>2.73</td>
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<td>3.76±.629</td>
<td>2.10±.680</td>
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<td>Critical Ratio</td>
<td>3.09</td>
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<tr>
<td>5.66±.753</td>
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<td>Critical Ratio</td>
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"Procedure," were treated statistically. The results are shown in Table I. Here it is seen that the amount of movement at the time of solution and after the last solution is significantly higher than either the periods just before or just after the presentation of the problem or the periods which were apparently devoted to "just plain thinking." Also, the period just before the signal showing that the subject had given verbal evidence of an abandonment of his mode of attack is significantly greater in motility than the period just before the presentation of the problem. This evidence seems to the writer to prove definitely that a person who gets "set" mentally to solve a problem does not reflect that set posturally.

Figure 8 shows the motility accompanying problem solving without regard for shifts, and superimposed on it is a similar curve for the motility accompanying the solving of a pencil maze. They both reach a low point in the middle, and their highest points are found at the points of solution. Since the mazes used were on the order of a spiral, blind alleys were encountered much more frequently, in point of time, at the start than after some progress had been made. Also, the nature of the maze situation was such as to show the subject that he was making progress. This, of course, makes the maze situation quite different from that involved in the solving of the problems, where the longer the subject worked the more discouraged he was likely to become; and such differences might account for the difference in the amount of motility shown. The increasing encouragement and the relatively decreasing number of impasses may be reflected in the decreasing motility shown in
Figure 2. Motility curve in solving problems and mazes, showing merely successive periods.
the first part of the curve for the maze. The leveling off might be due to the reaching of a sort of normal level of motility, which is maintained until the end of the maze is reached. In the case of the problem solving, however, the more a subject works on a problem, the more likely he probably is to run into impasses over and over again, and if each one means a shift, then we have seen that such shifts accompany increased amounts of motility. Why there should be an increase in motility between the first and second quarters of the problem solving attempt is difficult to say. At any rate it may be said that the results of this study do not clearly agree with either of Ruth Clark's statements mentioned above to the effect that "movement increased as thinking progressed" and that the "number of movements tended to decrease as the thinking was prolonged."
EXPERIMENT II

The Problem

In Experiment I it was shown that in a problem solving situation an individual's motility is not due to postural sets appropriate to the task at hand. The literature tells us almost nothing about the factors influencing such motility in a waking person above the infant level. Ruth Clark's observations included the statement, however, that in her study "the gross bodily behavior of each individual represented a characteristic mode of adaptation which he adopted toward all his problems" (32). This statement suggests strongly that an individual's motility is related to his personality makeup. The problem of this second experiment may be stated as follows: Is a person's motility related to factors "within" the person; and if so, what are some of them?

Technique

Since a fairly large number of subjects was desirable in this experiment, as well as subjects of considerable age range, it was considered advisable to use children as subjects, most of whom were too small to give satisfactory results in work with the stabilimeter chair available, not to mention the difficulties involved in arranging "sittings" with a large number of small children. Furthermore, the writer was anxious to try out the effectiveness of an observation.
technique in the handling of a motility problem. Consequently, then, an observation technique was used.

All the observers were members of the child psychology and experimental psychology classes of Louisiana State University, and all were trained in the technique before data obtained by them was used. More specifically, the time sampling method or method of short samples was used. This method has been described by Goodenough and Anderson as follows:

"A series of graded categories descriptive of .... behavior is worked out on the basis of preliminary observation. These categories are described in as detailed and objective a manner as possible. The categories refer directly to the kind of situations .... to be observed. Only one child is observed at a time. The observer who is provided with a convenient record blank chooses an inconspicuous position from which he can see and hear the subject easily. Each observation is continued for a set period of time which is checked with a stopwatch. At the end of this period the behavior is classified and recorded according to the system previously devised. The observer then moves on to the next child in the group and classifies his behavior in the same way. This is repeated until one record has been secured for each child. On the following day, another record is secured for each child, and so on until a sufficiently large number of samples have been obtained for each subject." (71, p. 246f.)

In the present experiment, the length of time chosen as a unit for observation was a "moment." Such a unit was used with satisfaction by Bagman (75, p. 67) in the study of children's companionships, and preliminary trials in the present study indicated that a momentary observation was best suited to the needs of the problem at hand. In other words, the movement or lack of movement of the child at the moment first observed was recorded, and nothing more. In no case was the observer to continue watching the child in order to make a judgment, but he
merely looked at the child and then looked away again, deciding upon his motility on the basis of that momentary glance. Following each such observation checks were made on a mimeographed sheet showing what part of the body was moved or recording "none" as showing that the child was not moving at the moment the observation was made. A sample of the sheet used appears in Figure 3. Inasmuch as the observers were not well acquainted with the children observed, it was considered wise to limit the number to be observed in any one series of observations, and five was chosen as that number. On a few occasions only four children were observed in one series.

Deviation from the method as described above by Goodenough and Anderson occurred in the matter of successive observations on the same child. The nature of the present study was such as to make it unnecessary and unadvisable to make only one observation on each child each day. The method used was to list in advance the names of the children to be observed and then observe each in turn making note on the record sheet of the movements of each immediately after each observation was made. Then, when one observation had been made on each child on the list, the observer returned to the first one again and went through the list once more, continuing in this way until the sheet was filled, giving ten observations on each child. The number of such sheets used in one series of observations depended upon the time that the observer could devote to the task as well as upon such matters as class interruptions, etc. As a rule two sheets were used in each series of observations. The average time elapsing between any two successive observations on
Observer's name __________________________

(Use reverse side for remarks.)

MOTILITY
Symptom Sheet
Weather

<table>
<thead>
<tr>
<th>CHILD'S NAME</th>
<th>talking</th>
<th>face</th>
<th>head</th>
<th>shoulders</th>
<th>arms</th>
<th>hands</th>
<th>trunk</th>
<th>legs</th>
<th>feet</th>
<th>none</th>
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</table>

Note: Record only "definite" movement of the larger, striated muscles, omitting such movements as tremors, twitchings, blinking, breathing movements, etc. Make special note where these seem important.
the same child in a given series was found to be between 1.5 and 2.0
minutes, according to the speed of the observer in making the necessary
notes.

Before observations for the purpose of the experiment were under-
taken, the question of reliability between observers was investigated.
In order to do this all the observers worked in pairs, each pair ob-
serving the same child simultaneously but classifying and recording the
movements independently. The method used to help the observers see the
same child at the same moment was that suggested by Hagman (75, p. 18).
Observer A called the name of the first child on the list. Observer B,
sitting next to A at the side of the room, nodded to show that he was
ready. This was the signal for both to look down at their clip boards.
B tapped his clip board with his pencil, and both looked at the child,
judged and recorded his movement, and went on to the next child. Data
gathered in this way was calculated for percentages of agreements between
the members of each pair of observers, with the result shown in Table II.

Of 893 judgments, 86.7 per cent agreed (Table II). Hagman, who
threw out cases where one observer recorded \_ (incomplete observation)
or \_ (doubtful as to inclusion of activity in criteria), obtained only
83.0 per cent agreement in 688 judgments (75, p. 18). Other studies
give no results to which these percentages are exactly comparable.
Where the time unit used is longer, the reliability is usually not so
great. Hagman lists several such studies, the highest reliability not
dependent upon revision of criteria being that found by Arrington’s
three pairs of observers, who (working on nursery school children)
Table II

Percentage of Agreement between Pairs of Observers on the Various Items of the Symptom Sheet for 893 Observations

<table>
<thead>
<tr>
<th>Talking</th>
<th>Face</th>
<th>Head</th>
<th>Shoulders</th>
<th>Arms</th>
<th>Hands</th>
<th>Trunk</th>
<th>Legs</th>
<th>Feet</th>
<th>None</th>
</tr>
</thead>
<tbody>
<tr>
<td>94.8</td>
<td>83.7</td>
<td>80.2</td>
<td>90.5</td>
<td>81.5</td>
<td>79.3</td>
<td>69.8</td>
<td>9.00</td>
<td>88.4</td>
<td>89.5</td>
</tr>
</tbody>
</table>

Grand Average - 86.7
agreed on 88 to 93 per cent of their judgments as to the occurrence of physical contacts within five second intervals (75, p. 19). The observer reliability obtained in the present study compares favorably with those mentioned, especially in view of the seeming difficulty of the judgments made.

Subjects

In this experiment all observations were made on 123 pupils, ten of whom attended the eleventh grade of the Louisiana State University High School and the remainder of whom attended the seven grades of the Louisiana State University Elementary School. When the observers visited the classrooms to make their observations, they were obliged to depend upon the teacher in charge in each case to seat them where they could see the movements of five pupils; and consequently, there was danger that these teachers might try to arrange that observations be made on those whom they thought would make good subjects. To offset this possible factor of selection each classroom teacher was warned of the importance of a random sampling of the whole classroom. Also, the number from each class was not equal to that from every other class. The number from each grade was as follows:

<table>
<thead>
<tr>
<th>Grade</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>11</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of pupils</td>
<td>15</td>
<td>20</td>
<td>17</td>
<td>13</td>
<td>15</td>
<td>15</td>
<td>18</td>
<td>10</td>
<td>123</td>
</tr>
</tbody>
</table>

The nature of the treatment given this data, however, made the matter of grade level unimportant.
Procedure

The types of procedure were used in obtaining the data for this experiment. First, it was desired to see whether there is any consistency in the relative motility of those observed under similar conditions but on two different days. For this purpose all available observers worked simultaneously on two different days, from 9:00 to 10:00 A.M. on December 16, 1938 and during the same hour on January 13, 1939. Since the nature of the class activities from day to day at a given hour is approximately the same, it was felt that in this way the kind and degree of external stimulation affecting the children’s motility would be held relatively constant. On the first day there were observations made on 72 children, and 91 were observed the second day. Forty children were included in both groups.

The second type of procedure used was planned to give a record of the motility of the same children for every hour of three successive school days for the purpose of studying changes in motility in different parts of the day. This was done by arranging to have ten children in the first grade and ten in the seventh observed during part of every clock hour from eight to twelve in the morning and from one to three in the afternoon for three successive days. Unfortunately, however, bad weather caused some of those being observed to be absent as early as the first afternoon, and in such cases substitutes were observed. Also, the observers failed to appear to make the observations during isolated hours the second and third days, seven hourly observations being missed.
in all, three in the first grade and four in the seventh. Nevertheless, sufficient data was obtained to make it possible to construct curves showing diurnal effects on motility, based on the average number of movement checks recorded by the observers during the various hours of the day. The data obtained by this second procedure were used only to obtain these diurnal curves.

The data obtained by the first procedure described above were treated statistically to discover possible correlations between motility and other influencing factors. All record sheets obtained in the two periods of observing on the two days nearly a month apart were examined for checks and the average number of movements checked for each child for each day was calculated to obtain a motility score for each child for each period in which he was observed. Forty children were observed on each of the two observation days, and their scores showed a product-moment correlation between the two days of .404 with a Probable Error of .09. The difference in the means for the two days was only about one-half its Standard Error (.126 S.E. .129). In view of this consistency in motility between the two days the average was taken for those cases included in both days' records, and the rest of the data for the two days were lumped together to obtain the 123 cases upon which most of the rest of the results were worked out.

The age in months for each of these cases was obtained from the classroom records, as were the I.Q.'s for most of the children. These I.Q.'s were based on scores made in the Otis Group Test of Mental Ability, results for which were already available in most cases. In a
few cases where these scores were not available, the writer gave the
same test to the children to obtain the necessary information. This
took care of all cases above the first grade. In this class, however,
the teacher was frankly suspicious that a timing error had occurred in
the giving of the Otis test in view of the scores obtained. Consequently,
the writer gave all the members of the first grade the Pintner-Cunningham
Primary Examination, and receiving therefrom evidence confirming the
class teacher’s suspicions that the Otis results were greatly in error,
he used in this study the I.Q.'s based on the Pintner-Cunningham test
in the case of all the first grade pupils. Product-moment correlations
were then obtained between motility and age and between motility and
I.Q. Partial correlations were also found.

It was also desired to discover whether the amount of sleep a
child receives is related to his motility. In order to study this
question, a questionnaire was sent by way of the children to their
mothers asking them to state at what hour the child went to bed "last
night," at what hour he got up "this morning," at what hour he habitu­
ally goes to bed, when he habitually arises, and some other questions
that were not used in the present study. A sample of the questionnaire
is shown in Figure 4. In the case of some of the older children the
teachers permitted them to fill out the answers themselves, but there is
no reason to believe that this in any way made the results less meaning­
ful. The questionnaires as a whole indicated that on the night before
the filling out of the questions the average child did not sleep as much
as his parent stated it was his habit to do. Because of this rather
Dear Parent,

We are trying to find out how much sleep is had by the children of the elementary school and are asking you to help us by answering a few questions about your child. Your replies will not be used personally in any way but merely to help us discover how much the average elementary school child actually sleeps, and we are anxious that you be as accurate as possible in your replies. Your assistance in this matter will be greatly appreciated.

Sincerely yours,

Alan D. Grinsted

Date________________

At what time did he (or she) go to bed last night? __________

At what time did he (or she) awaken this morning? __________

At what time did he (or she) get up this morning? __________

At what time does he (or she) usually go to bed? __________

At what time does he (or she) usually awaken? __________

At what time does he (or she) usually get up? __________

Over weekends, is his (or her) sleeping schedule different, as a rule? ______ If so, please explain ________________

Does he (or she) usually take a nap? If so, when and for how long a time? ________________________________

Signed _____________________________

Figure 4
commonly revealed tendency in the answers received, it was deemed best to disregard the statements as to the child's "usual" amount of sleep. (The questionnaires were submitted to the parents in the middle of a school week.) Instead of this information, the amount of sleep that the child was reported to have received "last night" was used to obtain a product-moment correlation between sleep and motility. Of the 123 cases upon whom motility scores were available, sleep data were obtained for 93; and upon these correlations were worked out.

The question of sex differences was also investigated statistically on all the 123 cases.

Results

The product-moment $r$'s obtained between motility scores and the other factors investigated are shown in Table III. In the case of age

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<tr>
<td><strong>Product-Moment Correlations between Motility Scores and Other Factors Investigated</strong></td>
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<tr>
<td>Motility Scores on 2 Different Days</td>
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<tr>
<td>Motility Score and Age</td>
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<tr>
<td>Motility Score and I.Q.</td>
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<td>Motility Score and Hours of Sleep</td>
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and I.Q. correlations with motility scores, partial correlations were worked out, with the result that between motility and age with I.Q. held constant was found to equal: $-.78 +.025$. Motility score and I.Q. with age held constant showed a partial correlation of $-.81 +.021$. 
As previously stated, the correlation between scores obtained on two different days for 40 cases equaled .40±.09, with the difference between the means equalling .126±.04 (SE). This is not only interesting from the point of view of consistency of relative motility between those observed, but it also gives us some hint as to the reliability of the technique used.

In the case of hours of sleep and motility score a product-moment correlation of .27±.06 was obtained on 95 cases. It occurred to the experimenter, however, that this correlation might be merely reflecting the above-mentioned negative correlation between motility and age, since the younger children usually tend to sleep longer than the older ones. Consequently, a partial correlation was worked out for hours of sleep and motility scores with age held constant with the result, $r = .04±.07$. This seems to show definitely that the correlation found above was simply a function of the fact that younger children are not only more motile than older ones but that they also sleep more.

No sex differences of any significance were found, the difference in the means between the motility of the boys (50) and that of the girls (75) being equal to only .101 with a standard error of .150.

In addition to the above statistical results diurnal curves were worked out as shown in Figure 5. Separate curves were developed for the two grades by half hourly periods. A remarkable resemblance is found between the two curves in spite of the differences in grade level. As would be expected in view of the high negative correlation found above
Figure 5: Motility Each Half Hour
Average of Observations
on 3 Successive School Days
between age and motility, the younger group shows more movement throughout almost the entire curve, but the general shape of the two curves is very similar. The two curves are not exactly comparable in view of the fact that the first graders have their recess on the playground from 10:00 to 10:30 each morning, while the older children go out from 10:30 to 11:00. The peak for both groups is reached from 9:00 to 9:30, but the points of lowest motility do not coincide. The seventh graders decrease continually in motility from their high point between 9:00 and 9:30 to their low point, which comes during the half hour the younger children are on the playground. When they in turn go to the playground, their motility rises, but not to as high a point as it had formerly reached or as high as it does reach during the first half hour after their return to the classroom. The first graders curve of motility, however, continues to fall from its morning peak, seemingly arrested in its descent by the playground half hour, then quickly falling to its lowest point during the half hour just after return to the classroom, after which there is some ascent toward the noon hour. Here, it seems, is one of the two outstanding differences in the two curves: the seventh graders reach their point of lowest motility just before going out to the playground, the first graders reaching theirs just after their return. The other noticeable difference between the two groups is seen in the fact that during the half hour just preceding the end of both the morning and the afternoon sessions the first graders show a rise in motility, while the older children do not. Another interesting fact revealed by these curves is that both groups reached both their
highest and their lowest points of motility in the morning session, showing much less variation in motility during the afternoon session.

In general, the results obtained in Experiment II seem to the writer to throw some light upon the question of what is the nature of the factors influencing a person's motility. The relation between lack of movement and age and that between lack of movement and I.Q. indicate not only that maturation is a factor but that the integration of the nervous system must be involved just as it is in the development of a person's intelligence. We have here still more evidence that motility as discussed in this study is more closely related to the kind of "person" one is than to the particular mental activity with which he may be occupied from time to time.
EXPERIMENT III

The Problem

This was really the first experiment in point of time. However, it is mentioned last in this paper because it is not considered by the writer to give results that are more than suggestive. The purpose of the experiment was to determine whether any relationship might be found to exist between motility and so-called personality traits as revealed in a standard measure of such traits.

Subjects

The subjects used were 14 members of a class in educational psychology who were attending the 1938 summer session at Louisiana State University. Most of the subjects were school teachers during the regular school year and consequently were, as a group, somewhat older than the usual college student.

Apparatus

The stabilimeter chair described in Experiment I above was not yet very satisfactory in its performance when this experiment was performed. Instead of pens the polygraph recordings were made with weighted pencils containing soft lead, and the carriers moved the entire pencil rather than merely the point as described in the present setup. The increased friction thus involved, plus the occasional failure of a pencil to record at all for a time, made the results obtained of less value than would
have otherwise been the case. Nevertheless, it is felt that in the
light of the results obtained in the experiments already reported,
these results seem reasonable and highly suggestive. Needless to say,
however, the results here reported are in no sense to be looked upon
as doing more than suggesting the kind of results that might be obtained
from further, less crude experimentation along these lines.

Procedure

Each subject called upon the experimenter, ostensibly for an inter­
view. He was seated in the stabilimeter chair, which was operating at
the time. The subjects gave no indication of knowing that their move­
ments were being recorded or that there was anything unusual about the
chair. After a few minutes of free conversation had taken place the
subjects were asked to respond to a word association test. The words
used were those from the second page of the Pressey X-0 test, the
experimenter merely reading each stimulus word with the subject requested
to respond with the first word occurring to him. Later on those who had
acted as subjects in this procedure were asked to fill out the questions
in the Bernreuter Personality Inventory, the answers obtained being
scored in accordance with the Flanagan Scales, F 1-C and F 2-S, the
keys and norms for which are furnished with the manual of instructions
for the administration of the inventory (15).

Signals made by the experimenter (without the knowledge of the
subject) had left marks on the polygraph record to show when the word
association test had begun and when it had ended. Between these points
the record was divided into ten second segments, and the average amount
of movement for each subject per ten seconds was calculated by counting the total number of pencil movements and dividing by the number of ten second intervals. Rank order correlations were then calculated between the motility averages and the scores obtained on the Bernreuter Personality Inventory.

Results

The rank order coefficients of correlation obtained were as follows:

- Between average motility and the F 1-C scale, \( r = .275 \); and between average motility and the F 2-S scale, \( r = .222 \). Converted according to tables furnished by Garrett (58, p. 192), these coefficients are found to be equivalent to \( r^* \)s of .287 and -.232 respectively. Using the formula for a \( PE \) of an \( r \) found from a \( r^* \), namely

\[
PE_r = \frac{.7065(1 - r^2)}{N}
\]

we obtain the following:

- Between average motility and the F 1-C scale results, \( r = .287 \), \( PE = .033 \)
- Between average motility and the F 2-S scale results, \( r = -.232 \), \( PE = .067 \)

According to Flanagan, the F 1-C scale is a measure of self-confidence, lack of self-confidence increasing with increase in score; similarly, the F 2-S score is supposed to increase with lack of sociability. Accepting this interpretation, then, our correlations here would indicate that under the conditions of the experiment, motility tends to increase with lack of self-confidence; and it tends also to increase with degree of sociability. These results, as stated before, must be considered only as tentative.
DISCUSSION AND CONCLUSIONS

It is often a matter of common observation that an individual in the process of solving a mental problem will strike various poses from time to time as he shifts from one to another mode of attack upon the problem. It has sometimes been suggested that these shifts in bodily posture are really shifts in postural set in the sense that an athlete gets set to run a race, such a set in the case of problem solving being appropriate to the attack about to be undertaken. A study* by Ruth Clark (32), however, threw some doubt upon such an idea, for she noted a characteristic mode of adaptation which the subject solving a problem adopted toward all his problems, which she said "seemed to have no bearing on the solving of the various problems but was simply a bodily adjustment for the better concentration of attention" (32, p. 28). Not only does this throw considerable doubt upon the idea that a person assumes postural sets in thinking through a problem, but it suggests that a person's motility is a characteristic mode of adaptation — perhaps a personality trait.

The writer feels that Experiment I, reported in this study, proves that the "postural set" idea is erroneous. If a person were to "get set" to solve a mental problem or to assume a new mode of attack upon such a problem, it is reasonable to suppose that he would show increased motility just before undertaking a problem solving task or just before making each new attack upon the problem; but the experiment described

* Discussed in the Introduction.
above has shown definitely that a person does not show increased motility at such times. It is true that a person does move while performing such a task; but his movements come at the end of effort, not at the beginning. When the problem is solved, the greatest amount of movement occurs. When a shift in attack is made, movement occurs; but that movement occurs in connection with the abandonment of the old attack, rather than in getting set for a new one. This is shown in the fact that in the experiment as reported the period before the old attack was definitely abandoned is significantly higher than either the period just before the presentation of the problem or the neutral periods. The fact that the movement occurring after the solution of the problem is not due to the subject's getting set for a new problem is shown in the fact that such movement occurred even when the subject knew that he had solved the last problem he was to be given.

What, then, is the explanation of these movements? Perhaps a key to the answer to this question is to be found in H. M. Johnson's explanation of the motility that occurs in sleep. He says,

"The longer a person lies still, the more evident it is that he is asleep to a very important group of disturbances, operating upon, and inside, his skin. As soon as he assumes any given position in bed, certain conditions begin to build up, which presently become irritating. A large area of skin is in close contact with the mattress-covering and a smaller area immediately in contact with the body-coverings, so that cooling by ventilation is prevented. All this skin grows warmer and warmer until it reaches a temperature very near to that of the interior of the body. Moreover, the blood and other bodily fluids tend to gravitate to the parts which are the lowest and there settle, while pressing upon the bodily tissues. The visceral organs themselves are movable and some press upon others, while straining the membranes by which they are attached to the walls of the trunk. The body as a whole presses upon the skin and muscles next to the mattress, and thereby restricts their blood supply."
Some muscles are under tension in maintaining posture, while some joints are cramped. A muscle, even when relaxed, becomes irritable merely from being kept still. All these irritating conditions increase with time, and normally produce a change of bodily position, by which they are relieved. Thereupon, the subject can rest until similar conditions are built up in other regions. To lie still for a considerable time requires a disregard of present irritation, or a condition of sleep with respect to it. (106, p. 251f.)

It does not seem unreasonable to suppose that the motility of the waking person can be explained in similar fashion to the above explanation of the motility of the sleeping person. As Johnson points out, the person who moves while ostensibly asleep is attending to particular stimuli — stimuli operating upon, and inside, his skin. When outside stimuli regain his attention, he awakens. Thus, motility in sleep is positive and purposive. The writer believes that the condition in the case of the person who is not sleeping is similar. It rather seems reasonable to suppose that just as attention is a factor in the motility of the sleeping person, so it is in the case of a person solving mental problems. The person while solving the problem is attending to that job, and is “asleep” to certain other stimuli playing in upon him. After the problem is solved, he attends to the job of adjusting his body to the strains that his lack of motion so far has put him to. Similarly, there may be times during the solving of the problem that he may move because the tensions building up within his body have become so great that they have come to demand his attention. In other words, one’s attention is turned to the prepotent stimulus of the moment, and the stimulus that is prepotent at the moment may not be prepotent at another moment, if another stimulus (in this case a tension) becomes great.
enough to demand the attention. So, if a person sits relatively still until he gets to a solution, a partial solution, or an impasse in his problem, a resultant lagging in interest, either permanent or temporary, permits his bodily tensions to become more important, relatively, than they formerly were. His attention turns to them, and he moves. The results of investigations by Ovsiankina and Zeigarnik (132, p. 254f) imply strongly that the unfinished task holds the attention more strongly than that which is finished. Thus we should expect that the task which is solved would show more movements than occur when the task is unfinished or unsolved. We should expect also, then, that when one of the subjects in this experiment reaches an impasse he should move more than when active in his pursuit of a solution, but less than he would when the problem is actually solved and finished. Inspection of the results indicates that this is probably true, although the differences between the shift periods and the solution periods are not quite large enough to be judged as statistically reliable. Miss Clark was correct, then, in her observation that the movements of a person trying to solve problems "seemed to have no bearing on the solving of the various problems but was simply a bodily adjustment for the better concentration of attention."

If a person's movements have no bearing on the problem solving but are simply bodily adjustments, it then becomes apparent that they must be a function of the kind of person he is at the moment. Experiments II and III give us some insight into the question of what factors do enter into the determination of this motility. It seems not to be related to
the number of hours one usually sleeps, although this study does not
tell anything about the immediate effect of loss of sleep on one's
motility. It also seems not to be related to sex. Johnson found that
college women sleep more quietly than men of the same age, and that
middle-aged men as a class were considerably more restless than their
wives. In the latter case, however, he believes "this difference may
be laid in part to occupation rather than sex," (103, p. 8) and it
seems possible that the day time activities may have been a factor even
in the case of college youths. Garvey reports that among his subjects
of preschool age "the boys rested a little more quietly over the whole
night than the girls" (55, p. 66f), but his differences are not statisti-
cally significant.

Garvey found no consistent age relationship (55, p. 66), but
Johnson says, "On the whole, young children change from one position to
another more often than adolescents," and he find the college women to
be the quietest sleepers of all, for "middle-aged sleepers are satis-
fied with one position for much shorter times .... " (103, p. 8).
Apparenty the negative correlation found in the present study between
age and motility holds also for motility during the sleep of children
of similar ages. In these sleep studies no consideration was given to
the relation between motility and intelligence, so we have nothing with
which to compare the results of the present study showing motility to
be negatively correlated with I.Q.

The results of Experiment III, showing correlations between motility
and sociability and between motility and lack of self-confidence bear
out the idea that a person's motility is really a reflection of his personality. As stated in connection with the statement of these results, however, these findings must be regarded as purely tentative until further study is made upon the question.

The diurnal curves offered in connection with Experiment II agree, insofar as they go, with those results obtained by Szymansky (110, p. 25f), who found that the main periods of motility came in the forenoon and late in the afternoon. Our observations did not continue long enough, probably, to show the afternoon peak. Also, the subjects in the present experiment were free to move as they chose, while Szymansky's were under instructions to remain as quiet as possible. This might make a difference in the results.

So far as the writer knows, there has been no other truly quantitative study of motility reported in which the subjects were unaware that their movements were being measured, except for the studies of infants. The attempt was made in this experiment to obtain measurements of motility on subjects in as "natural" situations as possible. There is, therefore, every reason to believe that the diurnal curves obtained are typical. The writer believes also that the following facts have been established:

1. The stabilimeter chair described in Experiment I is a practicable instrument for the quantitative study of motility in such a way that the subjects do not know they are being experimented upon.

2. The time-sampling technique is useful in the study of motility by observation where quantitative results are desired.
3. Amount of free, gross bodily movement (motility) decreases with age at least up to adolescence.


5. Among children of the ages studied the relative amount of motility among those in a group remains fairly constant.

6. Neither habitual hours of sleep nor sex factors are of importance in their effect upon motility.

7. In periods when a person is trying to think his way through a problem, his motility is not related to the problem at hand but is rather a mode of adjustment, expressing to an extent the individual's personality.

Further Implications

It is always interesting to speculate upon the implications of a study. In the present case it seems to the writer that the results of this study are disturbing to the typically behavioristic point of view. If the idea holds that a person in a problem solving situation employs implicit trial and error movements, those movements are not large enough to be demonstrated on the basis of that person's gross bodily behavior. In fact, it has been shown that a person's motility is rather a function of factors other than those directly involved in the problem solving behavior. A person moves when he is not busy in his search for the solution. If, then, the idea of implicit trial and error behavior in such situations is to be held at all, it must be conceived of as being
much more "central" than has usually been supposed. Perhaps the "postural set" is a "nervous system set," rather than a "muscle set." Such an idea is certainly not in accord with popular notions, but it seems like the best way out of the dilemma which here confronts us. Indeed, it is conceivable that such occupation of the nervous system at a given point in the problem solving process accounts for the lack of motility found in this study at the points where the subject was "attending" to the search for a solution. It seems almost as though the system might be "geared in" to do some implicit behaving, and that then the "gears" might be "shifted" to permit some necessary bodily adjustments, after which a "shift" back again might permit some more implicit trial and error. All this is, of course, mere speculation, but it is interesting from the point of view of psychological theory.

Addenda

Since the major part of this paper was written, two articles have come to the writer's attention which are of interest in connection with the present study. The first is a discussion by G. L. Freeman (51), in which he discusses the problem of mental "set," comments upon the fact that it has never been adequately defined, and finally suggests that it is a "central expression of the limiting effect of proprioceptive-tonic activity upon exteroceptive-phasic activity." The other interesting article is that of Balazs, abstracted by P. V. Schiller (Psychological Abstracts XIII, No. 1875) as follows:
"A mental problem was given and it was observed in what stages of the solution fidgeting occurs. Some subjects start manipulation before the problem is fully understood; thus it seems that fidgeting aids concentration. While starting the effort no one ever manipulates, but as soon as difficulties arise manipulations, especially with the fingers, occur. If the subject is successful or if he is not at all interested, fidgeting returns. Especially frequent manipulation was found in the case of a conflict, when the subject tried to escape from the task. While getting the solution nobody manipulated. Summing up: any failure and any freedom of prepared activity seems to give rise to fidgeting. Mental activity, hindered, raises a need for satisfaction in another territory in which it is not threatened by failure. Fidgeting thus seems to be a sort of compensatory activity."

Freeman's definition of "set" is not, perhaps, out of line with the findings of this paper; and the study by Balazs, although seemingly mostly observational, verifies beautifully our findings as reported above.


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BIOGRAPHY

Born in Orange, New Jersey, on July 20, 1905, Alan D. Grinste attended the public schools of that city and was graduated from the Orange High School in June, 1923. After doing office work in New York City for four years he entered Maryville College in the fall of 1927, transferring to Bucknell University, Lewisburg, Pennsylvania, one year later. From the latter university he received his B. A. degree in June, 1931. During the following two school years he held a teaching assistantship at the University of Florida, where he was elected to Phi Kappa Phi, national honorary scholastic fraternity, and from which university he received his M. A. degree. In his work for this degree he majored in psychology and minored in sociology and philosophy. His Master's thesis was entitled: A Study of Certain Personality Traits among Criminals.

During the next two years Mr. Grinste taught psychology and sociology at the University of Florida. Then, in 1935, he went to the State University of Iowa, where for two years he held research appointments in psychology and child welfare, leaving in the fall of 1937 to accept a teaching fellowship in the psychology department at Louisiana State University, where he has since remained. He is now a candidate for the degree of Doctor of Philosophy.
EXAMINATION AND THESIS REPORT

Candidate: Alan D. Grinsted

Major Field: Psychology

Title of Thesis: Studies in Gross Bodily Movement

Approved:

[Signature]
Major Professor and Chairman

[Signature]
Dean of the Graduate School

EXAMINING COMMITTEE:

[Signature]
[Signature]

Date of Examination:
May 10, 1939