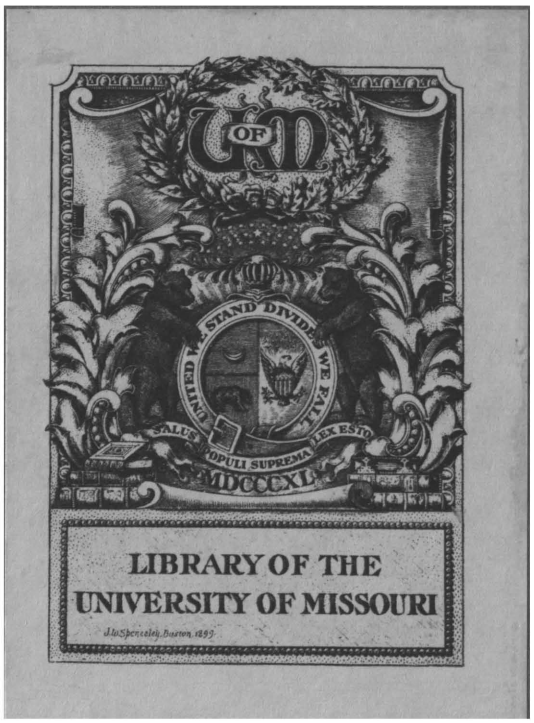

ON THE INHERITANCE OF RYTHM.

by

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Table of Contents.

Page.

Introduction ---------- 1

Examination of some recent Studies of Rythm------ 2

First Experiment----------12

Apparatus and specific Method----------16

Results----------20

Second Experiment----------31

The New Apparatus----------33

Results of Experiments with small Children-------39

Results of Experiments with Adults----------44

A Discussion of Jaques Dalcroze's "Rythmical Gymnastics"----------45

Some experimental Results derived from the use of the Fannie Church Parsons Method------47

On the Inheritance of Rythm.

Introduction.

It is perhaps unfortunate that the term rythm must be used in this investigation, but usage seems to demand it. The objection to the term is that it may have three distinct meanings, physical, physiological, and psychological. The last of these concerns us here. There is certainly no danger of confusing the psychological with the physical rythms, but confusion of the two organic rythms, viz., the physiological and the psychological is a common circumstance. For this reason it would seem well to point out the difference between the two by the following concrete example. The physiologists say normal walking is a rythmical act. Whenever a man is so badly paralyzed in one of his limbs that he is obliged to give the diseased member a peculiar swing in walking they use normal walking as a control and say the afflicted man walks non-rythmically. On the other hand the psychologists call the " hobbling metre" movements of the paralyzed person a two-rythm. The chief characteristic of rythm to the psychologist is the systematic accentuation and subordination of the elements of a series. This interest at once eliminates all physiological and physical rythms from the field of psychology.

Views of rythm ranging in time from early Greek mythology

to the present are based on the assumption that rythm is instinctive. In earlier writings the hypothesis is explicitly stated, but in later editions there was apparently no use in stating a theory so well known. The old assumption is never contradicted and it is only by making the assumption of the innateness of rythm that the problems investigated and the attitude in general of the experimenters can be justified. There is only one other hypothesis that can be substituted for the old one. It is; rythm in general is not instinctive, or in other words it is the result of habit. If, with this assumption in mind, one undertakes a review of the literature on rythm one is bound to be shocked by the preponderance of pseudo-problems and by the great amount of attention that has been given to them. Even in the light of the old hypothesis there is one reason for pronouncing them false-problems. The reference is aimed at the contradictory conclusions of those investigators who, in large measure, were concerned either primarily or secondarily with the same problems, but applied slightly different means for their solutions. Perhaps it would not be out of place to examine a few of the most recent investigations in order that these contradictions may be shown and also that the status of affairs concerning the problem of the origin of rythm may be hinted upon.

Warner Brown ('11) took the problem: "To determine which is the more essential to rythmical grouping, the uniform time of recurrence or the uniform character of the thing that recurs". The conclusion drawn by this investigator leaves the impression

that the phenomenon of rythm if understood must be attacked from the time point of view. Lotze, Herbart, Wundt, and Titchener are supporters of this view. Brown speaks of temporal rythm and of accentual rythm. As a result of his work he is led to conclude that " time aspects are fundamental and the accentual features while necessary are not at the root of the phenomena" because his data shows that " on the whole the temporal structure was maintained twice as well as the accentual ".

Another investigation I may mention is that of J. E. Wallace Wallin ('11) who was concerned with the extent to which the time of rythmical groups may be varied without appreciably affecting the rythm. Concerning this problem he concludes:

" Absolutely periodic or regular occurrences are not essential to the appreciation of rythm. To engender a feeling of rythm always requires a certain amount of periodicity; but the margin of irregularity which may obtain is quite considerable ".

It seems rather remarkable that his conclusions should so explicitly contradict that of the first mentioned investigator. It is in view of the following evidence that he drew the above conclusion. He was able to distinguish five grades or qualities of rythm in relation to the amount of irregularity introduced between the beats of the rythmical series. (The point from which he measures the degree of irregularity is the general average of both the slow and the fast speeds. The slow is 1.075 and the fast

is 0.57 sec.) He speaks of five grades of rythm, excellent, good, medium, poor, and disrupted rythms. The average amount of time displacement or irregularity in terms of percentage from the average amounts respectively to 6.36 %(grade excellent), 8.53 %, 12 %, 14.5 %, and 17.8 % (grade disrupted). In absolute units of time these figures amount to 0.0526, 0.0734, 0.0991, 0.1182, and 0.1488 sec.

Herbert Woodrow ('09) in a paper entitled " A Quantitative study of rythm " dealt with a two-rythm and found that he could change it from an iambic(accent on the first syllable) to a trochaic(accent on the second syllable) and vice versa by making either the time interval following or that preceding the accent longer. By a like process with the three-rythm he was able to pass from dactylic(accent on the first syllable) to anapestic (accent on the third syllable) and vice versa. Some other important facts found by Woodrow are the following:

(1). The amount of rythm as determined by the indifference point may vary with the intensity of the stimulus. That particular point between two forms at which the nature of the second perceived rythm is unpredictable the author terms the indifference point. This is the measure for the amount of rythm.
(2). The amount of rythm is found to increase with the rate.

Woodrow believes that rythmical grouping is altogether a matter of temporal relations and the effect of the accent in determining grouping is to produce temporal illusions through the overestimation of the interval preceding the accent.

In another experiment Woodrow discusses the " Role of Pitch in Rythm". He used here the same method of measuring as before and determined that giving one of the beats a pitch differing from that of the others does not influence the accent. Here too part of the paper is taken up with the consideration of the rate of recurrence of the rythm elements. From the introspections of his subjects he judged that the preferred rates of auditory stimulation of the elements range from 0.305 to 1.37 sec.

Much work has been done on the preferred tempo and t there seems to be about as many different conclusions as there are investigators. For example, this tempo according to Vierordt is 0.62 sec., according to Stevens its lower limit is 0.53 sec., the upper limit is 0,87 sec., for Martius it is 0.50 sec., for Meumann it is 0.40 sec. In these cases we see the preferred tempos vary from 0.40 to 1.0 sec. These statistics mean only that people seldom make like movements with like speeds. The particular tempo a person may choose depends in large measure upon the rapidity with which he ordinarily makes movements. This can be verified as I have done by correlating a person's reaction time and tempo. The correlation is always very close.

Another investigator who is generally considered as having contributed much to the understanding of rythm is Bolton ('93- '95). He said his attempt was to reduce rythm to a more fundamental activity of the mind. In the first part of the paper the author discusses all kinds of periodic movements from " cosmic rythms " to " incubation of fowls", and in the meantime shows

how "organic rythms" have been decidedly affected by "cosmic rythms". After this he talks about " physiological rythms" which are no more than periodic recurrences of certain activities. " Of these walking and speech are the most important and are true types of rythmical activity ". All such movements he calls rythms. These however are not the movements he investigated; but in his experiments he was interested primarily in what he termed secondary rythms. A secondary rythm is derived from a rythmical series of elements by accenting one of the elements through increasing its intensity, pitch, or tone-color, etc. at regular intervals. " Accent simply arranges the materials already rythmical through some temporal recurrence."

In speaking of rythms in poetry Bolton states that the number af accents to the verse may be four, six, or eight. The eight however fails to become popular because it exceeds the mental span. Because of the limitations of the mental span the accents are limited to those two or three numerically small quantities, and it is only for the sake of variety that verses are made to contain five and three accents. The most primary rythm, "two," according to his way of expressing it seems never to have been used in poetry because of its extreme simplicity. In ordinary life two and its multiples occur more often than three and there are many more associations of four than three, hence groups of two and four occur more often. In a conclusion however the author makes the statement that " a member of a sequence may contain <u>one</u> or <u>more</u> simple impressions" , which expression does

not exclude any number.

The author raises the question of the inherent nature of a rythmical group. The following quotation may serve to show his attitude toward this question:

" The conscious state accompanying each wave of attention groups together of unifies all the impressions that fall within the temporal period of a wave. As a result of a number of attentive efforts a series of auditory impressions takes the form of a sequence of groups. This rythmical grouping is due to the unifying activity of the mind.....
.....Each succeeding wave groups a like number of elements so that the series is conceived in the form of groups. The rythmical grouping is an attempt to conceive a series of sounds in a simpler form ".

It seems to be by this unifying activity of the mind that a series is transformed into a"secondary rythm ". The author's last statement, taking in connection what he has said about the most popular rythm, would mean that the four- and six-groups are simpler forms than five-groups. If all is dependent on the wave of attention then four- and six- should be no more prevalent than five-groups, but according to Bolton's theory of attention waves it would seem that the five- must occur more often than the six- since it is numerically simpler.

~~There can certainly be no basis for saying that a six-group is simpler than a five-group.~~

Stetson ('03, '05) may be considered as beginning where Bolton left off. He is interested to know why the "mind"

possesses this unifying activity, or, as he would probably say, "Why the human organism tends to divide a series of elements into unit groups". He is further concerned with the causes of the organism's preference for certain groups,- why some groups of particular numerical values are performed more often than some others. Stetson was the first to mourn the fact that rythm has always fallen into the hands of the investigators of attention, or the span of consciousness, or the perception of time. He says that not the temporal relations but the movements involved are the fundamental things to study if we intend to understand the phenomenon. He states further that "it is a sheer assumption that regularity is characteristic of the pure rythm; and it is easily proved that very wide irregularities can be introduced into a simple sound series without destroying the rythm". He speaks of the human body as a device for producing rythm. The larger muscles of the arm for example perform heavy movements while in the meantime the fingers execute finer movements. Repetition of this movement of the organ (the arm) means naturally a series of accented and unaccented elements of movement at comparatively regular intervals. " The unit group is the form in which the various muscle-sets and segments of a limb or organ can all work together freely and easily in a single movement cycle". The grosser muscles of the arm themselves are means for the same end. These can be spoken of as the major and minor muscles, the former performing the accentual movements, the latter making the finer movements. Furthermore, combining

several organs we find for example that the hand may make a series of movements which may be accentuated by a foot movement or, perhaps, by a movement of the entire body. These movements may require a short time or they may require a long time. The speed differs with the particular nature of the parts of the body which move.

Stetson makes clear why two and three, including their powers and products, should be rythms, but he does not explain any real distinction between five and three. The following quotation is his only attempt to explain why we often have unified movements (5 and 7 perhaps) in non-rythmical experience. " The unity of an act seems to depend on the continuous character of its constituent movements and on the purposive habit which gave rise to it, rather than on the anatomical relation of the parts involved." He considers five and seven no rythms. This may enable us to understand why he says accentuation and subordination are "perhaps" essential elements in rythmic perception. He seems to believe that there is possibly another essential element hitherto unknown. If these two, accentuation and subordination,are the all important factors then why is it not possible to call five a rythm just the same as four ? If four is a rythm and five is not, then accentuation and subordination cannot be of chief importance because elements can be accentuated and subordinated to form groups of five just as surely as to form groups of four.

I am left to infer from Stetson's paper that rythm is instinctive. One reason for my inference is the fact that he does

not contradict the old view that rythm is an instinctive something. Furthermore this is the only assumption that makes clear his attitude, viz. " There is no reason for assuming that the nature of the unit-group of verse differs from that of other rythms". "Most recent writers are inclined to reduce the types of feet to four; iambic, trochaic, dactylic, and anapestic. All of the numerous kinds of feet occasionally given can be separated into these elementary forms". " The ordering of the unit-groups into larger unities is possibly a matter of historical development and might be studied in primitive art works".

Robert MacDougall ('03) seems to have about the same attitude. With him also time is not such an important factor as it is with many other investigators. He concerned himself with many different problems some of which I shall not discuss in this paper. I only wish to speak of his work since he, like Stetson, superimposed his problems upon a hypothesis of instinctive animal activity. This hypothesis is made no more explicit than Stetson makes it; but I think I am justified in saying he assumes, as does Stetson, that rythm is instinctive. Both are concerned with rythm since it is a particular form of activity peculiar to certain animals and not to others. In other words, they are concerned with the fact that human beings group their actions into certain unit groups.

MacDougall places considerable emphasis on the fact that the human organism prefers certain groups of movements to others. The preferred groups are spoken of as rythmical. The others do

not concern the investigator after they are once determined to be non-rythmical. He makes no search for an organic law to explain why one group should be more preferable than another. To bridge over the difficulty he assumes at the start that rythmical action is innate, meaning to exclude all group movements which are not automatic as being non-rythmical. His non-rythmical groups are the prime numbers higher than three. He does not explain the selective principle which causes the human organism to unconsciously choose either four or six in preference to a five-rythm. This phenomenon can be explained only in terms of organic activity as dependent upon the arrangement of parts of our mechanism. MacDougall convinced himself that the seven movement is not innate (and is consequently not a rythm), and substantiated his conviction by introspections from the subjects who said that seven furnished no feeling of rythm. He calls the two and the three simple and fundamental movements; the movements of six and eight complex and secondary; and the movements of five, seven, or eleven, no rythms at all. One of these introspective statements is the following: " The sense of equivalence fell off at five and practically disappeared at seven beats while groups of six and eight retained a fairly definite value as units in a rythmical sequence". He can only mean by this statement that the ability to accent every second or every third element is instinctive and that four, six, or eight, furnish experiences of rythm because they have the instinctive rythms as their bases. In performing the nine-rythm

the three is still performed; but each unitary group of three becomes one element for the secondary rythm of three or nine. " The nine is a rythm superimposed on the three having as its elements the structural units of the three." This statement is important since it shows that absolute periodicity between elements is not at all an essential factor in rythm.

I have not at all attempted an exhaustive review of the literature on rythm. I have only mentioned in a brief way the contributions of some of those who were chiefly interested in the time aspects of rythmical action and of those who subordinated this problem to others.

The contradictions noted in some of the preceeding studies ~~experiments~~ may not only cause one to be doubtful as to the usefulness of the primitive assumption, but may be cause for his becoming an aggressive skeptic. Let us now examine the following experiment which is designed to test the usefulness of our new assumption.

First Experiment.

In this investigation I wish to concern myself with rythm as a problem of organic activity. For some reason a characteristic of human beings is to divide their movements into certain numerical groups, or rythmical units. A normal adult, if asked to beat a long succession of like strokes or listen to such a succession of sounds, manifests a tendency

to group the elements into periods, that is, successive units numerically the same. The grouping, or accented, element may be a pause, an exceptionally heavy stroke, a change in pitch, or any other means which may likewise serve the purpose. Rythm is just this process of subjectively accentuating and subordinating elements of a series. I purposely speak of units numerically the same without implying true periodicity of time because I mean to lay particular emphasis upon the principles of accentuation and subordination. Time, of course, is important in rythmical grouping since these movements must occur in time, but this is no less true of any other form of activity.

I hesitate to speak of time as being the chief factor in rythm primarily for the following reasons. If it is the chief factor, why is it that in ordinary life the group of five, if made at all, is performed with greater difficulty than the four, or the six, or the eight, but is produced with greater ease than the seven which in turn offers greater difficulty than the six, the eight, the nine, or possibly even the ten? If it is the chief factor there is no reason for such a preference of groups. Again if it is the chief factor the accent would be of no value. There would be no reason for making a distinction between physiological and psychological rythms. This, however, is the very mistake Bolton made. Our movements in rythm must correspond to the normal movements of our organs involved. Our anatomy is not such that any member of our body requires longer than a few seconds to act. The fact that some members

require more time than others gives a basis for understanding the function of the accent, or why it is insisted upon in rythm. There are other things which contribute a meaning to the accent, viz., the fact that in ordinary life we find it quite necessary to make certain tentative or preparatory movements before the real purposive action is executed: Further there is the fact that we are bilaterally symmetrical. This affords us two means for executing like acts. One member of a pair usually becomes subordinated to the other, and makes the accentual movements and leaves the production of the finer movements for the more skilled member. Again right or left handedness ordinarily becomes permanently established at the age of six or seven months. We can therefore see why rythm appears early in life. Our inherited structure is such that in performing purposive actions that require the use of the two hands serially, we must often make weak and strong movements alternately, or one hand must make two movements while the other hand makes only one.

In ordinary life two and three and their multiples are regarded as fundamental rythms and five, seven, eleven, etc. as no rythms. They are regarded thus by MacDougall. He avoided, or at least did not raise the question as to the possibility of making the groups of five, seven, or eleven seem rythmical. Since they were not rythms they were not instinctive and were therefore not considered in the investigation. The question of

chief importance in my investigation naturally follows at this point. It is: is rythm instinctive and if so what kinds ? In order to have a working basis for this investigation I formulated, provisionally, an hypothesis, similar to the one MacDougall was led to assume in view of the same facts. My assumption was that the simple movements of two and three and consequently their multiples were instinctive, and that the prime numbers higher than three were no rythms at all.

One solution of the problem is this:- I can first test the subject's ability to perform the movements of two and three and then his ability to perform those of five, seven, eleven, or thirteen. Of course the error in performing more complicated ones will be greater. If, however, by performing certain purposive actions that will necessitate groupings of five, seven, or eleven elements any one of these can finally be performed with as much ease and accuracy as the two or the three, the conclusion must follow that there is no evidence whatsoever for saying that the simple movements of two and three are instinctive and those of five and seven not; but only that our life actions, espedially early in life, call for these simpler movements more often than they do for the more complex movements, five or seven. It is very seldom that our actions call for five or seven while almost every purposive action we perform involves either two or three. The mere fact that a person can repeat a difficult act until it becomes automatic in as great a degree as an instinctive

one does not, of course, prove that the latter is or is not instinctive, That is, if all that is known is that one is automatic to begin with and the other not. If, however, I have two acts distinguished from one another by so slight a numerical difference that their complexity is practically the same, and their qualitative difference is so great as to cause in real life one to be performed more often than the other the one that is called for more often must become more automatic as probably four as in the cases of rythms of four and five, or six in rythms of six and five, or eight in eight and seven. We can see the nature of the movements and understand the environment that calls them forth. If an intended modification of environment will call forth artificially the undeveloped movement with the final result that it is automatic, we conclude as was stated before that we have as much right to assert that this movement is as instinctive as the two or the three. Such an experience entitles us to speak of rythm as a <u>habit</u> rather than an instinct. It is further evident that the most active individual must develop all movements in a greater degree than the person who is more phlegmatic. Since some quite mature people are inactive I expected to find occasionally subjects without or with very little ryhtmical movement of any sort.

The apparatus to which I shall make reference is represented in Figs. 1 and 2. It consists of two electric buttons in block (A), two rubber mallets (B and B'), an exposure

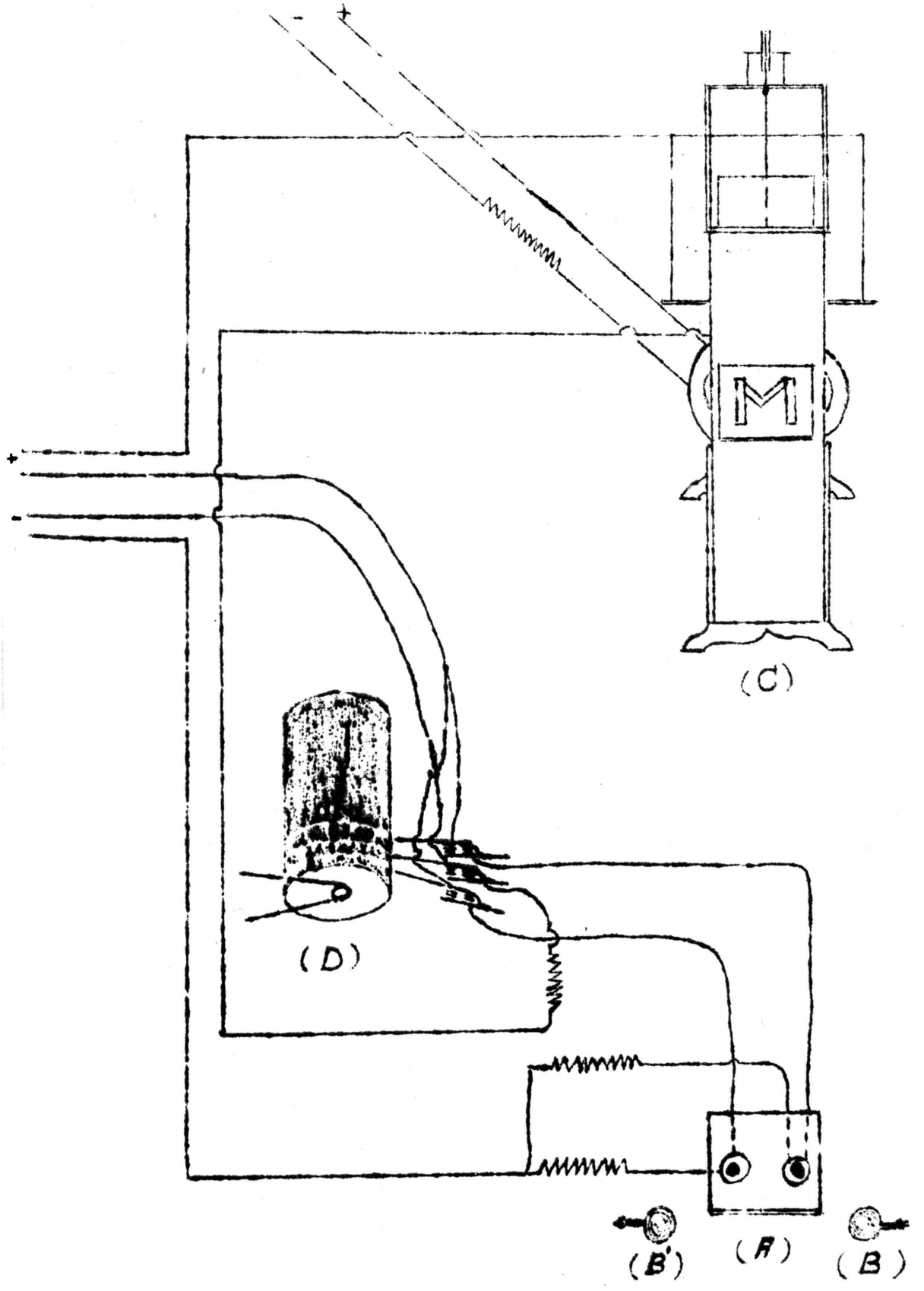
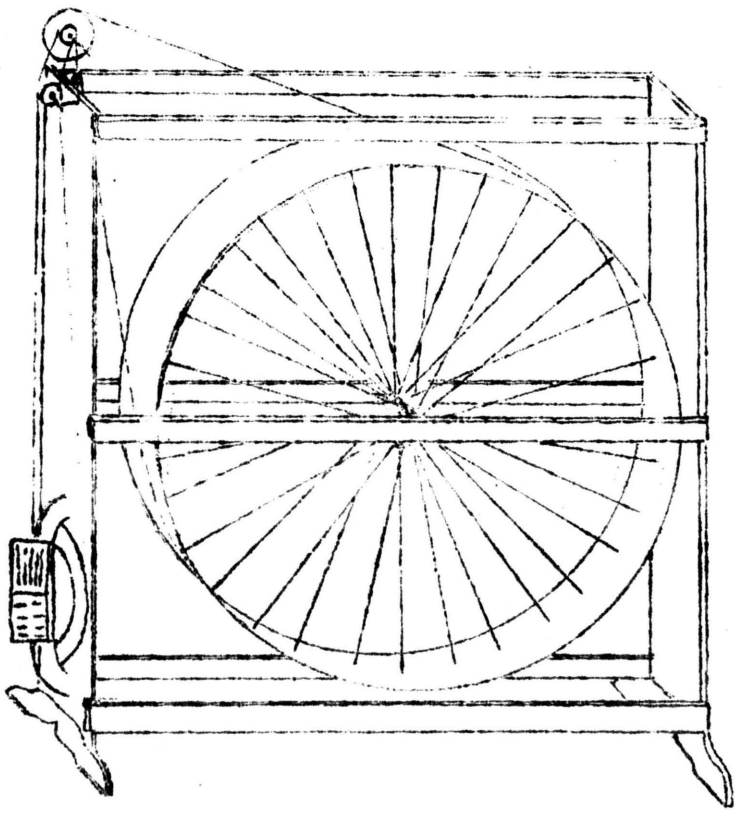
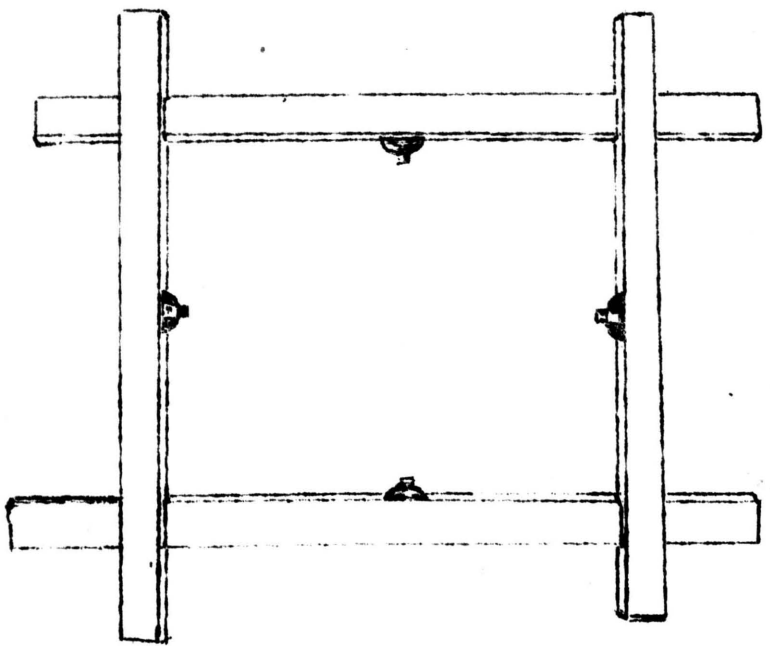


FIG.1



(F)



(E)

FIG. 2.

apparatus (C) showing thirty-two different stimuli, a kymograph (D), and an especially arrangedcframe (E). (F) represents a side view of the exposure apparatus (C). The two buttons when beaten with the mallets make and break circuits to run two electric markers on the moving kymograph drum. The exposure apparatus serves the purpose of detracting the subject so that he can not count his strokes. An electric marker is also connected up with the exposure apparatus to record on the kymograph the time of appearance of each stimulus. The frame (E)) bears four bells so arranged that almost any numerical type of purposive movement can be performed on them. Generally the purpose is to sound one or more bells. No counting is allowed while doing so. The movements which a subject has to go through are these:

Take for example the five-rythm. He first performs a purposive movement on the frame which necessitates a combination of four light movements and one extra heavy one to sound the bell. This continues for ten minutes. He is then tested out to see if the special activity has caused him to improve over a previous test. In the test he takes a rubber mallet in each hand, and with the right hand (if he is right handed, otherwise he will use his left hand) he beats the electric button every time a stimulus appears on the exposure apparatus. In order to make the movements rythmical he accents every fifth element. This is done by bringing more muscles into play by

beating with both hands simultaneously. This test would be valueless however without the function which the exposure apparatus serves, as stated above. The detraction is possible since the appearing stimuli on the apparatus which marks the times for action must be read off aloud as the beat is made. They must be spoken so distinctly that any possible mistake may be recorded by the observer. The test as to whether any movement is automatic is the subject's ability to produce it while thinking of something else.

The particular movements this division of the paper is concerned with are three, five, and seven. I had subjects ranging in variety from a high school student to a university graduate. Some of these were more mature than others. For this reason I expected a number of peculiar variations to appear. My intention was to get such a great number of subjects that I could disregard such variations, and would at the same time have my conclusions limited to no one particular type of individual. The work was begun with twenty-one subjects. Only fifteen of these however completed the test.

The data sheet (Fig. 3) is the record for one individual. The column to the right is of chief interest in this investigation. It shows the person's ability to arrange his actions into required groups such as five, six or seven elements. The third column to the right indicates the degree of attention the subject can give to something else (The exposure apparatus)

Name F. L. H.

Date	Rythm.	Stimuli.	Calling Error. Correct:	Theoretical No.	Striking Error Correct:	Theoretical No.	Accenting Error Correct:	Theoretical No.
Nov.	3	966	883	966=91%	962	966 = 99%	315	322 = 97%
Nov.	5	1027	933	1027 = 90%	980	1027 = 95%	61	205.4= 29%
Nov.25	5	406	393	406= 96%	398	406 = 98%	75	81.0= 92%
Dec.2	5	570	567	570= 99%	569	570 = 99%	110	114,0= 96%
Dec.9	5	556	550	556= 98%	552	556 = 99%	109	111.2= 98%
Dec.16	5	471	461	471= 97%	467	471 = 99%	89	94,2= 94%
Dec.23	5.	478	478	478= 100%	477	478 = 99%	88	95.6= 93%
Nov.								
Nov.	7	908	851	908= 93%	896	908 = 98%	71	129,5= 54%
Jan.11	7	301	289	301= 96%	295	301 = 98%	19	42.9= 44%
Jan.13	7	225	225	225= 100%	225	225 = 100%	34	36,4= 93%
Jan.16	7	337	334	337= 99%	337	337 = 100%	47	48,1=97%
Jan.19	7	364	363	364= 99%	362	364-= 98%	52	52.0=100%
Jan.20	7	393	393	393= 100%	393	393 = 100%	55	56.1=97%

FIG. III

and still produce rythmical movements. In each of the three columns I took the ratio of the correct production to the theoretical number. In this case(with the three-rythm) the particular number of stimuli happens to be 966. Then the theoretical number of calls is 966, the theoretical number of beats the same, and the theoretical number of accents 966÷3 or 322.

The curves (Fig. 4) show the records of the twelve persons who performed the special activity on the frame (E). The abscissa represents the number of tests, and the ordinate represents the percentage of error. The light continuous black lines are the individual records on the five-rythm, and the continuous black heavy line is the sum of all the individual curves. The continuous red line represents the three-rythm. According to my assumption it may be called the instinctive curve. The first point was obtained by actual experiment, then to get the other points on the curve, which is a straight line, I assumed that if under the same conditions the same twelve persons (or twelve just like them) were tested a number of times they would make the same error each time. I did this to avoid the effect of practice. The twelve subjects (almost all were the original twelve, but some were new, consequently I have another instinctive curve) on the seven-rythm are likewise represented, but with dotted lines. There is no reason for giving the separate individual curves except to show how they fluctuate about the

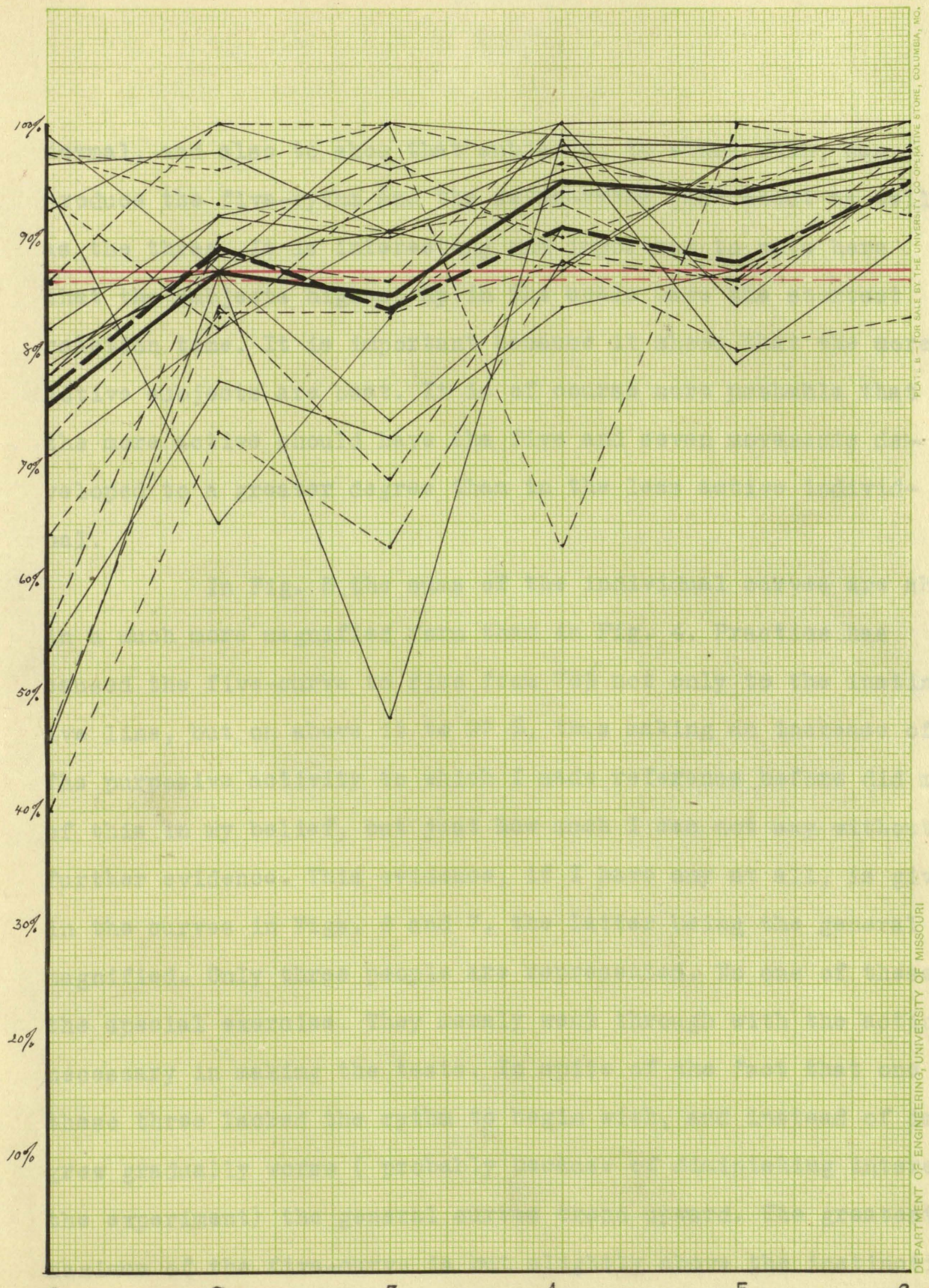

100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
1
2
3
4
5
6
tests
PLATE B — FOR SALE BY THE UNIVERSITY CO-OPERATIVE STORE, COLUMBIA, MO.
DEPARTMENT OF ENGINEERING, UNIVERSITY OF MISSOURI

FIG. 4

normal, and also to show that subjects, to start with, can produce the five-rythm and the seven-rythm just about as well as the three. It is interesting and important to note here that the subjects highest up in the scale have the shortest reaction time. It is important because it shows that the more active subjects are best. It is of course more probable that the more active should have the five and seven movements developed to a greater degree than in the less active individual.

In Fig. 5 the sums of the individual curves are shown in a much more magnified form than in Fig. 4. Practice has caused the five-curve to rise from 76% not only to the instinctive line, but on above it to 97 %, thus making an increase of 21%. The purposive activity to which I made reference before did most of this in my belief, but just how much I can not say without further evidence. This evidence, if I have any at all, is given in the curves in Figs. 6 and 7, the latter being the general curves magnified. Only three people are represented. No one of these took the special exercise. They merely went through with the actions necessary in making the tests. In spite of the fact that one of these three lacked the rythm to begin with, and instead of improving grew gradually worse (probably because of diminishing interest in the experiment) the general curves trend upward. The greatest maximum of the five-curve is but slightly above the instinctive line. The seven-curve never reaches this line. But I consider this evidence so scanty(involving unusual conditions as here

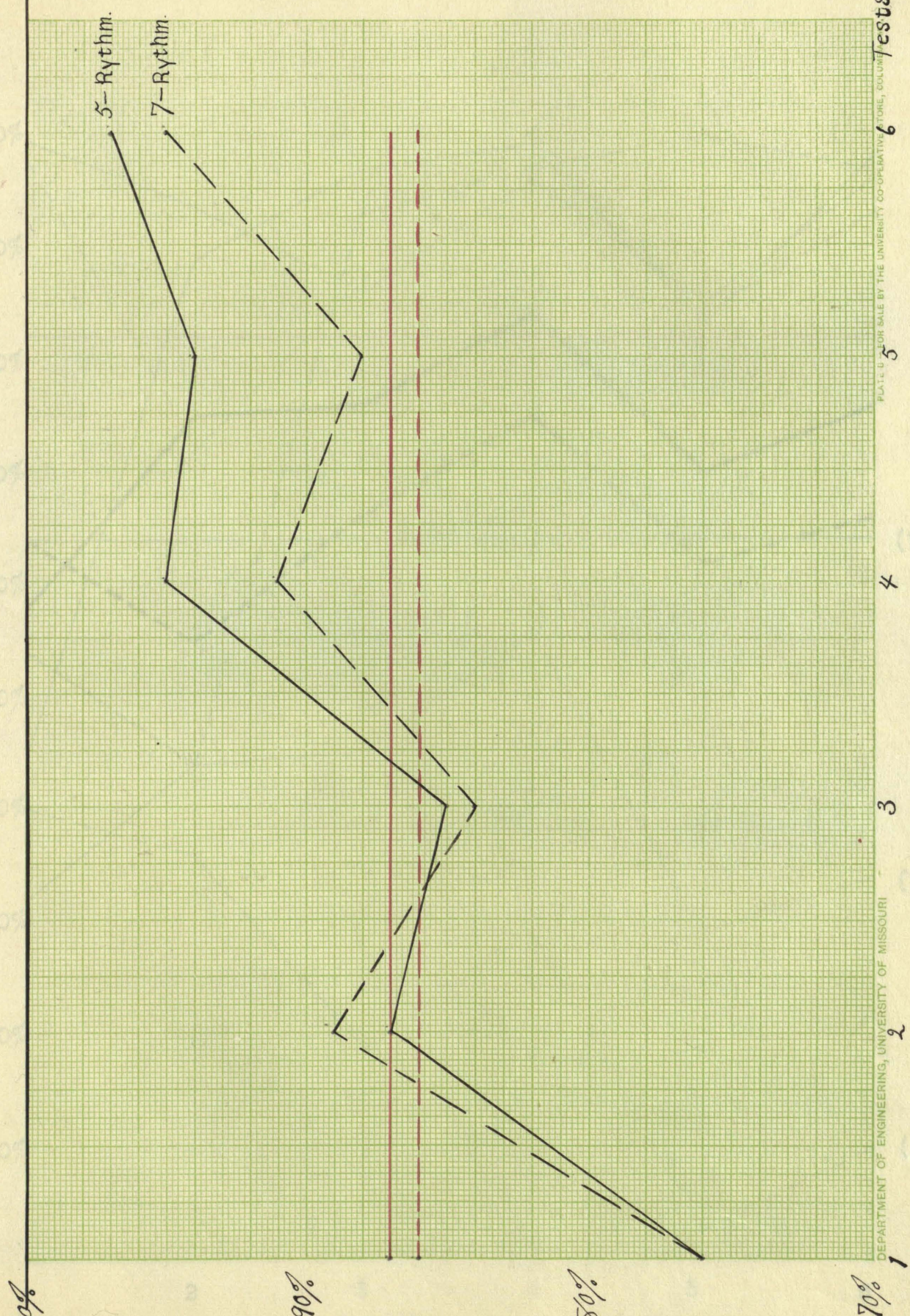

5-Rythm.
7-Rythm.
100%
90%
80%
70%
1
2
3
4
5
6
Tests.
DEPARTMENT OF ENGINEERING, UNIVERSITY OF MISSOURI
PLATE 5 FOR SALE BY THE UNIVERSITY CO-OPERATIVE STORE, COLUMBIA

FIG. 5.


100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
2
3
4
5
6
(W) 3.
(W) 5.
(W) 7.
PLATE 6 — FOR SALE BY THE UNIVERSITY CO-OPERATIVE STORE, COLUMBIA, MO.
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FIG. 6.

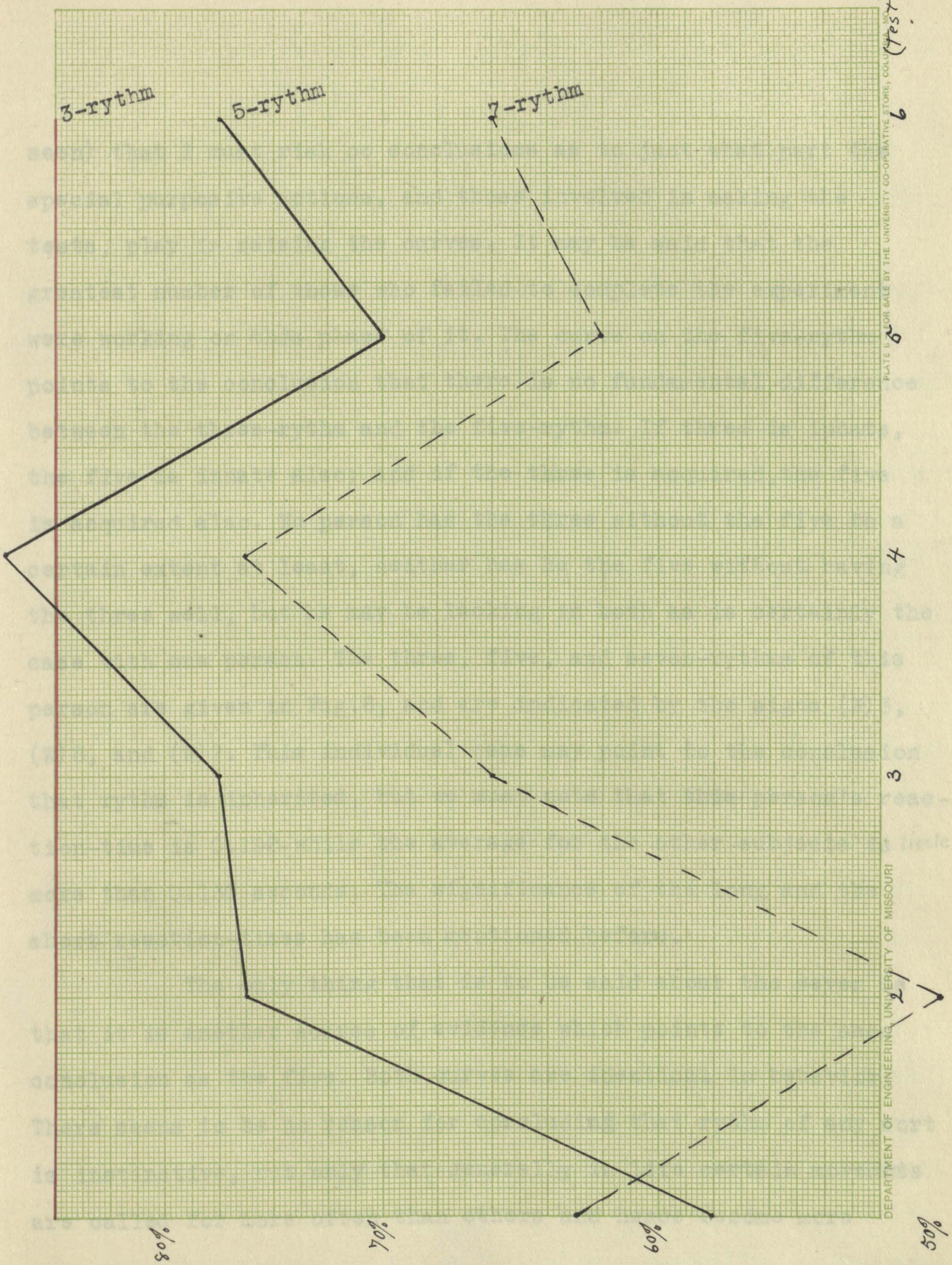

FIG. 7.

seen) that I must risk no conclusions as to just what part the special purposive actions, and those involved in making the tests, play in raising the curves. It may be said that the greatest number of those who failed to complete the experiment were working on this phase of it. The curve on the five-rythm points to the conclusion that there is no fundamental difference between the three-rythm and the five-rythm. If three is innate, the five is innate also; and if the three is acquired, the five is acquired also. No person has the three without the five to a certain extent at least, neither has he the five without having the three well, but he may be lacking in both as is certainly the case with one person. The three, five, and seven-rythms of this person are given in Fig.6, and are indicated by the signs (W)3, (W)5, and (W)7. This individual case may point to the conclusion that rythm is inherited, but we must note that this person's reaction-time is 0.192 while the average for the other subjects is little more than 0.130 seconds. The significance of the long and the short reaction-times has been mentioned before.

The only thing that is to be said about the seven is that it is another source of evidence which points to the same conclusion as the five. Both curves are identical in behavior. There seems to be no reason for concluding that rythm of any sort is instinctive, but only that generally in life certain movements are called for more often than others and hence become more

automatic, more habitual. Our mechanism is so constructed that in performing purposive actions as in work or play the movements have usually a particular numerical make up.

In contrast with MacDougall's notion that seven furnishes no feeling of rythm I must say that I found just the contrary to be true, or at least the contrary effect was produced. Although I do not deny that MacDougall's subjects failed to have that experience I am convinced that this feeling may be found even outside the psychological laboratory. The composer Tschaikowsky was fond of musical measures of five-elements partly because he enjoyed them and partly because his audiences showed great appreciation for his compositions. The strongest evidence is that after the experiment was over certain subjects invariably made greater errors when the exposure when the exposure apparatus was going slowly than when it was going rapidly. If seven furnishes no feeling of rythm then the subject should be more able to produce the seven-groups when the machine goes slowly than when it goes rapidly. When it moves rapidly the subjects must go altogether on their feeling of rythm. This could not be done at first, because the subjects in question had found no opportunity to develop the feeling. The same is true for the five as for the seven. Introspections also supplemented this evidence. I think considerable confidence may be placed in the introspections for I was careful at all times to keep the subjects from uhderstanding the problem. I told them at the end. This eliminated the effect of suggestibility on the part of the subjects. It was difficult if not impossible for them to

tell when they were making themselves agreeable and when disagreeable. In other words they were not in any way influenced by what they thought I expected of them.

In order to show that the increase in accuracy upon the faster appearance of the stimuli is not due to a happening in a desired tempo (the desired tempo has been of considerable interest to a number of investigators) the following facts have direct bearing:

(1). The fast tempo was considerably faster than any of the subjects chose to beat in the absence of the exposure apparatus, (2). Approximately the same results were obtained by allowing the subjects to beat a slower tempo but recognize and call more complicated stimuli thus keeping out number images and enforcing strict attention. This method was quite as effective as that of increasing the speed.

Time does not seem to play an essential role. As to the preferred tempo I found that there was a wide disagreement among the individuals and that some individuals preferred different tempos from day to day.

It might seem that the chief thing to be learned from this experiment is that the major premise of many previous investigators may profitably be replaced by a new one. If we give up the old notion that rythm is instinctive, then the new conception follows as the only and as the reasonable alternative. It appears at this stage more evident than ever that the more

important problems heretofore investigated may be looked upon as pseudo-problems. If habit is at the basis of rythm perception the time element in all rythms should be expected to vary. General agreement as to tempo would be truly remarkable. The time required to execute the movements, the time between the movements, the tempo, etc. should make such fluctuations as can be accounted for only when the nature of the environment that called forth the rythm is well known.

I do not mean to say that all the previous investigations were valueless. The old hypothesis, conducive as it was to the preponderance of pseudo-problems, nevertheless furnished a background for the execution of some very interesting investigations.

SECOND EXPERIMENT.

The following section of this paper has direct bearing upon the problem just discussed, but here the primary undertaking is to secure more stable evidence to determine how much a subject may gain by the mere process of beating buttons which action is necessary in the test. As a problem to be investigated my purpose may be formulated thus; to determine what kind of activity best develops rythm. The methods here used are somewhat modified. Instead of having certain persons work on one problem and certain ones on another, I had all the subjects work on both problems. One subject

beat the five-rythm on the electric buttons and counted while so doing, then he was tested to determine the improvement this practice caused. To develop the seven-rythm the same subjects performed the special activity of seven on the frame, and then was tested. One case alone cannot furnish evidence as to the part played by purposive activities in rythm formation, so another subject, instead of beating the five-rythm on the buttons and the seven-rythm on the frame, reversed the process. This arrangement was extended to all the pairs of subjects. Particular pains was taken to make the frame work purposive activity which required no counting. This was done that the ordinary conditions of life might be approximated. In life we do not ordinarily count our movements, for example, in eating, working, playing.

The experiment already described was conducted during the first semester of the school year 1910-11. While one short semester is not sufficient time to arrive at substantial results we may look upon it as a preliminary to give a more comprehensive view to the problems which the following portion of the investigation concerns. This time, instead of having such a large number of subjects as before, I dealt with a few intensely. By taking an interested few I secured an ideal degree of regularity and promptness. Heretofore my subjects had no incentive to come to the laboratory except that I asked them. With some of them promptness was entirely out of the question. Further troubles to hamper the experiment were due to the apparatus. The electric buttons were small and hard to hit. Every time a subject missed a button the er-

ror was recorded against him. It is quite evident I think that to miss a button is an error of little importance. I am interested in the execution of the movement, and not whether it was made with such accuracy as to hit each time a small button. The constant noise made by the exposure apparatus was no serious difficulty but was not at all desirable. The most pronounced objections to the exposure apparatus were the following:

First, the streaming effect of the letters as they passed the slot in the apparatus often caused the subjects' heads to " swim ". After one watched the letters go by for a while he often experienced the very common illusion of stationary letters and a rising frame. Secondly, my only means for regulating the speed of the exposure wheel was to increase or decrease the resistance in circuit with the motor that drove the wheel.

As to the apparatus several changes were made. In fact the old exposure apparatus was set aside. Another one was built on the same plan as the memory apparatus of ~~Wiordt~~ Wirth. This apparatus (Figs. 8 and 9) is run by a weight and regulated by a metronome. The metronome makes and breaks circuits to run two electromagnets which alternately attract armatures to free the axle carrying a disc of letters. The letters come quickly into view, remain stationary long enough to be clearly perceived, and then pass away quickly so that all the subject sees is first one stationary letter and then another, hence the illusion referred to in connection with the other apparatus is not experienced. With the metronome any

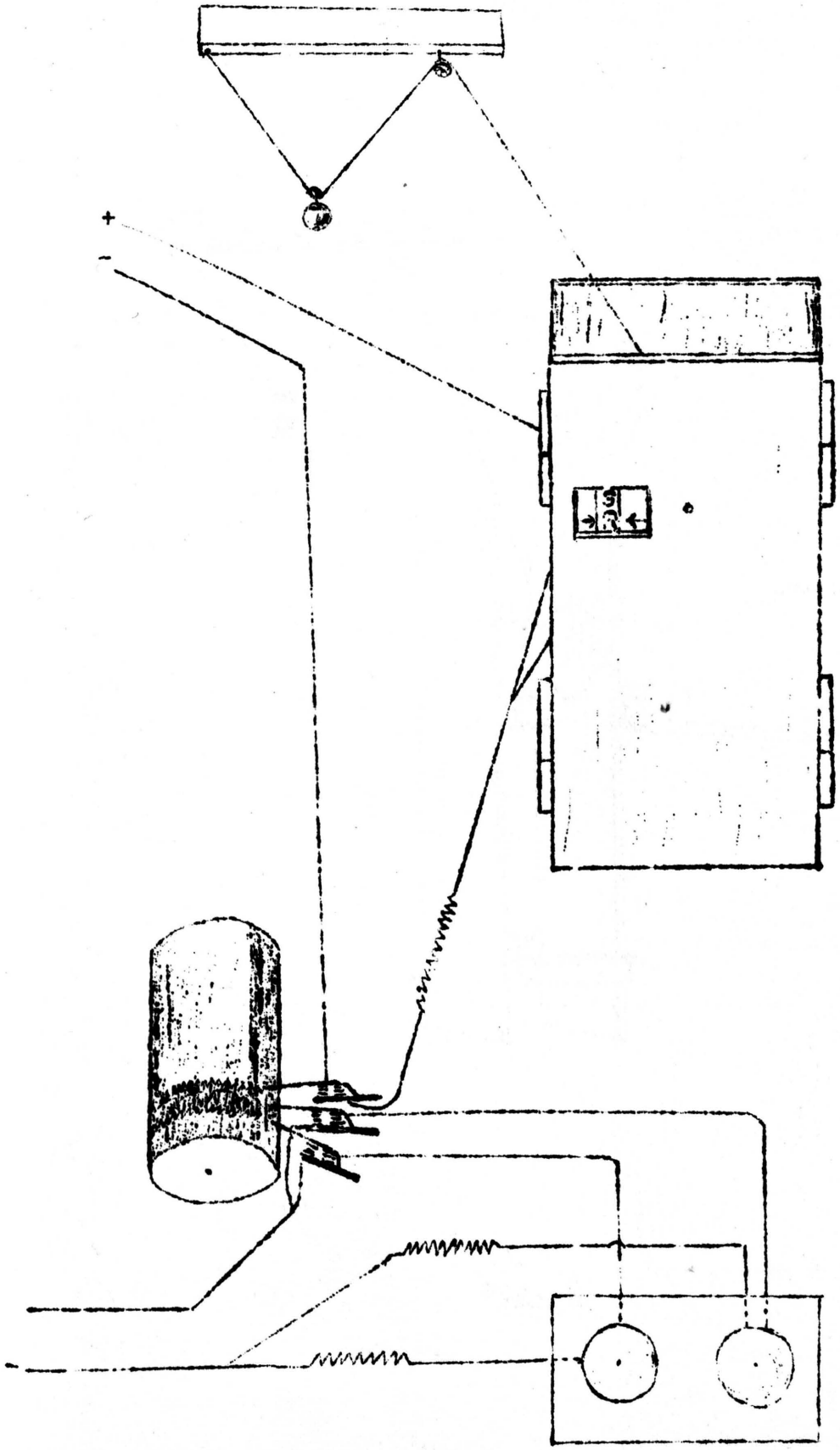

Fig. 8

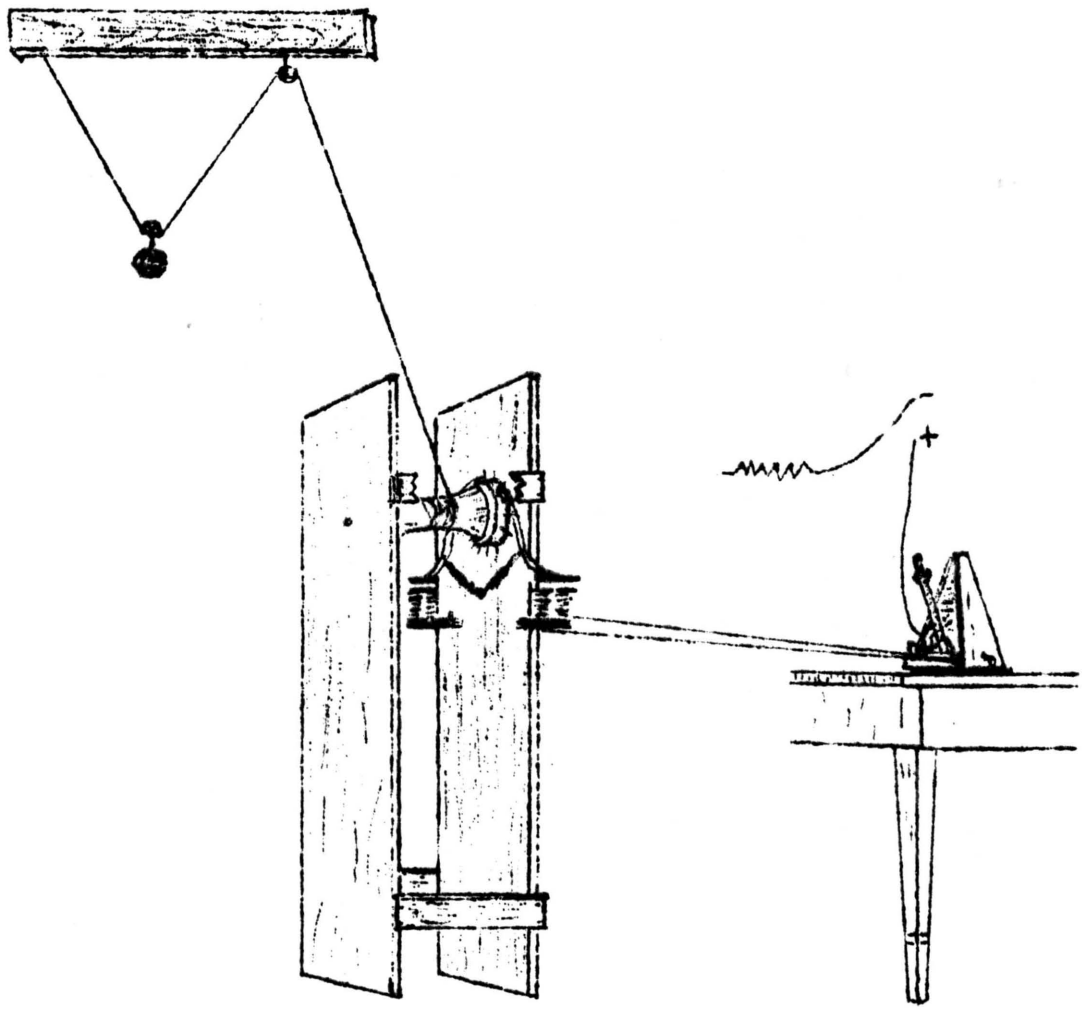

Fig. 9

desired speed may be obtained. For the small electric buttons larger ones of about two and one half inches diameter were substituted. There is absolutely no excuse for missing the large buttons.

Aside from the above difficulties, there are some others which exist by virtue of the nature of the experiment, One of the most serious ones is the fact that any adult can and wants to strike the electric buttons faster than he can recognize and call a series of promiscously arranged letters. It requires some time to adjust one's self to call a new letter, often so long that hand movement is greatly retarded. This condition means that the apparatus must go more slowly than the hand ordinarily makes such movements. The calling makes the difficulty. The letters can be recognized soon enough, but unfortunately this fact cannot help matters since it affords no test as to whether the subject is conscientiously recognizing the letters or counting his strokes. One remedy for the difficulty was to use only three letters, which were chosen beacuse of ease in pronunciation. They were S, O, and R. These were so arranged that no two successive letters were the same. The second remedy was to enlarge the slot so that two letters could be seen at the same time. This enabled the subject to call each letter just as it arrived at a certain point designated by side arrows and just as a hand movement is made, and to adjust his vocal organs for calling the next. I desired that the conditions be such that the subject cannot analyze the units, he is asked to make into smaller units. It may be true that when a subject acquires the seven-rythm he may be able to describe his experience as two groups of two, and then one of three, or any other likely combination of small units such as three, three, and two.

If this be the case then the test to determine whether he has acquired the seven-rythm would be absolutely worthless provided he is allowed to make the same groups during the test. It would only show me that he has the seven-rythm made up of the groups 2, 2, and3, and this may be a three-rythm with the accent on the last element. Without some device for eliminating such subdivions I could say that my test is valuable provided I know the exact conditions, that is, the subjective states of the person while the rythm is being acquired, and could make a selection of those who had developed the desired rythm aside from any smaller groups. This can not easily be done since my only hopes for gaining this knowledge is by introspection.

In the first place I tried to avoid counting while the rythm was being developed. Then for the test I arranged the apparatus so that there was no possibility of even subconscious grouping.

For the small children who acted as subjects the arrangement was sufficient to do this, but for older people it was not difficult enough and another device was necessary. I first tried this scheme. I made electrical connections with a large pendulum which made connections each second to run the exposure apparatus. At the same time the subject beat the rythm in time with a metrónome which vibrated faster than the pendulum. This meant that the tempo of the visual perception and that of the movement were entirely different. It interfered seriously with the production of the very simplest form of rythm, the two-rythm.

No one seemed able to manifest such a degree of divided attention. Those who attempted it were of the opinion that the historical account of Caesar's letter dictations should suffer a displacement from the field of history to that of mythology. We should not expect a person to be able to perform simultaneously two different acts with like parts of the body, each having a different tempo, because there are no demands in life to give us such a habit. The conditions in life for our responding differently to two simultaneous stimuli (even where the question of tempo is not involved) are very rare.

The following scheme was then adopted. The three numerals 2, 3, and 4 were arranged at promiscous places on the disc of the exposure apparatus so that no two of the same kind appeared twice in succession. The promiscous arrangement was such that after the appearance of a stimulus some time elapsed before the next one appeared. During this time the subject beat to the usual click of the metronome and continued to call out with the same loudness the stimulus that last disappeared. By this means all forms of number imagery and practically all other images except the kinesthetic of the movement itself were eliminated. In order to make such a complete analysis certain factors had to be taken into consideration. The one of greatest importance was the fact that the ~~hand can~~ hand can execute the movement more quickly than the vocal cords can pronounce successive letters or numbers after time has been taken to recognize them. To overcome this difficulty numbers were

arranged in such a manner that it was not necessary to recognize them each time the metronome clicked, and too, the range of expectation was narrowed down to one or another of three easily pronounced numbers. Another factor encountered was considerable tendency with adults to count and group subconsciously. The introduction of the nimbers, moreso than the letters, offerdd a decided interference with this tendency. In fact I feel quite sure that the tendency was overcome entirely. The letter system was just as satisfactory for the children as the number system was for the adults. Children do not possess the tendency to beat so fast as older people, and when they are quite familiar with the three letters involved they can pronounce them almost as well as the adult. The more coordinated a person's movements become the faster he wants to beat.

In Fig. 10 the upper curve shows the ability of a five-year-old to perform the movements as indicated by the numerals on the curve. The data for the curve 2, 3, 4, 5, 6, 7, 8, and 9 was taken April 22, 1911, this being three days before any real work was done that might develop the five-, six-, and seven-rythms. If at this time the subject had any rythm it was the two. The further slight variations in the curve seem to be mere accidents. As contrasted with the three, the two always appeared more often while the four-, five-, six-, seven-, eight, or nine-movements were being attempted than the mere numerical difference would demand. This was not true of any other group.

The curve 2, 3, 4, 5', 6', 7', 8', and 9, represents the

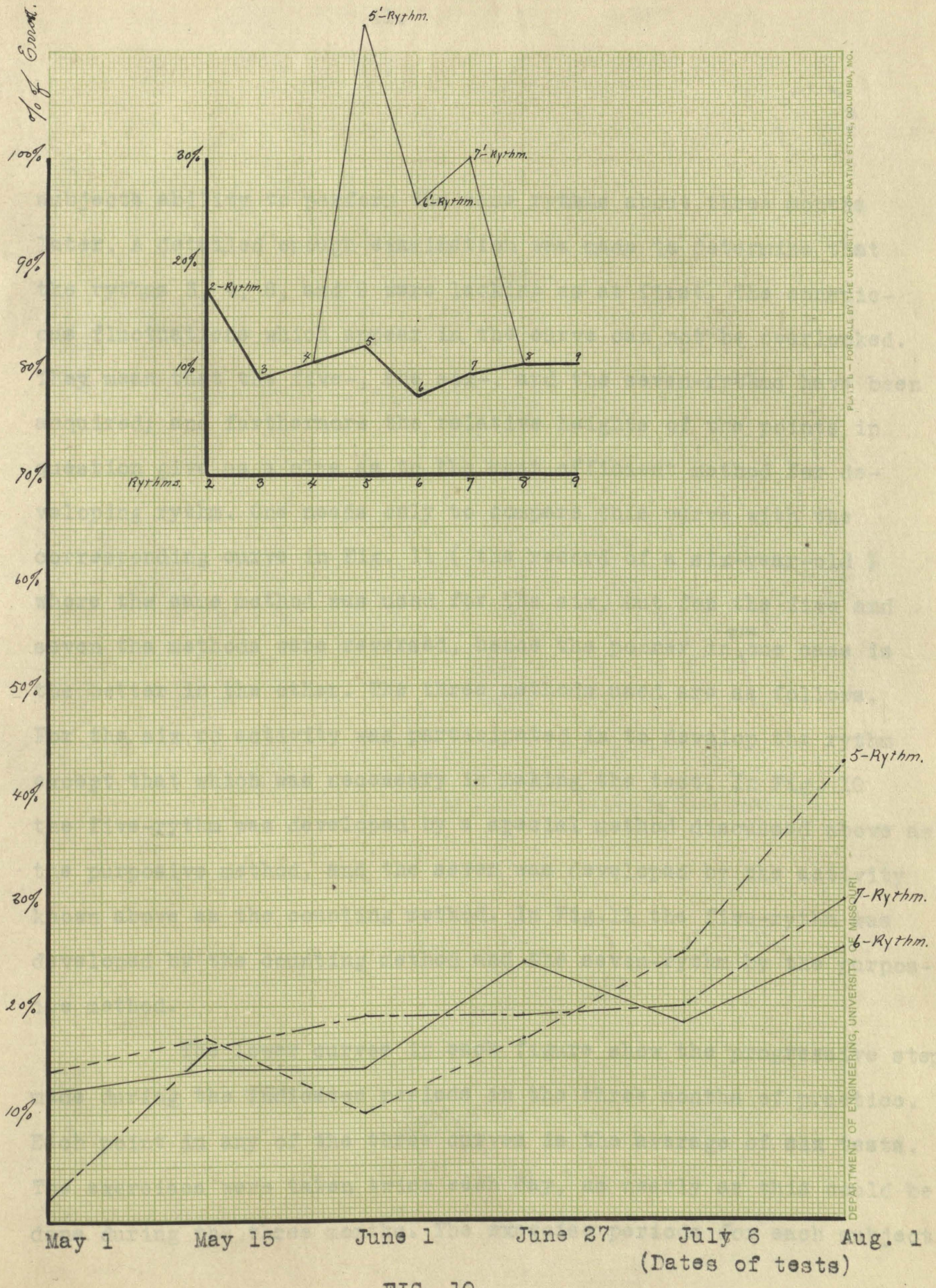
% of Errors.
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
30%
20%
10%
2-Rythm.
5'-Rythm.
7'-Rythm.
6'-Rythm.
Rythms. 2 3 4 5 6 7 8 9
5-Rythm.
7-Rythm.
6-Rythm.
May 1
May 15
June 1
June 27
July 6
Aug. 1
(Dates of tests)
PLATE B — FOR SALE BY THE UNIVERSITY CO-OPERATIVE STORE, COLUMBIA, MO.
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FIG. 10.

subjects ability to perform the same rythms about three months later. A detailed enough examination was made to determine that the rythms 3, 4, 8, and 9 were lacking as at first. The conspicuous fluctuations which appear in the curve can not be overlooked. They mean that the five-, the six-, and the seven-rythms have been acquired; and furthermore the relative heights of the points in question give us a clue as to the most efficient method for developing rythm. One needs only to compare this curve with the corresponding curve in Fig. 11 (the record of a six-year-old) where the same method was used for the six, but for the five and seven the methods were reversed, hence the poorer in the one case is the better in the other. The three methods used are as follows. For the six no activity was participated in to develop the rythm except that which was necessary in making the test. In Fig. 10 the five-rythm was developed by a special method discussed above as the purposive method, and the seven was developed by the activity known above as the counting method. In Fig.11 the five-rythm was developed by the counting method and the seven-rythm by the purposive method.

The lower curves in each figure show the progressive steps made during the indicated periods in the three months of practice. Each point in any of the three curves is the average of six tests. The exercises were taken twice each day, as nearly as this could be done during the three months. The exercise periods for each subject

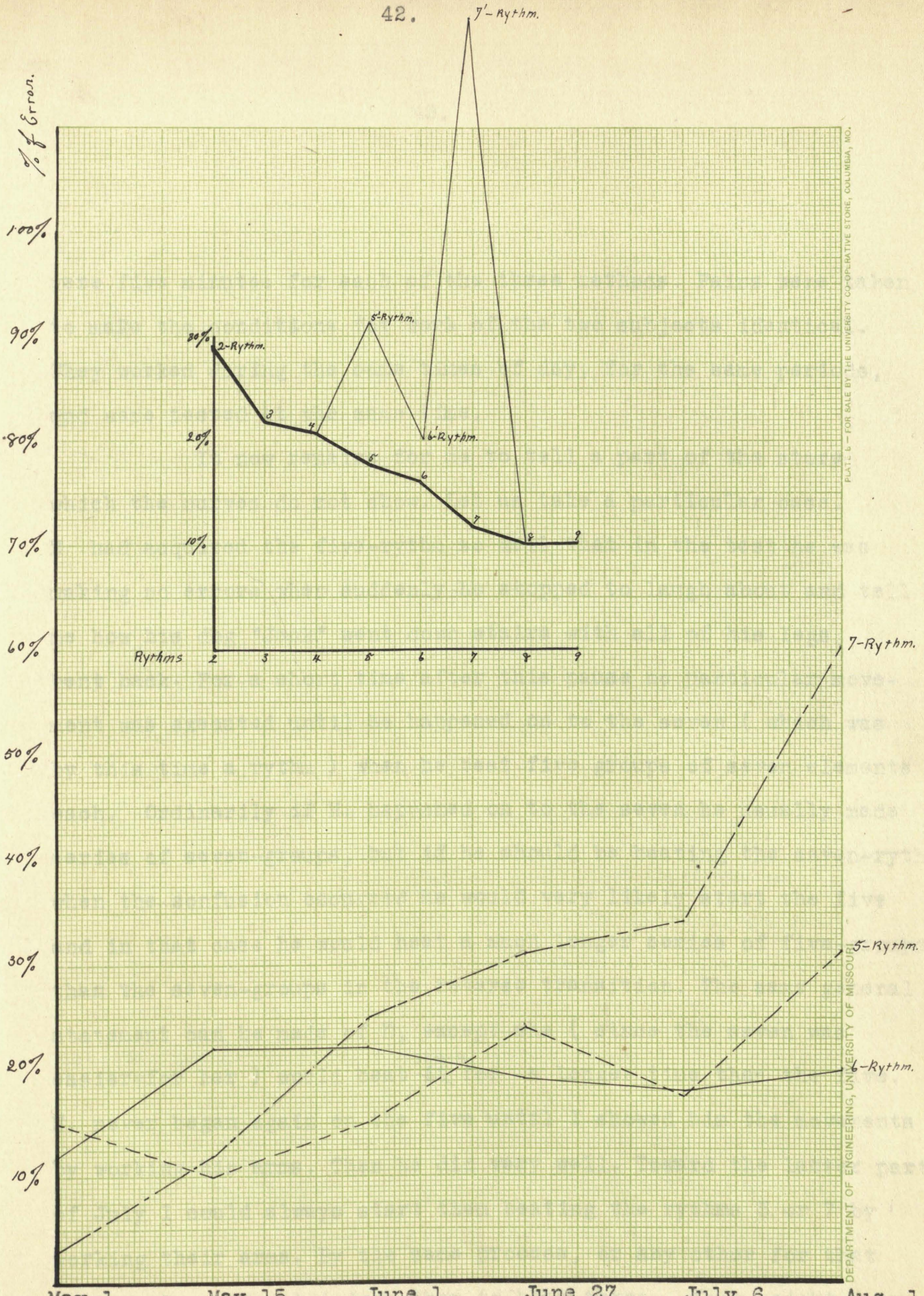


FIG. LL.

were five minutes for each of the three methods. Pains were taken to make the conditions for work of the two subjects identical. They worked during the same times of day, for the same periods, and were tested at the same time.

It now remains for me to tell a part of the story which the curves do not show. Let us take a particular case. H had acquired the five-rythm so well that in the test he was making no errors when suddenly he stopped to laugh about and tell me how his dog "Jack" went down stairs with all of his legs bent back. For a short time after this pause no particular movement was executed until he happened on to the seven (which was by this time a rythm) when he beat five groups of seven elements each. Ordinarily if H. happened on to the seven he usually made series of seven-groups, but if he should be beating the seven-rythm when the confusion occurred he would very likely start the five and in that case he would beat a much longer series of five-groups than the seven-groups in the reverse transition. The same general statement can be made of D, except she (since the seven was easier for her) would beat ~~it~~ seven for a longer time than the five. H. never began again on the five until I showed him the movements by working his arms. Then he did very well. Toward the latter part of July I could always start them beating the rythms 5 or 7 by working their arms. By the same process, or any other for that matter, I could not induce them to beat three-, four-, eight-, or nine-rythms. As soon as I let go they would very soon begin beating either the five or the seven.

After a lapse of five months H. and D. were again tested for the rythms 2 to 9. Those rythms which were learned by the purposive method were reproduced even better than at the previous test. At this test there seemed to be nothing new concerning the other possible rythms except that those learned by the counting method, and the six-rythm, were performed but slightly better than before any practice at all. What little had been learned about them seems to have been forgotten almost entirely.

The subjects were again allowed to practice. This time only the purposive method was used since the relative efficiency of the counting method was quite apparent, and as usual H. developed the five-rythm and D. the seven-rythm. At the close of seven exercises of about twenty minutes each they were able to produce the rythms by making only an occasional error.

Similar experiments with adults and with more complicated apparatus for adults stand in even more striking evidence of these conclusions. Here more difficult rythms were taken into consideration. One person practiced the thirteen rythm on the frame and used the counting method for developing the eleven-rythm, while a second person reversed the process, or beat the eleven-rythm on the frame and counted for the thirteen-rythm. After seventeen exercises each of these subjects made very little error in producing those rythms which were developed by the purposive method

VIZ., that of beating on the frame. On the other hand that rythms were developed by the counting method, no evidence could be brought to bear. Instead of beating this rythm the subject beat either no particular rythm or the one learned by the purposive method. This outcome seems truly remarkable, especially since in the experiment with the small children there appeared a bit of evidence to justify the use of the counting method. The adults not only produced well these rythms learned by the purposive method, but were conscious of almost every mistake made during the tests. These mistakes however, were so few that in one case the error was 5 %. while in the other it was 7 % the greater error being made by the person who beat the simpler rythm, viz., the eleven-rythm.

In view of these results it may be interesting to discuss the " Rythmical Gymnastics " of Jaques Dalcroze. Dalcroze is a music teacher formerly at Geneva but now at Dresden. His plan is to train his pupils to produce rythms well before they begin music. The pupil's training is identical to what has been called the counting method. Dalcroze and his followers are convinced that great benefit is being wrought by this method, but to substantiate the conviction mere opinion has been resorted to. No test has been made to determine whether or not the rythms were actually acquired by the Dalcroze method. It seems reasonable to suppose that the Dalcroze method is not altogether fruitless. The above experiment which would seem to unmercifully discredit this means is concerned with a very small number of seventeen exercises. Perhaps if this

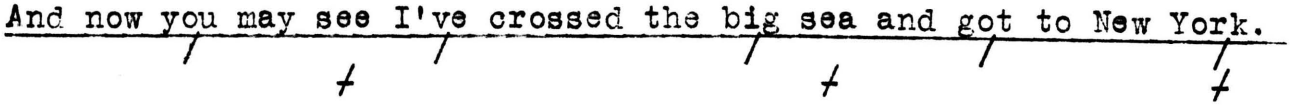
number were considerably increased something would finally be gained by it. At least since this was the case with the small children it is out of reason to think the exercises given by Dalcroze to his pupils is entirely fruitless. The writer is forced to believe, however, that the purposive, or ordinary life method is the more efficient system.

Either method involves the execution of the same movements. The purposive method eliminates all forms of imagery except the kinesthetic of the movements. In the counting method the analysis is not so complete; for by it two forms of imagery are developed viz., kinesthetic and number imagery. There are many activities which demand the kinesthetic imagery, but not the number imagery. (Take my test for an example of such an activity) For a person who has developed only the kinesthetic images, the test is almost identical to the procedure used to develop the rythm. Nothing more nor less is demanded. For one who has both kinds of imagery, the test is markedly a new activity the performance of which necessitates the elimination of the number images. It is an easy matter to inhibit these but to do so seriously disturbs the rythm. To a certain degree the rythm must be relearned. On the other hand the purposive method fits the person for any activity which involves the rythm previously learned.

The extent and ease with which one well learned rythm may be carried over from one form of activity to a different form

is worthy of note. The writer was able to perform the six-rythm without error, by the ordinary way of beating five times with one hand and then with both, simultaneously. After a half dozen ten minute exercises the same rythm could be accurately beaten in a new and more complicated way which will at once be described. This rapid transfer is possible since no new imagery needs to be developed or eliminated to fit the new activity. The training to bring about the transition is the following. Take this sentence of six simple words. I I can not go go. While these words are spoken the right hand beats three times while the left hand beats only two times. The right hand beats to the first I , the can , and the first go, while the left hand beats to the first I, and the not. By using such a simple sentence the subjects can learn to respond differently to certain specified elements of the sentence. This activity is deceiving; for it seems as though the human organism is producing a three-rythm with one member while with another it it is simultaneously producing a two-rythm. Careful consideration, however, reveals the fact that this is not the case, but that only the six-rythm is the outcome of all the movements made. Instead of first one hand and then both simultaneously each hand has a different share of the six movements to execute. Where the hands fail to work with the organs of speech (so that the hands may beat as much as possible at different times and so that each may strike at comparatively regular intervals) the muscles of speech execute the necessary movements to complete the six elements of the sentence.

By like means the fifteen-rythm was developed in two people. This sentence was used, which sentence has fifteen elements.



The single lines (/) below the sentence indicate the words accented by the right hand while the crosses (∕) indicate those accented by the left hand. In either case these hand movements are real accents since each brings into play more muscles than the speech organ muscles which function at every syllable. It is only a very superficial observer who describes this process as involving the production of the five-rythm with the right hand and the simultaneous execution of the Three-rythm with the left hand; for this process does not make one able to perform either the five- or the three-rythms, but develops only the fifteen-rythm quite independently of these.

In this case there was also a rapid transition, but the conditions were somewhat different from the first one. In the one already described the transition was from the simpler method of performing the six-rythm to the more complex form, viz., that method which involves the movements of both hands and the organs of speech as well. In the other case however the transition is from the more complex form(since it was learned first) to the simpler form of activity. The terms simple and complex are used only to designate those activities which necessitate few or many muscles.

The method of the previous experiment was originated and applied by Mrs. Fannie Church Parsons of Chicago to kindergarten

children. There seems to be, however, no reference to her work in the literature. This method does not develop number images, but it does develop visual and auditory images of the words of the sentence which images may be useless and perhaps a hindrance in the second activity, such as my test. Fortunately these images do not stand much in the way since they can be easily inhibited without seriously disturbing the rythm. This system therefore is decidedly at an advantage of the counting method and is comparable in efficiency to the purposive method.

This method is especially welcome in this investigation. Its chief advantage or its greatest theoretical significance lies in the fact that it emphasizes all the while to the subject as well as to the experimenter, that the productiin of the fifteen-rythm, for example, is a process altogether dependent upon kinesthetic images, largely independent of all temporal relations and altogether independent of number images. In view of the results of this method a so called fifteen-rythm, ~~as well~~ as well as others, becomes a myth. At least it is only a convenient term, although a misnomer from the theoretical standpoint, for describing a somewhat complex muscular activity. Number images are avoided absolutely and the temporal relations vary according to the way the subject reads the sentence. When it happens that one word is pronounced much longer than the neighboring ones this shows in space relations in the kymograph markings, even though the subject tries to beat exactly with the exposure apparatus which of course favors absolute time intervals.

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