THE DISTRIBUTION OF ATTENTION. I.

BY DR. J. P. HYLAN.

1. Recent Studies.[1]

The question as to whether the attention can be distributed or divided has recently found interesting experimental treatment by Jastrow and De Sanctis. Jastrow[2] selected two types of processes to investigate their amount of mutual interference when both were carried on at the same time. The first type consisted of finger movements involving rhythm and counting. The beating of the finger was recorded upon a smoked drum; while the second type, consisting of adding or reading under various conditions, accompanied this. It was found that when the subject chose a convenient rate for the finger movements, the adding or reading did not interfere with them; but when the subject was required to tap at his maximum rate, the mental task always caused an interference. Also simple movements were less affected than those complicated with a rhythm, and reading aloud was more disturbing than reading to one's self. The rate of performing the mental work was measured by taking the time necessary to perform a definite task. Simple regular beats did not increase the time of the mental process, but seemed to hurry it up. But when a rhythm was used in the movements, the time was increased, depending upon the complexity of the movement. The greater the number of beats in a rhythmical group, the greater was the interference. The reading of disconnected words was interfered with more than that of words which made sense. These results show that the more simple and automatic the processes, the less is the mutual disturbance; while the more complicated ones, whether pri-

[1] A concise historical review of this subject may be found in James' 'Psychology' in the chapter on attention. A recent and more extended discussion is by W. Wirth, *Philos. Studien*, XX. (Wundt's 'Festschrift,' II.), pp. 487-669.

[2] Jastrow, Joseph, 'The Interference of Mental Processes,' *Am. Jour. Psych.*, Vol. IV., 1891-92, p. 219.

marily mental or physiological, cause a very pronounced interference.

De Sanctis,[1] whose interest has been especially in the pathological aspects of the subject, has conducted several experiments with regard to the power of fixating and dividing the attention. One method of testing the fixation of attention was to keep the subject busy making movements with the finger, and keep time with the strokes of a metronome, upon which the whole of the attention was turned. These movements were registered upon a smoked drum. While the subject was thus engaged, continually stronger and more numerous distractions were given, and he was told to close an electric circuit whenever these distracting conditions caused an interference. The distribution of the attention was tested in a similar way by employing the subject with two, three or more operations, and directing him to give an equal degree of attention to all at the same time. The movements were registered, and the time employed for the operations was exactly measured.

The objection to this experiment is that one of the processes required no constant attention. Work like Jastrow's indicates how automatic and unconscious the tapping in time with a metronome may be. Movements of this kind are so easily fallen in with that they soon come to require hardly more attention than walking or breathing. Hence the supposition that these simple movements constantly retain a part of the attention is groundless.

Another way of testing the fixation and distribution of the attention, and one which the author regards as clinching the distinction between them, was to use a perimeter, having the subject fixate upon the fixation mark of the apparatus. To test

[1] De Sanctis, Sante, 'Studien über die Aufmerksamkeit,' *Zeitsch. für Psychol.*, Band 17, S. 204.

'L'attenzione e i suoi disturbi. Saggio di psicopatologia clinca,' *Atti della Soc. Rom. di Anthropol.*, IV., 1, 468, 1896. Reviewed by Külpe in *Zeitsch. für Psychol.*, Band 15, S. 144.

'Ricerche psicofisiologiche sull'attenzione dei normali e dei psicopatici,' *Bul. Soc. Lancisiana*, XVII., 2. Also reviewed by Külpe in *Zeitsch. für Psychol.*, Band 19, S. 234.

'Lo studio dell'attenzione conativa.' *Atti della Soc. Rom. di Anthropologia*, IV., 2. Reviewed by Binet, *L'Annèe Pyschologique*, IV., 1897, p. 581.

the fixation of attention, the extent of the field of vision was ascertained when the subject was having read to him an interesting story, or being painfully pricked. To do this, supposing the right eye to be used, the slide on the perimeter would be moved from the right temporal to the nasal side until seen. Successive trials of this kind were used to determine to what extent the visual field was contracted by the distraction. As the meridians are numbered from o at the point of fixation towards the temporal side, the greater the reading the larger was the field of vision. In testing the division of the attention, the subject was required to count points, marks or circles exhibited at the point of fixation, and at the same time to attend to the field of vision and note when the object in the lateral field appeared. First, the subject's field was tested under normal conditions with the attention neither distracted nor divided, and the result compared with that when distraction or division was present. An examination of the results of this experiment shows that the extent of the field was somewhat less for distracted than for normal attention; while the average extent for divided attention was less than half that for the normal. The mean variation here also rises to about half the average. According to our author, the contraction of the field is evidence that in divided attention the disturbances are greater than in distraction, and shows conclusively the difference between the concentration and division of attention. Also the division of attention is attended with greater difficulty than its concentration, and in mental disintegration, as with paralytics, the insane, hysterical and aged persons, the power becomes vitiated or lost. The power of distribution thus becomes a prominent feature for psychogenesis, for, developing out of the simpler process of concentration, its accession marks the growth of a greater will power, and the ability to become acquainted with more objects in a shorter time.

I have gone into some of the details of this work because it is on an important subject, and has attracted a good deal of attention, especially in Europe. There is a serious criticism to be made regarding it. It is that the experiments are altogether too crude to deal with the subtle conditions of the problem.

The evident objection to the perimeter experiment is that it is impossible to tie the attention down when there are no distractions, while the difficulty is more obvious with them. It is continually flitting back and forth with reaches of varying breadth and minuteness, and with changing frequency and rapidity. What the experiment with distraction tested was the time and frequency of the fluctuations of the attention from the temporal field with the distracting stimulus as compared with these when the distraction was absent. With an effort to keep in mind the temporal field, the attention was intermittently diverted from it by the distraction, and the object in the temporal field allowed to move in towards the nasal side farther without being noticed than was the case when the distraction was not present. When an effort was made to divide the attention between the lateral field and the fixation point, the fluctuation was naturally more frequent than before, and at the expense of the lateral field, which now retained it for shorter periods and at longer intervals, thus allowing the object to move still farther without being noticed. Another condition which contributed to this result was the fact that counting involved a much more continuous mental effort than the listening to a story or feeling one's self pinched, the agencies used in the distraction experiment. Hence, on this account also, the lateral field was, in the case of division, more neglected. It is but to be expected, therefore, that under these conditions the object in the temporal field could often be moved very far towards the nasal side without being noticed, or that it sometimes reached, as the figures show, clear to the o meridian.

This interpretation is suggested by an experience of several years which have been devoted to experimental study of problems of attention. It is supported by the extraordinary variation shown in the division experiment. If the attention were really divided, why should the object in the temporal field be seen sometimes as quickly as under normal conditions, while at other times it was not seen until it had reached the median plane? Evidently because sometimes the attention was on the temporal field, while at other times it was not.

What becomes, then, of De Sanctis' theory and its significance for psychogenesis? Whatever may be its foundation upon other data, it certainly is not justified by these experiments. Any one can readily discover the great effort necessary to balance one's attention so that its fluctuations shall be rapid and regular between two or more objects. The difficulty of this feat is a sufficient reason why the demented are unable to perform it.

Other authors who should be mentioned are Münsterberg, Binet, Loeb and Krohn. If we discover the time (by the reaction method) needed for naming a member of a single class of objects, as giving the name of an American novelist, and also the time needed for making a comparison, as saying which is liked better, Irving or Cooper, we shall have the time for two different mental acts, each determined separately. The subject may now be required to do both these acts in one, as by telling which of the American novelists he likes best, and the time taken for the combined process. Münsterberg combined acts in this way, and found that while it took 103 σ to name a particular member of a class, and 922 σ to make a comparison, it required but 1,049 σ to do both together, a saving of 46 σ over the sum of the times when done separately.[1]

Binet required his subjects to press regularly a closed rubber tube which caused a tracing on a smoked drum. While this was being done the subject was required to execute some mental work, like reciting by heart, or performing a mathematical calculation. It was found that these operations greatly disturbed the regularity of the compressions. When different tasks were given to the two hands to do at the same time, as that of making two different outline drawings, one drawing would be deformed by similarities from the other. A voluntary process, however, could well be accompanied by one that was purely automatic.[2] Loeb describes experiments quite in accord with these results, and also with those of Jastrow. The rhythm in turning a wheel and that of repeating at the same time verses by heart were found to coincide, or one to be a multiple of the other, and without mutual interference; while raising the pressure on a dynamometer to its maximum was

[1] 'Beiträge,' I., S., 64–188.

[2] 'La Concurrence des états psychologiques,' *Revue Philosophique*, Vol. XXIX. (1890), p. 138.

found to interfere with the performance of mental number work. Thus two simultaneous, maximal, aperiodic processes of innervation which require effort were found to disturb each other.[1] Krohn gave ten simultaneous touch sensations to different parts of the body. Tambours carrying corks as instruments of touch were used, which immediately withdrew when the stimuli had been given. It was found that if the points of contact were somewhat scattered, and the subject attended closely, six out of seven simultaneous touches could be clearly grasped and correctly localized. It should be said, however, that after-images of touch were very persistent and were used to a considerable extent in locating the sensations.[2]

These experiments suggest the need of more critical and exhaustive methods of studying the problem in order that the purely mental factors involved may be given more accurate measurement. Other experimenters will be discussed as occasion suggests. There is also a class of pathological cases which has been interpreted as illustrating a doubling of the mental process. Lack of space prevents their discussion here, although I believe they differ from the normal in degree rather than in kind. At the present time there is an almost universal tendency to believe that the power of division is possessed by the normal mind. We have now to consider some new evidence bearing upon this question.

2. Counting Simultaneous Series of Similar Impressions.

From the foregoing discussion it is evident that two conditions must be fulfilled in devising experiments to test the division of attention. First, no dependence should be placed upon the subject's ability voluntarily to divide his attention; and second, if the mental work employed is continuous, the purely mental processes involved, the simultaneity of which is to be tested, must be accurately measured. In conformity with these principles, the following experiments were planned and executed:

[1] 'Comparative Physiology of the Brain and Comparative Psychology,' p. 289, *et seq.*

[2] Krohn, W. O., 'An Experimental Study of Simultaneous Stimulations of the Sense of Touch,' *Jour. of Nerv. and Mental Diseases*, 1893.

A revolving kymograph drum held horizontally had placed upon it a paper having two series of short horizontal lines extending partly around the drum. A screen, placed closely in front of this, had in it two small openings a centimeter apart, and so placed that when the drum revolved, the lines upon the same could be seen one at a time through the openings; each series of lines being adjusted to its own opening. A fixation mark was placed half way between the openings, and the subjects arranged at a convenient distance for counting the lines. With the eyes of the subject fixed upon the fixation mark and the attention upon one opening, the other being closed, the drum was started at too rapid a rate for correct counting of the lines. A signal was given before the first line appeared, and the rate gradually adjusted for the successive repetitions of the series, so that the maximum rate of counting at which the subject could count correctly and feel a fair amount of certainty in the correctness of his work was ascertained. The exact number of lines was known only to the experimenter. This process was repeated five times for each subject. The time taken for the passage of the whole series past the screen opening was taken with a stop-watch, and this divided by the number of lines in the series, giving the time for each line. The average and mean variation of this for the different trials were then reckoned.

In a similar way, with the eyes fixed as before, and both screen openings in use, the time was taken for counting the lines which appeared in both openings, both series being exposed simultaneously. In this part of the experiment the subjects were directed to divide their attention, if this were possible, between the two openings. Four subjects took part in this experiment, two men and two women, all young, vigorous, and having had some training in experimental work. The mental part of the process was never so rapid as to be delayed by the natural motor accompaniment in the vocal organs, and the number counted was always approximately the same, aggregating from thirty to forty, either when one or more than one series was used, in order to avoid inequalities of fatigue. When more than one series of lines was used this number was divided about equally between the different series. As it was found impossible to keep a separate count for each series when more than one was used at once, a single count was kept for all.

In addition to this double series, a triple and also a quadruple one were used, having three and four openings respectively, for the purpose of ascertaining the effect of an effort for the greater distribution of the attention, and comparing this with the single series. Similar methods and precautions were used here as those already mentioned, although only three instead of four subjects took part.

At first, when more than one opening was used, there was a strong tendency for the eyes to turn directly to the openings in response to the lines as they appeared. After practice had served to correct this, there still remained a responsive shifting of the attention which could not be prevented. A rhythm appeared in the counting which materially aided its rapidity. The nature of this rhythm depended largely upon the order in which the lines appeared, and was hence facilitated by an acquaintance with the series. A feeling of certainty in the correctness of the counting arose in connection with this rhythm, and depended upon the coincidence of the rhythmic beats with the appearance of the lines.

Since the time required for the passage of the lines as a whole was taken for each series, and the average time for the counting of each line in each series was reckoned from this, we have a basis of comparison for the different series. When the time of the different subjects was averaged, the time required to count a single line was found to be 437 σ for the single series, 307 σ for the double series, 278 σ for the triple series, and 1021 σ for the quadruple series. The mean variations were 9 σ, 14 σ, 11 σ and 8 σ respectively. No marked tendency to improve the rate by practice showed. In the single and double series, the order in which the lines were shown presented no difficulty in being formed into a rhythm that could be remembered. This was also true of the triple series if the order were not too complicated. A triple series, made too irregular to allow being remembered, was used, which required 483 σ, or nearly twice the time required for the simpler triple series already reported. The mean variation for this was but 6 σ. The quadruple series was also too complicated to be remembered.

In order to control any details of this result which might come from the peculiarities of the visual sense, the problem was also approached by means of auditory sensations. A toothed wheel was made to revolve by an electric motor, the speed being reduced by an extensive gearing. This, with the additional assistance of a resistance bridge, made it possible to vary the rate of rotation as desired. A light steel spring was clamped in such a position that as each tooth of the wheel passed, a musical tone, having a distinct pitch, was sounded. One side of the wheel was partly covered by a non-conductor, and a metallic arm made to press against the side of this as it revolved. The arm and wheel were put into an electric circuit with a sounder, which was thus made to give a signal for the subject to start and to stop counting the clicks. The breaking of the current, causing the signal for starting, was made by the passing of the non-conducting section of the wheel by the arm; and the making of the current, causing the signal for stopping, was caused by the reinstated contact through the wheel.

The method of procedure was the same as that for the last experiment. The motor was first started briskly and then slowed down until the subject could give correctly the number of clicks sounded. This was repeated five times and very great care taken to make each determination accurate. A second, third and fourth series was formed by having two, three and four springs respectively used at once, each giving a distinctly different pitch, none sounding at the same time, but in succession. The tendency to form a rhythm was especially pronounced in this experiment. Since a distinct rhythm tended to make the clicks fall into groups, and these groups rather than the individual clicks to become the basis of counting, great care was needed in the arrangement of the apparatus to avoid an objective rhythm. In spite of this a subjective rhythm continued to be more or less in evidence, though not of an extreme form. Two subjects took part in this experiment. As practice appeared as a significant factor in the problem, its effect was not eliminated by means of preliminary series of experiments, but distributed between the different series by the method of rotation, *i. e.*, each subject taking one determination in series one, two, three and four consecutively, and then beginning with one again, and thus continuing.

According to the most probable theory of the sensations of pitch, each minor center composing the auditory area in the brain gives a slightly different pitch-sensation in response to the variations in the vibration rate of the stimulus. Hence we would have, *e. g.*, in the quadruple series of our experiment, four brain centers acting in response to the four rates of vibration set up, and producing or correlated with the consciousness of the four pitches. If the attention can be divided, it must act as a multiple consciousness based upon the simultaneous activity of these different centers: and the possible sum of counted sensations in this multiple consciousness should be four times as great in the quadruple series as in the single series for the same lengths of time. We can see how this proportion might readily correspond to the amounts of brain disturbance under these respective conditions. The essential question is as to whether this four-fold disturbance can articulate itself into a four-fold correlated consciousness. Evidently the same argument holds for visual as for auditory sensations.

What came from this experiment with clicks was a gradual increase in the time necessary for counting each click in passing from the single to the quadruple series, the opposite of what would be expected if the attention had been divided. As in the last experiment, the time necessary for counting the sensations in each series was taken, this divided by the number in each, and the time thus found for counting each sensation averaged between the different subjects. We thus get for the single series, 165σ; double series, 180σ; triple series, 182σ; quadruple series, 200σ. The mean variations were 16σ, 30σ, 23σ,16σ, respectively. The effect of practice was general, although most pronounced in the double and quadruple series.

Since in the visual experiment less time was required for the double than for the single series, and less for the triple series, when this could be remembered, than for the double, there is an obvious disagreement between the results of these corresponding series in the two experiments.

In order to test this point as fully as possible, a single and double series of touch sensations were also tried. The toothed wheel used in the last experiment was also used here, but without the steel springs used to give the auditory stimulations. In their place were substituted cardboard rests with small openings in them, and so arranged relative to the rim of the wheel that when the fingers were placed against the openings of the rests, each tooth of the wheel in passing caused a touch sensation. The finger nails of the forefingers were used for this instead of the fleshy parts of the finger tips, since it was found that the latter fatigued, and confused the sensations much more readily. In general, the methods formerly used were also employed here. There were three subjects in the experiment.

With the single series of touch sensations no rhythm was present, only a simple series of unrhythmic touches being felt. For the double series a subjective rhythm appeared for all the subjects, and this seemed to assist somewhat in the counting. The average time for counting a single sensation was 185 σ for the single series, and 189 σ for the double, numbers which confirm the results of the auditory experiment. The mean variations were 6 σ and 18 σ, respectively.

3. Counting Simultaneous Series of Disparate Impressions.

In his experiments with simultaneous mental processes Paulhan found that dissimilar operations conflicted less than similar ones.[1] In order to give the method thus far described as exhaustive a trial as possible, a disparate series was arranged, composed equally of visual, auditory and touch sensations. The toothed wheel before mentioned was used. One steel spring and one finger-rest were arranged for giving a single series of auditory and touch sensations respectively, while a single series of visual impressions was furnished by an arrangement of lines on the side of the wheel, before which was placed a screen with a small opening. The electric sounder gave a signal for starting and stopping. As in the other cases the subjects, of whom there were three, were directed to distribute the attention

[1] *Revue Scientifique*, Vol. 39, p. 686 (May 28, 1887).

equally, so far as this was possible, upon all the three kinds of stimuli. Upon first trying this experiment, the subjects experienced extreme confusion, unlike that caused by the other experiments, and so great as to make it impossible to count at all, except after some practice. After this had become possible, the rate was much slower than for any other series, although it rapidly improved. The average rate for the three subjects per stimulation was 333 σ, while the mean variation mounted to 77 σ, an amount largely accounted for by the rapid improvement in the rate of counting. The amount of this increase of rate is shown in Table I., where the time in seconds for counting the whole series for each successive time is given for each subject. It will be noted that a point is soon reached at which improvement

TABLE I.

Subject	*H.*	*F*	*D.*
1st time.	14.25	16.25	8.00
2d "	14.00	10.50	8.25
3d "	7.50	7.00	8.25
4th "	6.50	7.00	8.00
5th "	6.50	7.25	8.00

stops. With subject *D* this point had apparently been reached in the preliminary practice before records were begun to be taken.

This comparatively slow rate, and especially the confusion, indicate the relative difficulty of combining dissimilar processes, and hence is opposed to Paulhan's results. There is, however, this essential difference between his method and my own. In the experiments described above there was one motor expression for both processes, while with him, as in the case of writing one verse and repeating another, the processes had unlike motor expressions. In attempting, he says, to write one verse of poetry and recite another, sometimes strange mixtures of the two would appear in the writing, but this was not often, at least when the same elements did not enter into the two. He continues: "The words which form a line and the lines which compose a piece each hold well together; in general, always in reciting, I recall one or two features of the lines which I wish to write; after that I think no more, the writing follows mechanically." He describes multiplying figures with one hand and reciting poetry at the same time. He multiplied 421,312,212 by 2, which took six seconds. The recitation of four lines of poetry also took six seconds, but both were done together in six seconds. In trying to make two multiplications at the same time, one with the right and one with the left hand, time was lost. The two operations done at once took thirty-eight seconds; while when done separately, one took fifteen seconds and the other eight.

His experiments seem to have been many and carefully performed. He concludes that 'the most favorable conditions for doubling the mental process appears to be the simultaneous application of the mind to two easy operations and of a different kind.'

When the same elements entered into the two processes the motor expressions evidently became mixed, while when the processes were widely different this was not the case. Thus with divergent motor paths Paulhan found dissimilar processes to be more successful, while with convergent paths we have found similar processes to be more successful. From this we may readily infer that converging paths are more suitable for similar processes, and divergent for dissimilar ones. We have now the question as to whether distribution of attention really took place in these most favorable cases. The following table gives a more detailed view of the results of these experiments. The numbers denote thousandths of seconds required for counting a single sensation. Each number is an average from five determinations.

4. Was Distribution Present?

The table shows a striking uniformity between the different subjects employed. Thus all show a decrease of time in the double and first triple series in the visual experiment; all show gradual increase as the complication increases in the auditory experiment; the time remains nearly constant or the same for the two series in the touch experiment; and all show a marked increase in the combination experiment. The auditory experi-

TABLE II.

Subject.	*Hy.*		*Ka.*		*Ko.*		*B.*		*F.*		*D.*		All	
	Av.	M. V.	Av.	M. V	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.
One opening.	301	9	385	10	693	3	361	16					437	9
Two openings.	242	9	309	10	376	31	299	4					307	14
Three openings.	292	23	261	16			280	4					278	11
Three openings with irregular arrangement.	435	4	458	5			555	9					483	6
Four openings.	901	18	899	1			1263	5					1021	8
One click series.	171	13							150	18			165	16
Two click series.	197	34							162	26			180	30
Three click series.	200	34							164	12			182	23
Four click series.	220	24							179	7			200	16
One touch series.	195	5							180	7	181	5	185	6
Two touch series.	195	5							180	7	192	24	189	18
Combination.	388	129							328	97	279	5	333	77

ment shows the least time and a generally large mean variation. Here the rhythm was most difficult to suppress, a fact which partly accounts for both of these features; for when the rhythm controlled the counting, the tendency to combine single clicks into groups caused a marked increase in the rate, while the successful attempts to suppress the rhythm caused a decrease of rate, and hence made a pronounced variation. It was very difficult to tell when a rhythmic grouping took place. Obvious rhythms were avoided, but it was so difficult to draw a line between the rhythmic and non-rhythmic that it was often difficult in this experiment to know when the counting was properly performed.

It is the difference between the rhythmic and the non-rhythmic counting which probably explains the decreased time of the double and first triple series of the visual experiment and the resulting conflict between these and the corresponding series of the other experiments. With the two and three screen openings of the visual experiment the rhythmic possibilities were greatly increased over the single opening, while the complexity of the series was not necessarily so great as to preclude remembering it, as was the case with the quadruple series. Hence the introduction of a rhythm, with its natural accompaniment of an increased mean variation, caused the greatly increased average rate. In sharp contrast with this double and first triple series is the irregular triple series, which could not be remembered, and where the time was greatly lengthened and the mean variation correspondingly decreased. It would seem to be this, rather than an economic distribution of the attention, which accounts for these decreased numbers and increased variation in the double series, the only ones upon which evidence of a simultaneous distribution could be placed. Indeed, in all the series in which we might have supposed distribution to be possible, the subjects experienced a rapid fluctuation, or oscillation of attention, a fact that of itself argues strongly against distribution.

But did not distribution take place in Paulhan's experiment with dissimilar processes and with motor paths diverging to different organs of expression? It should be noted that Paulhan speaks of simultaneous psychic or conscious acts rather than of simultaneous acts of attention. If we restrict all psychic acts to acts of consciousness, and all acts of consciousness again to acts of attention with their varied aspects as voluntary and involuntary, active and passive, then Paulhan's experiments test the division of attention. I have assumed this much and have regarded attention not as something added to consciousness, but as the character which it from time to time takes on.[1] Our author, however, seems to restrict attention to its more active and voluntary aspects, and to regard it as a phase of mental activity added to consciousness or much narrower than it. Thus, while he notes that the attention often oscillates in his experiments, he does not regard this as a reason for denying the simultaneity of two acts of consciousness. The fact, also, that at least one of the simultaneous processes must be learned by heart as a preparation for the experiment, raises the question as to whether consciousness is necessarily involved in both at the same time, and also places great hindrance in the way of any economic value which might rise from the practice.

One explanation may be ventured as to the cause of the decreased rate of counting the disparate impressions in the combination experiment, and of the same effect in passing from the simple to the complicated series with similar impressions. We know that there is an inertia which affects the functioning of the nervous system. This is illustrated by the time it takes for sensations to become fully felt, and for the transmission of pain. Cattell has estimated that it takes from 47σ to 58σ simply to become conscious of a small object, as a letter upon a white surface.[2] In reaction experiments a preliminary signal needs to precede the signal for reaction by about 1½ seconds, in order to give the attention time to reach the right intensity for the quickest response. When no preliminary signal is given, sensorial reaction is lengthened 26σ.[3] It thus takes an appreciable length of time for a center to respond fully to a stimulation.

[1] I have briefly developed this idea in 'The Fluctuation of Attention.' PSYCH. REV., *Mon. Sup.*, No. 6, p. 62.

[2] *Mind*, Vol. XI. (1886), p. 383.

[3] Wundt, 'Physiologische Psychologie,' II., S. 348.

When, in the above experiments, the intermittent stimulations were most frequent, as in the cases of the double series of auditory impressions where the fluctuation was between two tones only, the centers were kept in nearly as constant a state of excitation as when the series was single; but when, instead of two, there were three and four centers to share these, the frequency for each center must have correspondingly diminished, and hence the impressions occurred when these centers were in a less stimulated, and hence less responsive, condition. Therefore it took a longer time for them to act, and the rate of counting was correspondingly decreased. The presence of rhythm probably explains the absence of this effect in the visual series, where rhythm was not so carefully guarded against, and where the rate of counting increased with the complication of the experiment.

This perhaps explains the gradual increase of time needed for counting the series as they became more complicated, but another cause also made the combination series slow and distracting. This was the difficulty of combining unlike sensations. If one tries to attend, *e. g.*, to a visual and an auditory stimulus at the same time, he will notice a greater difficulty to attend the effort than when the sensations are similar. The mind apparently oscillates between the two rather than distributes itself between them in the case of both similar and disparate sensations, but with the disparate sensations the amplitude of the oscillation seems greater and requires a longer time. Practice, however, rapidly increased the rate of the combination series, apparently by decreasing the time of this transition, a result common to all associative processes.

5. Reactions with Concentration and Distribution.

One experiment or one kind of experiments is not sufficient, however conclusive it may be in itself, to test so general a proposition as that of the distribution of attention, because of the great variety of conditions involved, and of processes which may be combined. An experiment was accordingly devised to test by numerical results whether or not the attention really fluctuates from one to another of two or more objects upon which an effort is made to distribute it.

A cardboard screen was fixed in a vertical position upon a table, with six small openings arranged in a horizontal line three cm. apart. In the middle was a fixation point. Behind these was an electric sounder, muffled to be noiseless, and with a white bob attached. This was so arranged that the bob appeared behind one of the screen openings when the current was broken. The breaking of the current started a chronometer (Verdin), from which thousandths of seconds could be estimated. The subject was placed three feet from the screen with his eyes upon the fixation mark. Upon seeing the object through the screen opening, he reacted and closed the current.

With the subject's eyes always directed towards the fixation mark, a series was taken in which the subject knew in which opening the object would appear and had his attention on it; and also a series in which the opening in which the object would appear was not known, and when the attention was distributed, so far as possible, upon all the openings at once. A preliminary 'ready' was given before each signal, and the reactions were sensorial rather than motor. In order to avoid any influence from the fact that some openings were seen more indirectly than others, the signals in both series were divided equally between the different openings. Three subjects took part in the experiment, and one hundred reaction times were taken from each in each series.

An examination of the conditions shows that the only difference in these two series was that in one the attention was concentrated, while in the other it was distributed over the six openings, so far as this could be accomplished. We may, therefore, assume that the differences in the results of the two series would come from the effort at distribution. If the introspective record and the interpretation of the numerical results in the counting experiments were correct, we should expect an increased reaction time in the unknown as compared with the known series. This would be due to the unavoidable fluctuation rather than distribution of the attention, in the unknown series, between all of the six openings. The reasons why this fluctuation should cause a lengthened reaction time would evidently be similar to those for the increased time of counting as the complication increased in the counting experiments. An examination of Table III. shows this expectation to be realized. The average reaction time for the known series was 161 σ, while for the unknown it was 178 σ, or 17 σ greater.

TABLE III.

Subject.	*Hy*		*D.*		*F*		All	
	Av.	M. V	Av.	M V.	Av.	M. V.	Av.	M V.
Opening known.	160	20	147	17	177	17	161	18
Opening unknown.	164	26	157	23	214	17	178	22

The objection, however, may be advance that the distribution of attention does not necessitate the giving of the same amount to each of the several objects between which it is distributed as would be given to a single object when that received it all. A decreased intensity, instead of a fluctuation, would thus account for the increased reaction time in the unknown series. Hamilton says attention is subordinated to a certain law of intelligence. "This law is, that the greater the number of objects to which our consciousness is simultaneously extended, the smaller is the intensity with which it is able to consider each."[1]

There is a way of testing the validity of this objection. If, when the effort is made to distribute the attention among several objects, it really fluctuates from one to the other, naturally, it would sometimes, in the unknown series, be upon the right opening when the signal appeared, but more often upon the wrong one. In the former case the reactions would be short, while in the latter case they would be unusually long. In other words, the mean variation would be somewhat greater in the unknown than in the known series. The table shows this to be the case; there being a greater variation by 4 σ for the unknown series. Here the exact amount of increase in the mean variation is evidently a matter of chance, since the attention could be expected to be upon the right opening but one time in six, and might be so many less than that as to make no marked increase in the variation. In this case, however, when the right opening would be chanced upon but seldom, the average reaction time would be distinctly increased. A glance at the table

[1] 'Lectures on Metaphysics and Logic,' Vol. I., p. 164.

will make it apparent that this was the case with subject *F*, whose mean variation is the same for both series, but whose average reaction time is increased in the unknown as compared with the known series 27 σ more than for any other subject.

I believe it would be difficult to devise a more convincing test than this experiment affords. Yet, as an effect of practice, there is a tendency for the results to change. In the combination series of the counting experiments, it will be remembered that practice had a marked influence in increasing the rate of counting, an effect presumably resulting from the use of the association paths relating the centers involved. A tendency appeared in the reaction experiment just described, which calls for a similar explanation. At first, when trying to distribute the attention in the distribution series, the fluctuation between the different openings was prominent, but as the experiment continued it became less so, and finally became almost unnoticed. Even the giving of the signal in an unexpected opening in the known series secured a reaction not appreciably lengthened. This raises the question, to which we shall return, as to whether practice may not be the means of making distribution possible, or if it serves to combine into a psychic unity things at first separate and unrelated.[1]

Wundt describes an experiment similar to the above, in which loud and weak sounds were irregularly interchanged as the signal for reaction. As the subject did not know which signal would be given, he was unable to prepare exclusively for the right one. There was an increase of 122 σ in the reaction time over the average of 121.5 σ when the right signal was

[1] The present study was continued for a period of four years, although during one of these years experimental work was suspended as it was impossible to take up the work at laboratories visited in Germany. The experiments thus far described were performed at the University of Illinois in 1898. While the present author directed and took part in them, they were personally conducted by Mr. J. M. Fisher, to whose ingenuity and enthusiasm much credit is due. The experiments which follow were performed later at the Harvard Psychological Laboratory. I am indebted for many suggestions to Professor Münsterberg, through whose courtesy the resources of the laboratory were placed at my disposal, causing the successful continuation of the work, and for the able and patient assistance received from the many subjects who took part in the work.

known, while the mean variation showed an increase of 46 σ.[1] A valuable experiment similar to this would be to have the subject's attention directed to several disparate sensations instead of all being of the same kind. The same author mentions one of this kind in which sensations of light, sound, and touch were employed. No numerical results are given, although a very noticeable lengthening of the reaction time is mentioned, and also a continual fluctuating of the attention between the different senses.[2] He does not mention these experiments in reference to the question before us, although their results are obviously in accord with those here described.

6. Tachistoscopic Experiments.

On the other hand, Wundt has contributed the most formidable experiment we have in support of the theory of division. Its general features resemble Hamilton's well-known experiment, in which the attention was turned to a handful of marbles thrown upon the floor and the attempt made to observe them all, although it has fewer technical objections. The apparatus used was named the *Tachistoskop* by Volkmann. Cattell made an improved pattern,[3] while a still more improved form is described by Zeitler.[4] The essential features of the apparatus consist of a shutter having a rectangular opening in the middle, and sliding up and down in the grooves of two parallel uprights. There is a card holder arranged behind the shutter, so that when it is raised the card, upon which are placed letters or figures, is hidden by the lower part of the shutter. When it is in this position, the subject's eyes are directed to a fixation mark on the shutter, and over the center of the card. After a given signal the shutter falls, and in doing so exposes the card through the middle, and then immediately covers it over with its upper part. The apparatus thus serves to expose the letters on the card from 76.2 σ to 93.7 σ, those at the bottom being exposed the shortest time on account of the acceleration of the falling

[1] From figures on page 351 of the 'Physiologische Psychologie,' Bd. II., 4th edition.

[2] *Ibid.*, S. 352.

[3] *Phil. Stud.*, III. (1886), S. 94.

[4] *Phil. Stud.*, XVI. (1900), S. 380.

shutter. The theory of the experiment is that the number of letters which the subject can give from the exposure indicates the number of ideas that can be apperceived or attended to simultaneously.[1]

Consciousness is believed by Wundt to have wider limits than apperception. To test this limit, a specially arranged metronome gives a series of single beats, and it is found how many of these can be correctly judged as equal to another series which immediately follows it. As these series increase in length, a point is reached at which the subject can no longer judge correctly of their relative lengths. Sixteen single, or eight double strokes form this limit. Hence, sixteen is the number of separate impressions that can be held in consciousness at once.[2]

The number of separate objects that can be apperceived at once with the tachistoscope is given as varying from four to five. Six is usually considered the extreme limit, although this may be greatly increased if letters are used and they are arranged in intelligible syllables. Unfortunately we are without an explanation of the limit of this multiple activity of the attention, so that little satisfaction could be given one who should ask why ten or a dozen objects should not be simultaneously attended to as well as four or five.

We have the question as to whether the attention is really divided under the conditions of Wundt's experiment, or if the results from it are susceptible of a different interpretation. It came to be my purpose, therefore, to produce variations of the experiment in order to ascertain the real nature of the processes involved.

When one performs the experiment described by Wundt, a slight hesitation is often noticeable before the number of letters or simple objects seen can be named. This suggests that the objects are not clearly perceived during the time of exposure as Wundt claims to be the case. To test this, a tachistoscope was made embodying the essential features of the different forms of Wundt's apparatus, but avoiding the acceleration of the shutter

[1] 'Physiologische Psychologie,' B. II., 286 ff.
[2] *Ibid.*, S. 286 ff.

which would cause some letters to be exposed longer than others. Its front consisted of a black hard-wood screen, 20 by 40 cm. and 0.6 cm. in thickness, so supported upon a base that when placed upon a table it was perpendicular to the sagittal axis of the subject seated before it. In its center was an opening 6.3 cm. square, with bevelled edges to avoid shadows. Behind this an oblong shutter of similar material, with a bevelled square opening in the middle matching the first, was made to slide horizontally instead of vertically, as with Wundt's apparatus. A heavy rubber band attached between the forward end of the shutter and a small windlass at the outer edge of the screen furnished a means of propelling the slide past the screen opening, and of varying its rate in proportion to the tension of the band. A card-holder was arranged behind the shutter so that objects on the card were exposed as the shutter passed. A lever released the shutter, and a spring and rubber cushion stopped it. The rate of movement was ascertained for all parts of the passage by means of a smoked paper attached to the shutter and which passed by a vibrating tuning-fork. This showed that for about the first and last inch of the movement there was an acceleration and retardation respectively; but that for the middle part of the movement, which was used for the exposure, the rate was constant. Fig. 1 represents the back of this apparatus.[1] Wilson's black gummed letters, 7 mm. in height, were used upon white cards as objects to be exposed.

7. Letters Exposed in Succession.

If the letters are exposed in succession, one at a time, but appear to be exposed simultaneously, evidently they are not perceived until all have been exposed, *i. e.*, the act of perception would not take place during the exposure, but after. To determine this, the opening in the shutter was, by means of the insertion of a blackened metal plate, closed to a narrow slit 7 mm. wide and extending vertically the width of the opening. Twenty cards were used and six letters were arranged irregularly upon each card, but so that as the opening in the shutter passed in front, only one letter was shown at a time. Thus while the let-

[1] The figure will appear in Part II. of this article.

ters were placed in all parts of the square surface of the card exposed, they were exposed in a definite order of succession. The time of the whole exposure was 20 σ, or approximately 3.6 σ for each letter. It should be noted that, owing to the method of exposure, first the first part, then the whole, and finally the last part of each letter was uncovered. The time when the letters as a whole appeared was extremely short, as the width of the opening was but a little greater than that of the letters.

A practice series of considerable duration preceded the regular experiment, although the effect of practice was not very marked. The method of procedure was as follows: The subject was placed at a constant distance from the apparatus, and kept in position by a head-rest. This distance was 1 m. for all but one subject (*R*), who required to be but 60 cm. in order to get a distinct impression. The eyes were fixed on a fixation mark placed on the shutter and in front of the center of the area to be exposed, and the attention was distributed as far as possible over this area. The experimenter gave a double signal, one two, and one one second before releasing the shutter. The subject gave the letters which he saw in the order in which he saw them, and also their locations in the field in order to keep account of those wrongly seen. In order to guard against fatigue, a short recess was given after each ten exposures, and only thirty exposures were given in an hour, as more than that was found to be fatiguing. Each of the twenty cards used was exposed five times during the series, thus making a hundred exposures for each subject. A record was kept of the letters seen correctly, their order, those seen wrongly, and those misplaced. When this series had been completed, another was taken, in all points similar, except that the shutter was made to move from left to right during the exposure instead of from right to left as in the first series. This served to reverse the order in which the letters were exposed. No succession in the exposure of the letters was perceptible by the subjects. It might be, however, that the order of exposure unconsciously influenced the order in which the letters were perceived, and this would show that perception took place, partly at least, during the exposure. It was to test this that the changed order of exposure was tried.

Table IV. gives the numerical results of the two series, showing the number of letters seen correctly, those seen wrongly, and those misplaced in the hundred exposures for each subject in each series. Of those misplaced in the consecutive exposure,

TABLE IV.

Subject.	*A.*	*H.*	*R*	*S.*	*Y.*	Av.
Consecutive Exposure.						
Number seen correctly.	212	222	158	160	224	195
Number seen wrongly.	40	30	58	50	58	47
Number misplaced.	17	16	5	18	23	16
Reversed consecutive.						
Number seen correctly.	208	225	167	151	229	196
Number seen wrongly.	32	22	64	53	52	45
Number misplaced.	6	13	7	10	9	9

75 per cent. were otherwise correctly seen, while in the reversed consecutive this number amounted to 69 per cent. The average number wrongly seen is about equal in both series, and also the number seen correctly. These features of close correspondence show that the conditions in both series were practically equally favorable for seeing the letters. If we divide these numbers by 100, we shall get the figures for a single average exposure. This would make the average number seen correctly slightly less than two, the smallness of which we shall see later is readily accounted for by the short exposure. Ten subjects took part in the first series, but since only five were employed in the second, and the results were uniform, only five are given in the table. All of the subjects were men, the most of whom had had considerable laboratory training.

It is, however, in the order in which the letters appeared and the introspective records that we find the most significant features of the experiment. Some features of the mental process would be more distinct with one subject than with another, and the experience would vary somewhat for different exposures, but a general harmony prevailed throughout. Uniformly at the time of exposure the card would seem to flash out without one side appearing before the other. Hence perception evidently did not take place until after the exposure was over. The first effect of the letters was that of a single complex impression, some characters appearing distinctly outlined, some confused, and some entirely unseen. This conscious impression followed the exposure in much the same way that a positive after-image follows a stimulation of light. It was sometimes possible to hold this impression with all its details an appreciable length of time without recognizing a single letter, until each character was recognized one at a time. But it was more frequent that one or sometimes two letters were recognized without being preceded by an appreciable interval, and these followed by one or two more, one at a time and in distinct succession.

The letters that came up last were nearly always less distinct, although it was sometimes the case that the order was not the order of distinctness. A special effort to recognize an indistinct character would frequently cause it to mature into complete recognition before others which were at first more distinct. Sometimes a delay in this maturing process would cause more distinct characters to be forgotten before they could be named, or else cause the indistinct one to come floating into the mind as an after-thought when all had been given that could at first be remembered. This indistinctness of the letters recalled last is a comment upon the common experience that the impression as it is first received rapidly fades. One grasps at the most distinct characters in order to secure them before they fade, but with the feeling that in doing so he excludes the possibility of catching others which he might have taken in their place. The naming of the letters makes them seem more sure, and this is hurried up in order to get as many as possible. One subject noted that the delay caused by locating each letter in the field as it was recognized caused fewer to be got than when they were all named at first and then located.

The order in which the letters matured seem to be in no way dependent upon the order of exposure. In general, those in the middle of the field of vision, and hence seen most distinctly, were given first. It was frequently noted that some factor, other than the distance from the center of the field and the occasional voluntary effort, influenced the order in which the letters were perceived. By referring the letters as given to the cards, it was found that the prevailing order was from left to right, as in reading; and this was as true of the reversed order of exposure as of the other. The habit derived from reading thus seems to have influenced the order to some extent. But no distinct influence seems to have been exerted by the reversing of the shutter.

Frequently the same letters would be given from a card for several exposures, and these would have their order varied independently of the direction in which the shutter moved. The letters in the center had the preference, while those sometimes upon one side and sometimes upon another would be given with these, a fact which would seem to indicate that the attention before the exposure wandered about the fixation point rather than distributed itself equally over the field, as was also the case in the reaction experiment with concentrated and attempted distribution of attention. This probably explains another experience which might be interpreted in favor of a distributed attention and which was common to the subjects. When several letters were seen they were less distinct than when only one was got. This is in line with the law above quoted from Hamilton and illustrated by Wundt,[1] that 'the greater the number of objects to which our consciousness is simultaneously extended, the smaller is the intensity with which it is able to consider each.' A comparatively large number of letters was commonly got as the result of a special effort of attention. Supposing this effort to cause the attention to fluctuate more rapidly about the field, a noticeable result of a special effort at distribution, and this to be correlated physiologically with a correspondingly intermittent rapid central stimulation of the visual centers, we can see that these centers would, as a whole, be kept in a more responsive state than when less effort was made, and the fluctuations were slower. This would cause the getting of a larger number with the greater effort. We can also see that with the less effort and slower fluctuation the center stimulated at the instant of exposure would reach a more highly excited state because the fluctuation was slower and the

[1] 'Physiologische Psychologie,' II., S. 268.

time of stimulation of the center longer. Hence the greater vividness of the single letter and the less vividness of the several. Thus this experience may be explained without the aid of distribution.

The fact that the letters in this experiment were not perceived or made conscious until after the exposure was over, separates the time after the instant of exposure into the two natural divisions, one a period of inertia, or subconscious period, and the other the conscious period.

8. The Subconscious Period.

When the eye is stimulated we have, first, a so-called latent period of variable duration during which no effect of stimulation is shown. This is founded primarily upon analogy with the general functioning of the nervous system, since electric stimulations applied to a nerve do not cause an immediate muscular contraction. This period is very short. Second, there is a very brief but relatively longer period during which the effects of stimulation reach a maximum. This is illustrated by 'recurrent images,' or the 'oscillatory' activity of the retina. It is shown when a black disk, illuminated brightly by sunlight, and containing a white sector, is rotated at the rate of about one revolution in two seconds. With the eyes fixated upon the center of rotation, the sector seems to have a shadow upon it a short distance behind the advancing border, and this may be followed by a second fainter one, and even by a third still fainter. The distance between these, and between the first and the forward edge corresponds to a time period of about .015 of a sec. "It thus appears that when light is suddenly thrown upon the retina, the sensation does not at once rise to its maximum, but reaches this point by a sort of vibratory movement."[1]

In addition to this inertia of the retina there is also inertia of a more central origin. By means of experiments in reaction time, Cattell found 119σ and 116σ as the time necessary for two subjects to distinguish one capital letter from all the others when the letters were the size of the capital of an ordinarily printed

[1] Bowditch, in 'American Text-book of Physiology,' p. 790.

page. With the same subjects he found the whole reaction time which included this process to be 308σ and 324σ respectively. It took longer for some letters than for others to the extent of some 20σ.[1]

This period of inertia preceded the completed act of perception in our experiment, and hence errors in the process are traceable to it. Thus it was a common occurrence for one letter to be taken for another which largely resembled it. *C* and *G* were often confused, also *V* and *Y*, *O* and *U*; here, no doubt, the part of one letter only was seen which resembled a part of the other, and the rest was filled out wrongly as in an illusion. More striking were the cases in which letters placed far apart on the card would be given a place half way between with the position seemingly sure, but with uncertainty as to which of the two letters it was. An *F* might be placed far from its real position and beside a *C* without the latter letter having been seen. A *Z* standing by itself was given as *Z* and *T* close beside each other. *G* was interpreted as *G* and *C* beside each other and in the place of an *I* which was not seen. Letters are made up of different parts or elements of form in the same way that words are made up of letters. Such cases as the above indicate that elements of form and position were received as disconnected impressions at the instant of exposure, and that while these in the majority of cases were correlated subconsciously into the right letters in their right places, they were yet often combined into wrong letters and in wrong places. It was not, however, until this correlation was completed at the end of the exposure that the letters reached their first conscious stage, and it was possible for the attention to be divided among them. So that if distribution can take place in tachistoscopic experiments, it is only when the exposure is over, or when it is longer at least than the 20σ exposure of this experiment.

In this connection it is important to know how long a time can be taken up by the consecutive exposure of letters and still have them seem to appear simultaneously. It was found that

[1] 'The Time Taken up by Mental Operations,' *Mind*, XI. (1886), p. 220.

For discussions of inertia see Wundt, 'Phys. Psych.,' I., 321 ff.; Fechner, 'Psychophys.,' II., 431 ff.; Exner, 'Herman's Phys.,' II., 215; *Pflüger's Archiv*, XXVIII., 487; Hofbauer, *ibid.*, LXVIII., 546.

the rate of the shutter could not be varied sufficiently for this purpose with the rubber band, so that a weight attached to a cord running over a pulley and attaching to the shutter was substituted for it. The slowest rate which could be attained by this means was produced by allowing the shutter to be drawn by the falling of the weight when it started from a position of rest. More rapid rates were produced by raising the weight to various heights before releasing it, so that a degree of velocity was attained before it reached the point in its fall at which the shutter was moved. The extremes of slow and rapid rate procured in this way were much greater than were needed by the experiment. The height from which the weight fell was recorded, and the exposure varied several times from both the too-rapid and the too-slow rates to the point at which succession was just indiscernible. When this point had been determined for each subject the time of the exposure was ascertained by means of the tuning-fork and smoked paper method previously described. The following table gives the time of exposure in thousandths

TABLE V.

Subject.	*Am.*	*An.*	*Ho.*	*Hu.*	*M.*	*R.*	*S.*	*Y.*	Av.
Time.	26	24	86	34	75	75	27	28	47

of seconds in which the sequence was just indiscernible for each of eight subjects. The average rate thus obtained for the whole exposure was 47 σ. This average is of little value because of the wide differences of rate found for the different subjects. In this test the shutter moved from right to left. When the exposure was too short for succession to be noticed, the order in which the letters came to consciousness was from left to right, as in reading. With all of the subjects except *Am* and *Hu* it was noticed that the first sign that the objective order had become apparent was the tendency to give the letters from right to left in the order of their exposure. With *Am* and *Hu* the movement of the shutter was first perceived as such.

What causes this lack in the perception of succession? At first we might think of it in connection with the after-effect of retinal stimulation which makes color mixing possible with the color wheel. The duration of this varies from .100 to .033 sec. according to the intensity of illumination, the length of the stimulation and the color. The difficulty with this solution consists in the fact that while in color mixing the same part of the retina is affected by the different colors, in the above test, different parts were affected in succession as the exposure took place. The divergence of these parts was about 0.97 mm. when one of 0.004 to 0.006 mm. is sufficient to distinguish two adjacent objects. In this experiment the breadth of the exposed field was 6.3 cm. and the distance of the eyes of the subject from the apparatus 1 meter, with the exception of *M* and *R*, for whom it was 60 cm. Even though the after-effect of the part of the retina affected first should continue until after the stimulation of the part affected last took place, this, it would seem, need not prevent the initial impulses from being felt separately. This suggests that somewhere on the route of transmission the visual impulses from different parts of the retina traverse paths held sufficiently common to cause the impulses received first to overcome inertia, thus allowing the later impulses to overtake the first and so reach the center of consciousness nearly or quite simultaneously with them. On the other hand, it may be that the stimulations which come in succession from different parts of the visual field are transmitted immediately to the cortex. A certain time interval between the central nervous impulses thus aroused might be necessary in order for them to remain sufficiently distinct to mediate discrete sensations.

(*To be concluded.*)