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(*Editor of the Psychological Monographs*)

The Johns Hopkins Studies in Philosophy and Psychology

No. 2

Studies from the Johns Hopkins Psychological
Laboratory

Communicated by Professor George M. Stratton

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Studies from the Johns Hopkins Psychological Laboratory.

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The MSS. of these studies were revised November 8, 1908.



THE LOCALIZATION OF SOUNDS.

BY DR. KNIGHT DUNLAP.

Several years ago I commenced the attempt to make comparisons between the location of sounds with both ears and the location with one ear, the other being stopped up as well as might be. The results of my first tests were rather odd, showing a condition which made it impossible to get at the comparison I wished, at least in any clear way; and subsequent tests which I have made from time to time and which students have made for me, on different subjects, have resulted in the same way. The condition mentioned has had so little (if any) consideration in connection with the problem of the location of sounds, that I have thought it important to give some account of my experiments. For the purpose of illustration some data obtained in the last year are sufficient, since it is strictly typical of the mass I have earlier obtained.

My method of experimentation requires but a few words of explanation. I used first the Titchener sound cage, and later the Pillsbury cage. The telephone receiver of the Titchener cage was excited by an induction current produced by the usual method of induction coil, storage battery, and 100 d. v. fork. In other cases it was excited by making and breaking a storage battery current connected directly. The Pillsbury machine was used first with the buzzer with which it is provided, and subsequently with a Galton whistle. The data presented below were obtained with the Pillsbury cage and buzzer.

Both cages have the stimulus radius too short, so that it is not practicable to experiment much below one hundred and twenty degrees from the vertex. The Pillsbury cage came with scale reading in one hundred divisions of the circle, and had to be rescaled. Both machines are provided with clamps for holding the subject's head. These clamps, being attached to

the frame carrying the stimulus-arm, must not be used, for in even light contact with the head they conduct sound appreciably. In the experiments described here, the position of the subject's head was constantly watched to see that it was kept properly.

Stimulations were given from forty-eight points chosen on the sphere described by the stimulus-arm, in accordance with the diagram of Fig. 1. The subject shut his eyes at a warning signal before the stimulus-arm was swung into position, and opened them at a second signal, after the arm had been swung out of position. The positions were chosen haphazard by the experimenter, and checked off as given on a chart like Fig. 1. After opening his eyes the subject indicated the position of the sound as it seemed to him, and the experimenter marked that down on his chart. In some experiments the subject pointed with his forefinger to the apparent position of the sound, and the experimenter swung the stimulus-arm to the position to which the subject pointed, reading the same from the scale of the instrument. It was found, however, that in many cases the experimenter could not tell where the subject was trying to point, and in others the subject did not point where he thought he did. For this reason, in most of the experiments (including the ones from which the data below are drawn), a small chart was supported in front of the subject, on a rod rising from the floor, and the subject (having thoroughly oriented himself in the chart), indicated the position thereon. He was, however, asked to point the direction first, and the experimenter called his attention to any discrepancy in the two judgments, not suggesting correction but allowing the subject to correct in whichever way he saw fit. A high degree of accuracy was obtained in this way.

A complete series of forty-eight judgments was all that could be obtained at one sitting without wearying the subject. On different days series were taken, now with both ears, and now with the right ear or with the left ear. When one ear was used the other was stopped by the method which I have found most effective: a plug of vaselined cotton was pushed into the meatus of the ear, not tightly, the outer end of the plug well inside; then the outer end of the meatus and the concha were

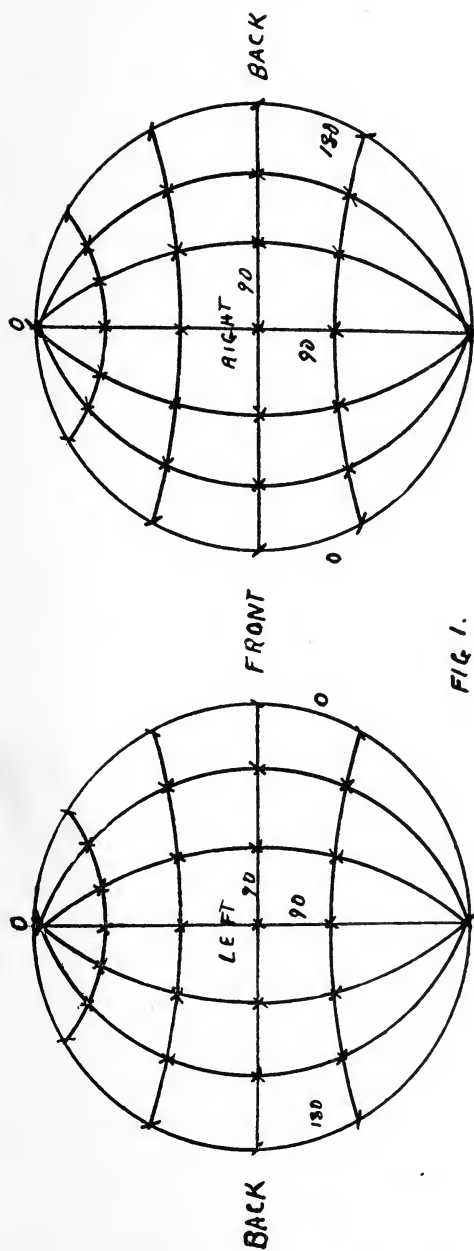


FIG. 1.

filled with thick paste. This deadened the ear more than could be done by any other method, with absolutely no discomfort, and was easily removed by washing out the paste with tepid water, and removing the protecting plug. Of course the ear so stopped still hears, and will hear to some extent (by conduction through the skull), as long as it is uninjured; but the sound employed should be so weak that the effect on the plugged ear is very slight indeed.

Figure 2 shows where the forty-eight stimulations were located by one subject with both ears. Figs. 3 and 4 show the locations by the same subject with the right and left ears respectively. Figure 8 shows how the same subject located with both ears three months later. Figures 5, 6 and 7 show how another subject located with both ears, and with the right ear and the left ear respectively. The conditions were exactly the same for all these cases, except the stopping of the ears as indicated. The first three series on subject S. (Figs. 2, 3 and 4,) and the three on subject D. (Figs. 5, 6 and 7), were obtained in January; the fourth series on subject S. (Fig. 8) was obtained the last of April. The peculiarity of all the results shows here at once. Each subject has at a given time a preferred position, to the neighborhood of which the sounds are very generally referred. The general tendency to increased concentration when only one ear is used is also clearly shown.

It is important to notice the location of the sounds from each part of the sphere. It might be supposed that the upper locations in the area of locations represent the sounds given in the upper part of the sphere; that the posterior locations of the group are of the sounds given in the back of the sphere, etc. The figures give no information on this point, as the attempt to introduce it there would make them into puzzles. The experimenter, however, kept a record of the location of each individual sound, and the locations for each meridian of longitude and parallel of latitude are given in tables I to VI.

In these tables the sounds given at the points in the median plane, that is, on the great circle dividing the two hemispheres, are not included¹, as they can not be considered in either hemi-

¹Except insofar as they are given in Table VIII.

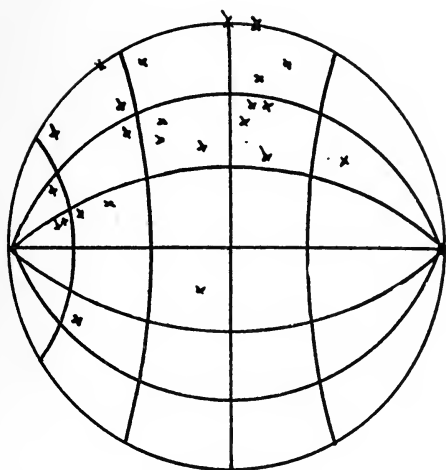
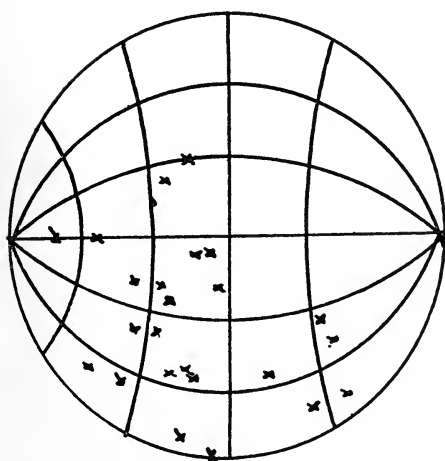


FIG 2



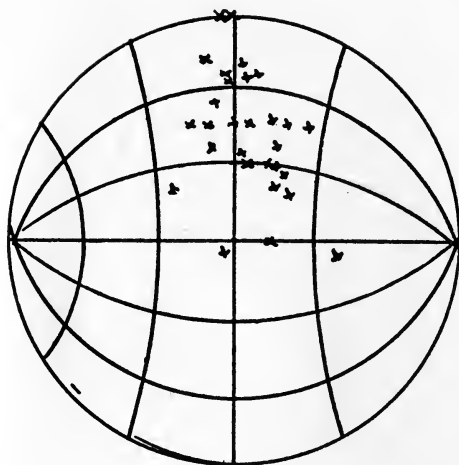
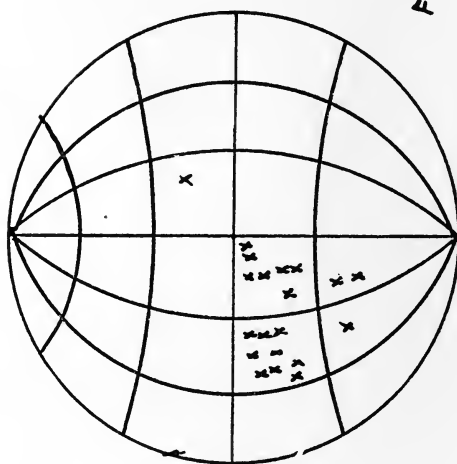


FIG. 9



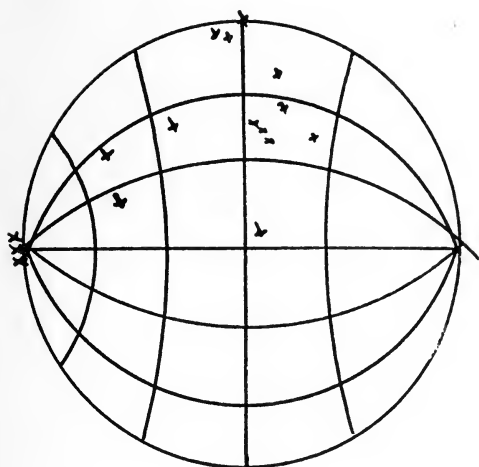
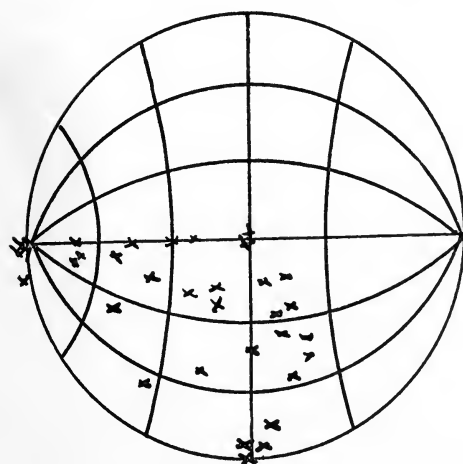


FIG. 4



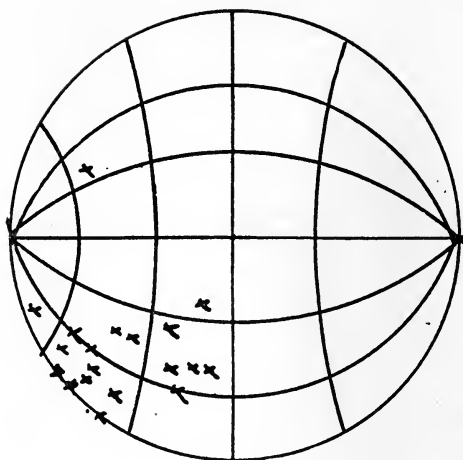
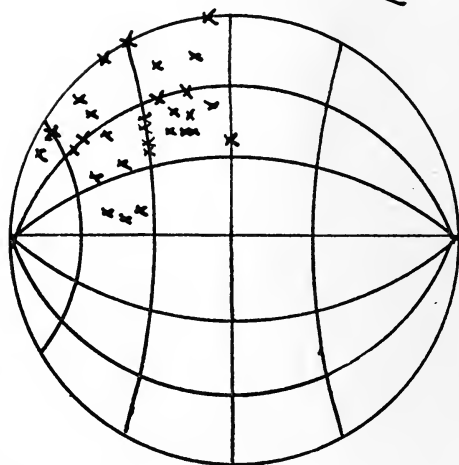


FIG. 5



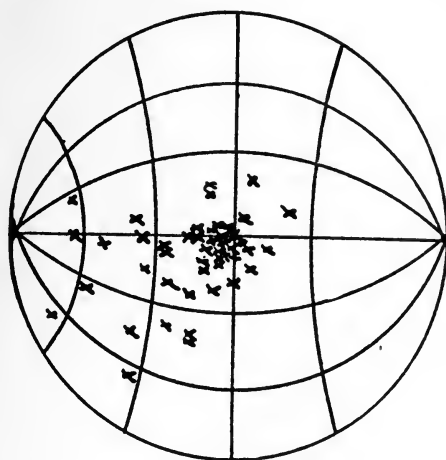
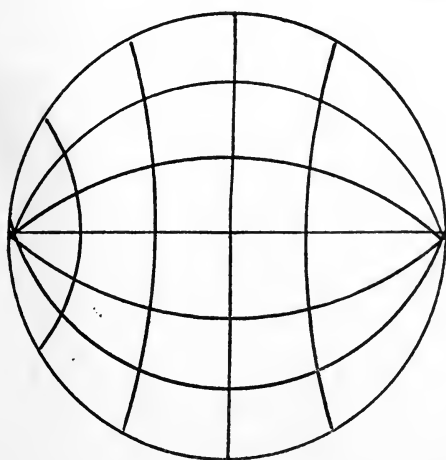


FIG. 6



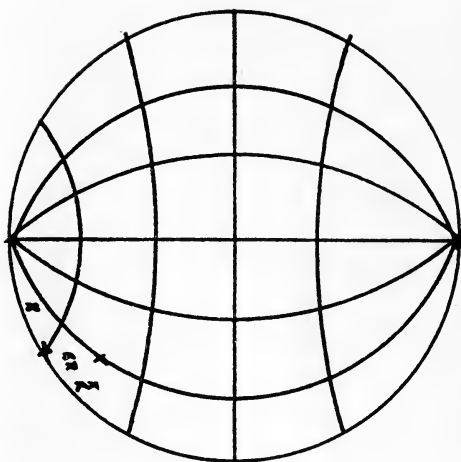
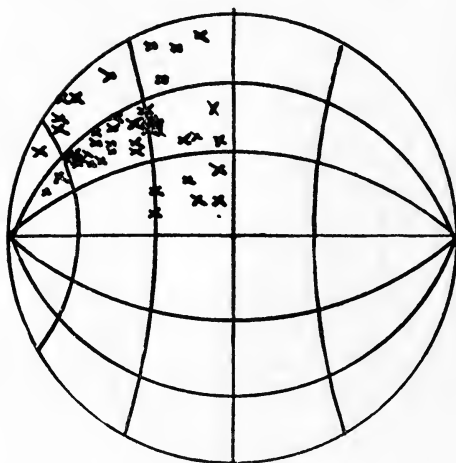


Fig 7.



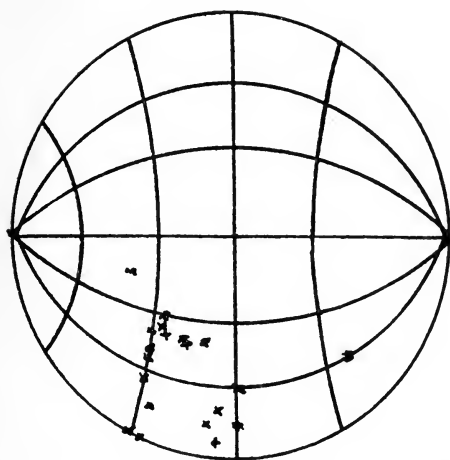


FIG. 8

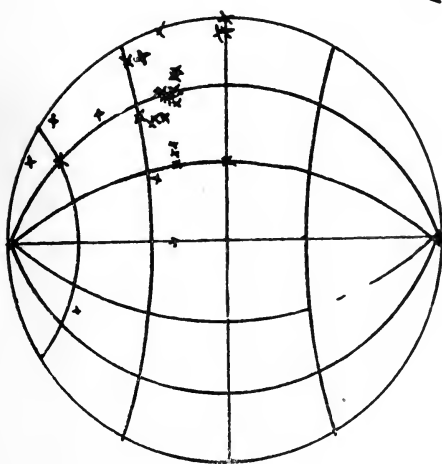


TABLE I.

Longitudes of Locations. Subject S.

Ears	Hem.	A. By Long. of Origin.					B. By Lat. of Origin.			
		30	60	90	120	150	30	60	90	120
Both	Rt.	141	155	132	136	141	138	124	154	149
	Lft.	132	131	140	138	122	138	125	125	142
Rt.	Rt.	127	143	116	135	142	145	145	124	118
	Lft.	102	143	128	108	115	127	109	122	121
Lft.	Rt.	138	141	128	103	110	151	120	126	100
	Lft.	110	137	117	125	121	133	102	124	126

TABLE II.

Longitudes of Locations. Subject D.

Ears	Hem.	A. By Long. of Origin.					B. By Lat. of Origin.			
		30	60	90	120	150	30	60	90	120
Both	Rt.	43	52	28	3	46	24	40	37	37
	Lft.	45	51	46	21	28	28	47	33	46
Rt.	Rt.	85	77	90	92	86	91	89	58	95
	Lft.	92	67	97	70	75	91	87	66	78
Lft.	Rt.	21	13	31	46	52	46	16	25	45
	Lft.	45	51	55	11	45	31	57	39	39

TABLE III.

Longitudes of Locations. Subject S.

Ears	Hem.	A. By Long. of Origin.					B. By Lat. of Origin.			
		30	60	90	120	150	30	60	90	120
Both	Rt.	37	42	50	17	17	24	27	46	37
	Lft.	18	41	58	66	36	26	49	63	39

TABLE IV.

Latitudes of Locations. Subject S.

Ears	Hem.	A. By Lat. of Origin.				B. By Long. of Origin.				
		30	60	90	120	30	60	90	120	150
Both	Rt.	36	50	99	77	93	65	48	65	55
	Lft.	68	63	122	97	73	102	102	90	68
Rt.	Rt.	84	96	101	106	105	83	97	95	102
	Lft.	109	95	99	103	100	105	96	102	103
Lft.	Rt.	68	58	78	51	92	50	66	26	68
	Lft.	40	59	99	89	87	70	47	61	92

TABLE V.

Latitudes of Locations. Subject D.

Ears	Hem.	A. By Lat. of Origin.				B. By Long. of Origin.				
		30	60	90	120	30	60	90	120	150
Both	Rt.	36	38	58	47	57	41	50	42	28
	Lft.	55	56	75	60	55	65	72	55	60
Rt.	Rt.	84	83	65	89	83	70	83	87	86
	Lft.	70	61	60	59	58	48	52	72	85
Lft.	Rt.	50	44	38	48	45	42	32	47	57
	Lft.	36	57	63	54	70	52	70	37	50

TABLE VI.

Latitudes of Locations. Subject S.

Ears	Hem.	A. By Lat. of Origin.				B. By Long. of Origin.				
		30	60	90	120	30	60	90	120	150
Both	Rt.	68	68	72	86	75	71	66	80	75
	Lft.	68	69	65	67	68	70	77	47	72

sphere. But their location differs in no way from that of the other positions, following the same rule. With this exception, the average latitudes and longitudes of location are given for the points on each parallel and meridian, so from the tables we may see the effects severally of the actual latitude and longitude of the sounds on the latitude and longitude of their location. The averages are strictly by position of origin, *i.e.*, by the positions where the sound was given, locations on the opposite hemisphere being taken at their measurements in that hemisphere. The fractions are dropped throughout the tables.

Table VII gives the average of all the locations of the sounds, first with reference to the hemisphere of origin, and second with reference to the hemisphere of location only. These averages were not taken from the figures of Tables I to VI, but from the same data from which those averages are drawn. Table VIII shows the number of sounds given in the median plane located on the right and left sides respectively, and the numbers given in the left hemisphere and located in the right and *vice versa*.

TABLE VII.

Averages of all Locations.

Subject	(Tables)	Ears.	By Hems.: A. Of Origin.				B. Of Location.	
			Hem.	Long.	a. v.	Lat.	a. v.	Long. Lat.
S.	(I, IV)	Both	Rt.	141	20	65	28	132 68
			Lft.	133	21	87	28	137 84
		Rt.	Rt.	133	20	97	9	133 95
			Lft.	119	16	102	6	115 104
		Lft.	Rt.	125	24	64	30	136 71
			Lft.	121	20	71	29	118 66
D.	(II, V)	Both	Rt.	34	24	44	10	32 45
			Lft.	38	15	61	13	39 61
		Rt.	Rt.	85	10	81	10	83 71
			Lft.	81	16	62	18
		Lft.	Rt.	33	19	45	10	12 35
			Lft.	41	17	55	12	41 52
S.	(III, VI)	Both	Rt.	33	14	73	9	33 73
			Lft.	44	20	67	8	44 67

If all the sounds given in each hemisphere were located accurately where given, the average locus for each hemisphere would be Longitude 90 and Latitude 75, with average variations of 45 and 30 respectively. Dividing these variations by the average variations in Table VII gives the indices of concentration for the several cases.

TABLE VIII.

Locations from Median Plane and From Opposite Sides.

Subject.	(Tables)	Ears.	Med. to Rt.	Med. to Lft.	Lft. to Rt.	Rt. to Lft.
S.	(I, IV)	Both	4	4	0	1
		Rt.	5	3	7	3
		Lft.	1	6	0	8
D.	(II, V)	Both	2	6	0	4
		Rt.	8	0	20	0
		Lft.	1	7	0	15
S.	(III, VI)	Both	0	8	0	0

As a matter of fact, the position in the area of location bears precious little relation to the actual position of the sound. The marks representing the sounds at the various points might to all intents and purposes be shaken in a box and dumped down in the preferred area on the chart. This appearance is amply confirmed by other series on the subjects. Repeated series give results which have no uniformity except in the general area of location.

The preferred position is not determined by the character of the sound, or by the environment. Two subjects in exactly the same circumstances may have quite different preferences (Fig. 2 and Fig. 5). A subject may show the same preference after six months or a year, or may show a decidedly different one without any known reason for the change (as Subject S., Fig. 2 and Fig. 7). The subject shows the same preference in different rooms, or if he is reversed in the same room. Alterations in the intensity of the sound produce no definite alteration in the preferred area. The Galton whistle gives practically the same results as the buzzer or telephone receiver. Of

course, in any test the sound was kept as constant as was possible.

So far I have not found a subject who does not localize in this preferential way. What the causes are I can not say. There are possible theories, but nothing more. Meantime, how to conduct profitable experiments in localization before solving this problem is another problem.

CONCERNING FLUCTUATING AND INAUDIBLE SOUNDS.

BY DR. KNIGHT DUNLAP.

In a previous piece of work (Psych. Rev., XI, 1904, pp. 308-318), I found that interruptions in a faint sound could be correctly reported by an observer who at the time failed to perceive the sound itself. I found this phenomenon occurring both when the interruptions were made during the 'silent' intervals of a minimal sound which was undergoing the alternation of appearance and disappearance sometimes designated 'fluctuations of attention,' and also when interruptions were made in a sound which was at no time above the observer's threshold. These results were obtained by means of a telephone receiver actuated by the secondary current of a DuBois-Reymond induction coil, the primary current being furnished by a gravity battery and interrupted by a fork of 100 vibrations per second. The cessations were produced by breaking the secondary circuit, the primary circuit remaining undisturbed. Recently suspicions have been raised against these observations through consideration of certain peculiarities in the behavior of the diaphragm of the telephone receiver, and these suspicions merit attention and removal.

When the diaphragm of a telephone receiver is set in vibration by an electric current induced as above described, it oscillates *through* an average position which may be nearer the magnet than is the position of rest, or which may be farther from the magnet than is the resting position.

The change from the position of rest to the *average* position, when it occurs, may be readily observed. One convenient method, applicable when the change is produced by a moderate current, is to notice the approach or separation of two lines mirrored on the surface of the diaphragm, after the familiar

ophthalmoscopic method. Such observations show, and theory also requires, that the average position is nearer the magnet than is the position of rest, when the terminals of the magnet winding are so connected with the induction coil that the major phase of the current (namely, that caused by the *break* of the primary current), strengthens the magnetic field, and the lesser phase (caused by the *make* of the primary current), weakens the field. If the connections are reversed, so that the stronger current weakens the field, and the weaker current strengthens the field, we would expect to find, and do find clearly, that the average position of the vibrating diaphragm is farther from the magnet than is the position of rest, *unless* the current is very strong, in which case conditions are set up which we need not consider, since the statements just made apply to intensities up to those giving loud noisy sounds, whereas in the psychological experiments in question we use only feeble intensities.

By the introduction of a proper condenser in parallel with the fork (and a condenser ought always to be employed, if only in justice to the fork), this change of average position can be lessened, but probably can not be entirely obviated.

The conditions attendant on the use of the primary current for direct excitation of the receiver do not concern us, for that method of producing the sound is not advisable, and is not employed in experiments on minimal sounds.

The practical doubts raised by the foregoing observations are: (1) Does the release of the diaphragm from its forced vibrations through an average position different from the position of rest allow it to spring back to its normal position with a final movement of greater amplitude than the amplitude of its forced vibrations? and (2) Does the release and consequent springing back result in a movement of more rapid rate (and hence in a tone of different pitch) from that of the forced vibrations?

The first doubt can be readily dismissed. In fact it could arise only through a misconception of the mechanics of the diaphragm and magnet. Suppose for instance the average position of the diaphragm in movement is nearer the magnet than is the position of rest; if the current is discontinued while the diaphragm is in a part of its path nearer the magnet than

the resting position, it will move to the resting position against the normal field of the magnet, whereas under the conditions of the forced vibrations it moves from that same point towards the position of rest against only the weakened field of the magnet. Similarly, if the current is discontinued while the diaphragm is in a part of its path lying farther from the magnet than does the position of rest, it will move to the position of rest under the attraction of the normal magnetic field, whereas otherwise it would move to that position under the attraction of the augmented magnetic field. A corresponding condition is found when the average position is farther from the magnet than is the position of rest. When the current is discontinued, the diaphragm moves towards the position of rest either *against* a *stronger* magnetic pull or *with* a *weaker* pull than is the case while the current is supplied. There is therefore no final vibration of amplitude greater than those which have preceded. The apparent rise in intensity of the sound just as it is discontinued is psycho-physiological. This does not apply to conditions where direct current or heavy current is employed, as already stated.

The second doubt, namely, as to the possibility of a sound pitch or timbre different from that of the preceding sound, arising at the moment of discontinuance of the current, probably also arises from a misconception of the nature of the action of the induction current on the magnetic field of the telephone receiver. If we suppose the diaphragm to be released and spring back into place when the current is discontinued, it is easy to imagine it thereupon emitting its own note, which will usually differ from that previously emitted. Such an event may occur with the excitation of the magnet by the primary current, but not under the conditions we are considering, where the 'release' at the discontinuance of the current is less marked than that which occurs once in each complete phase of the exciting current. Here as before, we except from our consideration alternating currents whose opposing phase is strong enough not merely to weaken, but actually to *reverse* the polarity of the magnet.

The above arguments may not be convincing, and of course

it is possible that a more expert electrician will find that there are considerations which I have overlooked. I do not propose to rest my case here, but to show how the original experiments may be repeated and the results verified by means which are not open to the questions above discussed.

Since in many laboratories the only practicable means of sound transmission is by electrical transformation, it is worth while to consider methods of producing minimal tones from the telephone receiver. The necessity for our purposes is an alternating current of equal phases of uniform frequency and voltage. A small bell ringer would answer admirably, if driven by power under proper speed control, were it not for the fact that it has moving contacts, an arrangement absolutely fatal to the uniformity we require. This defect can be remedied however by having suitable windings put on the permanent magnets themselves, in which case the constancy of the current is governed solely by the constancy of the speed. Such a machine however, is useful only for low tones, since it is not safe to drive it at high speed. A special generator with permanent magnets and stationary windings giving higher frequency for low speed might be constructed without difficulty. The generator invented by Dr. Cahill for his telharmonium would be splendid if obtainable, since the wave form of its current approximates the cosine curve more nearly than that of any other generator.

A device much simpler than the troublesome generator, and one highly to be recommended, employs a pair of telephone receivers, one being used as receiver and the other as a generator. If the two are connected in circuit and one be brought near a source of sound, the sound is reproduced in the other distinctly, and with intensity which is not great at best, but is dependent on the proportions of the receivers. If as source of sound a singing gas flame be installed in one room, with one receiver near either end of the tube, and the other receiver be fastened near the observer's ear in another room into which the sound of the gas flame itself does not penetrate, we have a practical mechanism for the production and control of a steady tone. The constancy of the tone can be assured, after the flame has burned long enough to warm the tube, by maintaining the gas supply constant and excluding draughts.

The gas directly from the main *may* be used in certain places at certain hours of the day, provided an indicator is attached, and constantly watched to certify that no changes in pressure occur during the time the observations are made. Variations not visible in a water-level indicator will produce no consequential variations in the loudness of the tone when the flame is burning high. If the infinitesimal variation produced in the weak transmitted sound by minute variations in the loud original tone were noticed, the phenomenon would be more remarkable than any we are describing. The strongest confirmation comes from the fact that considerable observable variation in pressure produces no concomitant 'fluctuation' of the tone, even with a subject accustomed to note the 'fluctuation.' The necessary plan in most circumstances, and the safest in all, is to use an independent gasometer. 'Gas regulators' are of course worthless in work of this kind.

The intensity can be regulated in several ways. In the first place the distance from the receiver to the ear of the subject can be fixed to suit the needs of the case. In the second place, a metal disc can be adjusted above the flame-tube, so that the sound is damped to almost any degree. In the third place, a controllable induction apparatus may be placed in series or parallel with the two receivers. In the fourth place, the distance between the transmitter and the flame-tube may be changed.

If a tone purer than the 'noisy' complex tone of the gas flame is required, a resonator introduced in front of the transmitter serves the purpose admirably. The tone produced in the other telephone is now of striking roundness and purity. An adjustable resonator is preferable, for by mistuning it another simple means of intensity control is introduced. If the resonator is fixed in position, the transmitter may be moved away from it with the result that the intensity alone is modified, or practically so, since the flame may be so regulated, that the effects of the sound waves other than those transmitted through the resonator are negligible. If no resonator is used, any change in position of the transmitter modifies the timbre of the tone on account of the standing waves which the gas flame sets

up, even in a large room. Damping the sound by lowering a disc over the top of the tube always changes the timbre if no resonator is employed.

It is no doubt suggested by the description, and will be apparent on trial, that the manipulation of the apparatus, simple though it is, requires large patience and that there are a number of cautions to be observed. It is however possible to perform the manipulation successfully, and obtain intensities of sound which manifest the characteristic fluctuations to observers capable of noticing them. A much simpler arrangement is to attach a rubber tube to the resonator and convey the sound directly to the other rooms, provided the building is equipped with a sound pipe or you have no scruples about drilling holes in the wall through which to run your rubber tube. In this last case the intensity may be controlled by a screw-compress on the tube, or the tube may be cut and a cock inserted. A collecting funnel might be used on the end of the tube, in place of the resonator, if the simplicity of the tone is not an object. I have not tried that arrangement, however.

The gas flame with resonator and either method of transmission furnishes the best available means for demonstrating the fluctuations of musical tones, and the only simple means at present for performing the experiment with a *nearly* simple tone.¹ For the interruption experiments referred to at the beginning of this paper, either form may be used, with or without the resonator. I have used the resonator in both cases. In the case of electrical transmission the circuit may be broken, and this is of course the simplest method. The phases of the current being symmetrical the diaphragm of the second telephone vibrates symmetrically through the point of rest, and the discontinuance of the current can thinkably cause only a speedy cessation of vibration. However, there is another method which does not break the current abruptly, but reduces it

¹ Whistles similar to the Stern 'Tonvariators' might be used if the constancy of the note could be assured. I have not been able to make them give a constant note. If used, such whistles should be blown loudly by air at constant pressure, and the intensity of the note reduced in transmitting; for they are more steady when loudly blown.

rapidly, entirely (it seems to me), disarming criticism. This method is to bring a card laterally over the anterior opening of the resonator (not touching the resonator). This progressively lowers the pitch of the resonator, lessening its response therefore, and practically silencing it at the end of the movement. It goes without saying that in using this method the resonator should in the first place be tuned to, or a little below, the pitch of the gas flame note, and that the lowest strong partial of the flame must be used. If the upper partial is used the lowering of the pitch of the resonator may 'bring in' the lower note. But, really, with a proper resonator, the lower note will excite the resonator tuned down to it in this way so feebly that this caution may not be needed.

When using direct air transmission, the just-described method of discontinuing the sound may be employed, but is rather needless. The simpler method of turning the stop-cock is perfectly adequate. Another method is to have the tube cut in a room intervening between that containing the gas flame and that containing the observer, and the ends secured just far enough apart to permit a card to be passed between them without touching. In this case the intensity may be regulated from this room by the compression method (any screw clamp will serve). I have obtained good results by placing the resonator in a room separated by a wooden partition from the room containing the flame, running the tube from the resonator into a third room protected by a non-conducting brick and cement wall.

In any of these forms the perception of the cessation of an inaudible sound may be demonstrated as readily as the fluctuations in a just perceptible tone. In both cases considerable practice in careful observation is usually required (though this is by no means always the case) and some individuals are totally unable to notice the fluctuations in any tone, and others only in certain easy cases.¹

¹ For practice or other general work on attention, I would especially recommend the water-dropping device described in *Science*, XXVI (1907), 247-8, since it is exceedingly simple and convenient of manipulation, the intensity of the sound being regulated by placing the plate on which the drops fall at the

The failure to notice fluctuations in minimal tones is really due to inability to notice fine distinctions in the tones. The observer is simply unable to notice whether the tone is present or not, and hence adheres constantly to an arbitrary judgment. This sounds like a reckless assertion, but is supported by strong evidence. If an observer either hears the sound continuously, or fails to hear it at all, and no intensity can be found at which he notices the fluctuations, you can readily find intensities at which he will go on hearing the sound for long periods after it is absolutely cut off, provided you cut it off rather gradually. He really is incompetent to distinguish between the presence or absence of the sound of that particular intensity. This sort of blundering is characteristic of those who are apparently constitutionally unable to notice the fluctuations, and also of those who simply have not yet had sufficient practice. Moreover, almost any observer is liable to relapse into this condition at times. The nicely discriminating subject, on the other hand, who clearly notices the fluctuations, can not be caught that way. However carefully the suppression is made he very shortly reports it. He does not make his judgment instantly, unless the sound is cut off abruptly; and in fact the most careful subjects make no pretense to record, or even notice the exact time of the disappearance or reappearance of the sound when physically constant but 'fluctuating,' simply recording the judgment that at the indicated time the disappearance or reappearance had taken place; but he is distinctly not to be caught napping. So uniform are the results of these check experiments in the cases of those subjects whose accuracy I would be inclined to grant on other grounds, that I place no reliance on the results of experiments with those who do report fluctuations, and are yet flagrantly caught by the check experiment. Observers who fail to notice accurately the presence or absence of the weak sound need not be caught if they abandon the attempt and report no sound unless it is above this intensity, for there is

proper distance below the dropper. By using a rapid rate of drop, and a highly resonant object for it to strike on (e.g., a hollow metal box), a *practically* continuous sound may be secured, with which the interruption experiment may be performed, interposing a piece of thick felt to catch the drop when desired.

no check to show they hear when they judge they do not. This however would argue a prejudice on the part of the observer, or a lack of proper attitude towards the task, which fortunately I have not encountered. A large part of the difficulty is obviated, of course, if the observer does not know of the check experiments.

A NEW KEY FOR REACTION-TIME WORK.

BY DR. KNIGHT DUNLAP.

The imperfection of the ordinary telegraph form of the reaction key has long been recognized. As a result of this recognition we have had a number of modified forms of the key, such as Jastrow's, Bergstrom's magnetic, and Scripture's sliding key, which are designed to simplify the attention required of the reactor, and which in some measure succeed. Experimenters who have not found these keys satisfactory, or who have concluded *a priori* that they are not satisfactory, have been led to employ the release form of reaction, with which the telegraph form of key is *mechanically* satisfactory, or else to discard keys altogether, and employ other mechanisms, such as air bulbs.

The chief source of trouble in the ordinary key with the 'press'-reaction, is the unintentional breaking (or closing) of the circuit before the stimulus is given. When the measurements are made by the tuning-fork and drum method the circuits may be so arranged that such accidents do not come to the notice of the reactor, and hence no harm is done beyond the loss of time and reactions, which under these circumstances is large. But if the reactor becomes aware of these faults, as he must in the ordinary arrangements of apparatus, the disturbance is much more serious; for he inevitably endeavors to avoid them and so complicates his attention and lessens the facility of the reaction. This is especially true with the so-called sensory attention; many reactors have told me that they were unable to put the fingers on the key ready to react, and then completely withdraw the attention from finger and key, because the fingers are so apt to press a little too strongly or to be raised from the key. If the fingers do maintain their position it is through a cramping of the muscles which impedes the reaction. Others find little difficulty in this particular; and of course it tends to

disappear with practice, as the fingers become a more and more automatic mechanism, adjusting themselves to the key.

Since much experimentation must be done with relatively unpracticed reactors (for in certain work it is just the reactions of the relatively unpracticed person which are important); and since in any case the distraction due to the key may be important even if the reactor should not think it so; it is important to have a key requiring the least possible attention. None of the improved keys so far introduced reduce this attention factor sufficiently without the use of an initial resistance so high as to be a disturbing element in itself. The Jastrow and Bergstrom keys are in no wise better than an ordinary key with a spring

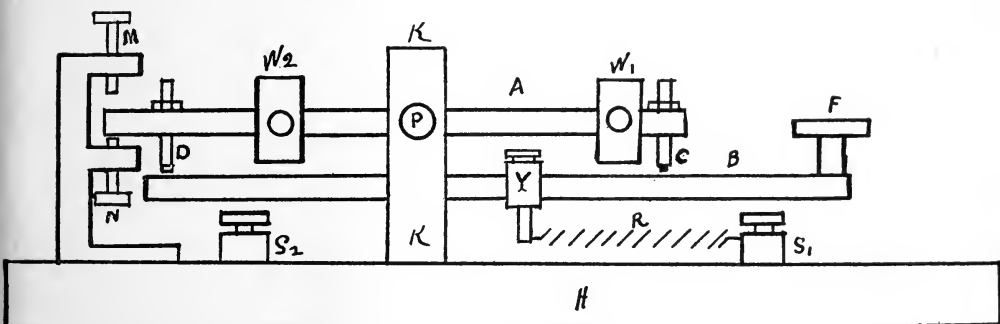


Fig. 1.

tension as strong as the initial resistance of one of these. The ideal mechanism would be the pneumatic bulb, were it not for the difficulties of long-distance air transmission. If the reactor could be in the same room with the recording apparatus, and if the kymographic method were always practicable, we would seek no farther.

In the attempt to construct an electric key having the advantages of the pneumatic system, I designed the key which is represented in Figures 1 and 2, and had it constructed by Mr. Childs, mechanic of the Physics Department of Johns Hopkins University. The instrument is there drawn to scale, the actual length of lever *A* being six inches. To allow for alterations if such should be needed, the proportions were purposely

made such that the instrument appears clumsy. A second key would be made much more compact.

On the three-ply wood base *H*, the rectangular steel frame *KK* is fastened, the lower side countersunk flush. The screws *P*, pass through the frame and are fastened by the lock-nuts *T*. The small rectangular frame *E-E-E* is tapped for screws *R* with lock-nuts *L*, the screws being hollowed at the tip for the cone points of the spindle *V*, and hollowed in the heads for the cone points of the screws *P*. The frame *E-E-E* and the spindle *V* therefore turn independently on adjustable cone bearings.

The spindle *V* carries the lever *A*, which is bored for it. The lever *B* is screwed to the frame *E-E-E*, but insulated from it by

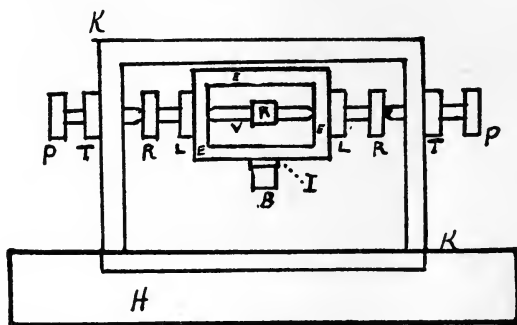


Fig. 2.

I. *F* is a hard rubber finger button, *M* and *N* are stops for the lever *A*, and *S* and *S* are stops for the lever *B*. The coil spring *R*, which holds up the *F* end of lever *B*, is adjustable by sliding the clamp *Y*. The set screw on *Y* is at the top, but will be changed to the side to be entirely out of the way. *W* and *W* are weights sliding on lever *A*, and clamped by set screws. *C* is a platinum-tipped screw, provided with lock-nut; and *D* is a screw, similar, but with an insulating hard rubber tip. *C* and *D* are interchangeable. The wires from the battery are led to levers *A* and *B* through binding posts and connections not shown in the cuts.

The weights are so adjusted that the upper lever exercises sufficient pressure on either *D* or *C* (whichever is desired) to follow the lower lever in slow movements, but has sufficient inertia to allow the lower lever to break away from it in a quick movement. The stops are so adjusted that when lever *B* is in extreme position up or down, contact is permanently made or broken (whichever is desired). Therefore, make or break by the beginning of a reaction-movement is maintained permanently as long as desired.

The key as represented is adjusted for break-circuit, 'press': (the adjustment of the contact and stops is not represented in the cut with absolute accuracy, of course). By sliding the weights so as to balance the lever in the opposite direction the key can be adjusted for make-circuit, 'release.' By leaving the weights as represented and interchanging pegs *C* and *D*, it can be adjusted for make-circuit, 'press.' By making both the changes mentioned, it may be adjusted for break-circuit, 'release.' Finally, by clamping the lever *A* fast between stops *M* and *N*, it becomes an ordinary, or telegraph form of key. This last possibility is important in making comparison between the new form of key and the old; for by keeping the same spring-tension nothing is changed except the factor under investigation.

The key works perfectly, except in the make-circuit, release. In this case there is a rebound, as there is with an ordinary telegraph key so used. This adjustment of the key has not been needed so far, but if it becomes necessary to use the key in this way (make-circuit, 'release'), a simple attachment, already planned, will perfect it. No loss of time between the beginning of the movement of the finger lever and the breaking of the contact is discoverable (by the spark method), with any fairly slow voluntary movement, when the weights are properly adjusted. It is almost impossible to make, when trying to do so, a single movement which shall give any interval. There is absolutely no loss therefore with the abrupt movements of reaction. Yet the unintentional movements (increase or decrease in pressure) of the reagent while attentively expecting the stimulus, occur without interrupting the contact. Such

interruptions as are made when the reagent is putting his fingers on the key, at the warning signal, make no difference, as the current should not be turned on at that time.

In practice, the key fulfills its intention. Accidental breaks of the circuit (in break-reactions) never occur, and the ease with which the subject can withdraw attention from the hand and key altogether is remarkable. No attempts have been made yet to find how low a reagent can bring the 'sensory' reaction with brief practice on that alone, but some tests have been made with both 'sensory' and 'motor' attention to compare the reaction with the new key with the reaction with the ordinary key (*i.e.*, with the lever *A* held firmly between the stops *M* and *N* as described above). Results with the two reactors with whom extended tests were made are given in Tables I to X.

These experiments were made with the Hipp Chronoscope, checked constantly with the tuning fork. The stimulus was the click of a telephone receiver held in the regular operator's headpiece. The room in which the reactor sat was constantly illumined faintly by a small gas flame, and the warning signal was the turning out of an electric light, enclosed in a box with a ground glass aperture which illumined the room strongly enough so that the reactor did not have to look at the ground glass. The light was turned on from three to five seconds, approximately, before it was turned out, giving the reactor plenty of time to put his hand on the key. When the signal light was out, after a reaction, the reactor was completely at rest; a condition which cannot be attained where the warning is a click or bell or flash, or the turning off of a light which is turned on again immediately after the reaction. In these latter cases the reactor is continually on the strain of expecting the warning signal or else is surprised by it in many cases, and hence the attention is not normal when the stimulus arrives. The warning signal (extinction of the major illumination) was given about two seconds before the stimulus. Exactness in this interval was of course purposely avoided. The interval finally seemed a little too long; one and three-fourths seconds would have been better; but with visual warning perhaps a little longer time is needed than when the warning is auditory.

The reactors were given the following instructions, in practically the following terms: "In the 'motor' series, put the fingers on the key while the light is on. Then as soon as the light goes off, attend strictly to the movement you are to make, with the intent of making it as quickly as possible when the proper time comes, dismissing the sound altogether, for it will do its work without your attention. In the 'sensory' series, place your fingers on the key while the light is on. Then as soon as the light goes off put your attention strictly on the sound you expect, with the intent of hearing it as quickly as possible, dismissing your hand and the movement altogether, for they will take care of themselves." All reactors are very soon able to carry out these instructions, although nearly all agree that the 'sensory' attention is the more easily attained.

Two series, one motor and one sensory, were taken on the same day. The reactor was taken out of his room for ten minutes or so, between the series, for exercise and diversion; yet there is no doubt that the first interfered with the second. This seemed to be a necessary evil, if both sensory and motor series were to be taken.

The results of the first series taken with the automatic key on two reactors are given in Tables I and II. In these and the following tables the first column gives the number of the pair of series (from the first pair), the second column gives the number of reactions in the 'sensory' series, the third column the average, the fourth the average variation, and the fifth the median. The sixth, seventh, eighth and ninth columns give the number of reactions, average, average variation, and median for the 'motor' series, and the tenth gives the order of the pair, e.g., 'M' signifies that the motor series came first.

Both of the reactors had had one period of practice in reacting, about a week before the first series recorded. At the beginning of each sensory and each motor series three reactions were taken without starting the chronoscope. No other practice was given during the progress of the experiment.

After the completion of the sets detailed in Tables I and II, sets were taken with the upper lever clamped as before described, the instrument being practically converted into a key of the

telegraph type. The results of these sets are given in Tables III and IV, under the caption of 'Plain Key.' With reactor B a third set was taken after the completion of the second, the key being 'automatic,' as in the first set. Results of this are given in Table V.

This work was scattered over a period of two months, in the case of each reactor.

TABLE I.

Reactor S. Automatic Key.

	Sensory	Av.	m. v.	Med.	Motor	Av.	m. v.	Med.	
1	(19)	144.78	25.28	146.	(19)	127.26	25.75	130.	M.
2	(20)	142.5	36.6	124.	(20)	138.85	33.52	125.	S.
3	(20)	134.3	22.2	134.5	(20)	135.25	33.8	127.	M.
4	(20)	131.65	28.78	136.5	(20)	117.75	24.33	128.5	S.
5	(23)	129.86	20.8	125.	(23)	134.04	27.33	138.	S.
6	(35)	88.17	26.38	80.	(35)	81.57	18.10	81.	S.
7	(37)	80.59	18.90	73.	(37)	89.90	30.37	80.	M.

TABLE II.

Reactor B. Automatic Key.

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.	
1	(15)	193.8	38.74	199.	(15)	156.6	52.98	135.	M.
2	(20)	157.05	25.35	157.	(20)	130.35	32.28	119.5	S.
3	(20)	97.	12.3	90.	(20)	94.45	18.9	83.5	M.
4	(35)	122.31	37.36	120.	(35)	119.42	58.22	97.	S.
5	(35)	99.74	17.23	97.	(35)	91.6	13.71	90.	M.
6	(32)	84.28	18.09	80.	(32)	90.25	29.45	78.	S.

TABLE III.

Reactor S. Plain Key.

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.	
1	(40)	128.75	22.3	122.5	(40)	99.17	21.17	95.5	M.
2	(38)	127.63	21.92	125.5	(38)	113.93	23.22	115.5	S.
3	(30)	126.86	24.65	120.5	(30)	125.83	20.71	127.5	M.
4	(30)	114.73	15.86	113.5	(30)	138.90	28.16	129.5	S.

TABLE IV.

Reactor B. Plain Key.

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.	
1	(30)	133.46	16.91	125.	(30)	128.56	22.53	120.5	M.
2	(35)	115.28	15.42	110.	(35)	110.05	17.73	102.	M.
3	(30)	123.96	20.09	120.5	(30)	148.3	41.85	134.	S.
4	(30)	110.96	24.38	93.5	(30)	92.23	17.36	84.	S.
5	(35)	104.11	18.74	99.	(30)	109.08	21.95	98.	S.

TABLE V.

Reactor B. Automatic Key.

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.	
1	(30)	95.73	14.16	92.	(30)	93.23	18.66	84.5	M.
2	(30)	96.01	17.33	92.	(30)	103.66	17.66	97.5	S.
3	(30)	102.76	22.70	97.	(30)	111.03	17.91	103.	M.
4	(30)	109.86	10.56	107.5	(30)	118.16	20.13	108.5	S.

These tables sum up the results of all the reactions, none being excluded, however unusual, except those which the reactor himself indicated currently as given during distraction; and such indications were characteristically few, although both reactors would allege after the completion of a series, that a number of the reactions were probably not good, because of disturbance by noises in another part of the building, etc. This reluctance on the part of a relatively inexperienced subject to cancel reactions or observations is always a favorable sign.

The rejection of data by the experimenter is in any case an arbitrary matter, and yet a few measurements which deviate markedly from the rest of a series ought not to be allowed to obscure the tendency manifested by the rest. That is to say, after presenting the figures for *all* the data, as we have done above, figures should be presented which represent the data from which exceptional cases have been excluded, provided the excluding is done with strict impartiality.

The most practical method of exclusion proceeds from an inspection of the data after arrangement in order of magnitude

for determination of the median. Different rules of procedure will be called for by different sorts of data. In the case in hand it was seen that by cutting out the lowest time in all series, the two highest in series ranging up to twenty reactions, and the highest four from series of twenty-five or more reactions, most of the exceptional times were eliminated. These exclusions were therefore made, and the averages computed on the basis of the reactions retained. The figures are given in Tables VI to X.

TABLE VI.

Reactor S. Automatic Key. (Table I.)

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.
1	(16)	140.75	19.4	144.	(16)	120.25	17.62	121.
2	(17)	134.41	24.64	123.	(17)	125.41	16.64	121.
3	(17)	129.23	16.09	133.	(17)	124.11	19.75	126.
4	(17)	128.64	23.66	135.	(17)	115.88	20.25	117.
5	(20)	126.95	16.35	122.	(20)	129.15	21.3	135.
6	(30)	80.36	16.4	77.	(30)	85.93	10.2	86.5
7	(32)	74.	10.75	72.	(32)	82.19	23.65	76.

TABLE VII.

Reactor B. Automatic Key. (Table II.)

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.
1	(12)	185.41	21.20	189.	(12)	147.41	21.23	132.
2	(17)	151.88	15.57	155.	(17)	118.46	19.95	119.
3	(17)	94.34	10.	90.	(17)	91.58	14.39	83.
4	(30)	114.23	16.51	113.5	(30)	103.13	35.93	92.
5	(30)	99.63	11.93	94.5	(30)	87.6	10.15	90.
6	(27)	77.92	10.67	78.	(27)	79.77	12.83	78.

TABLE VIII.

Reactor S. Plain Key. (Table III.)

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.
1	(35)	123.66	21.88	121.	(35)	93.07	14.87	91.
2	(33)	119.81	14.7	122.	(33)	112.	21.27	102.
3	(25)	110.72	11.57	111.	(25)	129.48	19.65	127.
4	(25)	118.	15.36	120.	(25)	120.6	15.56	125.

TABLE IX.

Reactor B. Plain Key. (Table IV.)

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.
1	(25)	123.19	8.27	124.	(25)	118.08	8.	117.
2	(30)	109.63	8.8	109.	(30)	104.66	9.7	101.
3	(25)	118.08	11.67	119.	(25)	135.04	29.44	121.
4	(25)	99.52	15.88	93.	(25)	88.2	12.48	85.
5	(30)	99.63	12.51	97.5	(30)	99.	14.34	97.

TABLE X.

Reactor B. Automatic Key. (Table V.)

	Sensory	Av.	m. v.	Med.	Motor.	Av.	m. v.	Med.
1	(25)	91.6	8.3	91.	(25)	84.29	8.50	93.
2	(25)	89.72	9.14	89.	(25)	97.4	9.88	96.
3	(25)	93.	10.32	93.	(25)	97.48	10.48	97.
4	(25)	105.	5.88	107.	(25)	109.76	11.35	109.

The reductions in the mean variations justify the exclusions made; and this really is the only possible justification of any arbitrary exclusions. When the rejection of a few members of a series produces a comparatively large shrinkage in the mean variation, we are justified in considering the excluded members as really exceptional. The only use we wish to make of the reduced tables, however, is to show that the relations of the unmodified tables are not accidental. This is shown by the substantial agreement of the two sets of tables in the relative values; what we have to say below will hence apply to either group of tables.

The effects of practice are clearly shown in the first two tables. Not until the last two series did either reactor get down to a stable point. The fourth table shows improvement in the use of the plain key, but the third table shows no improvement in the plain-key series of S. It must be remembered that the series were so scattered over a long period of time that no great amount of practice-improvement could be expected. What reduction in time occurs in the first five series of each reactor is due to the most general sort of habituation to the conditions

of the experiment. The alternation of motor and sensory attention in each experiment hour was a disturbing factor, and no idea can be gained from these tables of what a number of consecutive days practice on either type of attention would produce.

The mean variation is somewhat high, because of the features just mentioned; but it is always high in reaction time measurements with little practice. A review of the standard literature of reaction times discloses the great unwillingness of the majority of the experimenters to reveal exactly what their mean variations were.

The motor reaction-time is not universally shorter than the sensory. On this point some interesting observations occurred, confirming some suspicions which had been raised before. With 'sensory' attention, there is apt to be more or less division of the attention, if the reactor is not cautious; as the finger must be put in position on the key and kept there. With 'motor' attention the reactor is apt to keep his attention on the key until just about the time for the stimulus, and then shift it to the stimulus; this of course is the best form of sensory attention, and if the reactor calls this 'motor,' his so-called 'sensory' attention is pretty sure to be 'mixed.' I have no doubt that this is the case whenever reactors show consistently a motor reaction time considerably less than their sensory time. It is very easy for even an experienced subject to make these errors if his method of directing the attention is not carefully prearranged and retrospective criticism not carefully employed.

The tables show an apparent advantage of the automatic key over the plain. Under the conditions of the experiment no more could be expected. The spring of the instrument was adjusted to the best tension for the plain key, which was not the best for the automatic. The subject became so familiar with the tension of the spring during the experiments with the automatic key, that he was in the best possible condition for experiments with the plain key later.

That the automatic key gives greater speed, even under indifferent conditions, there can be no doubt. These observations have been confirmed by briefer series on three other subjects,

which agree entirely with the conclusion. Still other series, on two subjects, with sensory attention only, show a more striking advantage for the automatic key. The superiority of the new key results partly from increased facility in withdrawal of attention from the hand in the sensory series, and obviation of the cramping of the muscles in the motor, giving a freer movement in each case.

The key was designed and built to be used (if satisfactory) in an experiment, now in preparation, on an hitherto untouched point in reaction-times. The tests of this instrument have been so successful that it will be employed as planned.

THE INFLUENCE OF THE VARIATION OF WEIGHT UPON THE JUDGMENT OF EXTENT.

BY WILLIAM WILBERFORCE COSTIN, Ph.D.

In the study of illusions it is sometimes found that the experiment may be reversed and the result still be an illusion. It is interesting to note that the well known illusion of Aristotle in which the deception of touch is brought out—as when a pencil is placed between the crossed index and second finger and two pencils are felt to be there instead of one—has its converse, as seen in the fact that two objects quite a distance apart when touched by the crossed fingers will produce an impression similar to that habitually produced by a single object. The fact of the converse of Aristotle's illusion led to the question of the possibility of the converse to what is known as the weight-size illusion.

In the weight-size illusion the size clearly affects the apparent weight. For instance, equal weights (50 gr.) are compared while concealed in two boxes of different sizes. It is found, by lifting, that the weight in the larger box seems lighter than that in the smaller when they are in fact equal. The sight of the larger box leads the mind, guided by experience, to judge that it is heavier than the smaller. "But the actual weight of the larger box is so much less than the mind anticipates, and so much less than the hand is prepared to raise, that by contrast it seems much lighter than it otherwise would."¹ This illusion suggested the question as to what in some respects is, and in some respects is not, its converse, namely, what effect the variation of weight would have upon the judgment of extent.

In preparing for the experiment the following materials were employed: Rectangular blocks of rosewood were selected, cut and finished with constant dimensions in cross-section 22 mm. × 30 mm., but with lengths running from 42 mm. to

¹ Stratton, *Experimental Psychology*, p. 98.

54 mm. by steps of 1 mm. Thirteen blocks in all constituted the series. They were loaded invisibly with small shot and paraffine until each weighed 50 grammes. In addition to these there were three blocks called standards all of the same dimensions, namely, 22 mm. \times 30 mm. \times 48 mm. but of the following weights: 50 gr., 15 gr., and 85 gr.

Two methods were used in the experiment. The first the method of minimal changes. The second that of right and wrong cases, with some modification.

In the use of the first method all the blocks were employed, the series alternating from 42 mm. to 54 mm. throughout the experiment, and the three standards likewise. The subject, blindfolded, takes a position on the opposite side of the table from the experimenter, seated with his right side to the table. With his arm lengthwise with the table he holds his hand ready to lift each block, beginning with the standard, with its ends between the thumb and the second and third fingers.

Placed between the standard and the subject, half an inch away, was the first variable of the series. At the word 'ready' the subject reached forward and took the standard between his thumb and second and third fingers and lifted it from six to eight inches high. Then putting it down he took up the variable in the same way and gave a judgment as to whether the second in comparison with the first was longer, shorter or equal, or whatever his judgment might be. In this manner the whole series was gone through, both down and up, making twenty-six judgments in all for that standard. Then the 15 gr. and the 85 gr. standards were used in the same way. Seventy-eight judgments constituted one 'sitting.' This was all that was required of the subject in one day. Altogether there were seven subjects and each had five 'sittings,' a total of two thousand seven hundred and thirty judgments. Much care was taken to vary the order of series. Each subject in five sittings, was given the following variations in the standards employed: (1) 15 gr., 50 gr., 85 gr.; (2) 85 gr., 15 gr., 50 gr.; (3) 50 gr., 15 gr., 85 gr.; (4) 50 gr., 85 gr., 15 gr.; (5) 15 gr., 85 gr., 50 gr.

In going up and down the series from No. 1 to 13 and 13 to 1, the order was varied, in each sitting, and from sitting to sitting.

The following shows the form of the record kept for the judgments of each subject.

TABLE A.

STANDARD. WEIGHT, 50 GR. LENGTH, 48 MM.				STANDARD. WEIGHT, 15 GR. LENGTH, 48 MM.				STANDARD. WEIGHT, 85 GR. LENGTH, 48 MM.			
Length of variable in mm.	Judgment.	Length of variable in mm.	Judgment.	Length of variable in mm.	Judgment.	Length of variable in mm.	Judgment.	Length of variable in mm.	Judgment.	Length of variable in mm.	Judgment.
54		42		42		54		54		42	
53		43		43		53		53		43	
52		44		44		52		52		44	
51		45		45		51		51		45	
50		46		46		50		50		46	
49		47		47		49		49		47	
48		48		48		48		48		48	
47		49		49		47		47		49	
46		50		50		46		46		50	
45		51		51		45		45		51	
44		52		52		44		44		52	
43		53		53		43		43		53	
42		54		54		42		42		54	

Results by the Method of Minimal Changes.

In order to estimate the results of this method the various judgments were tabulated as follows:

The 'calculated equality-point' was determined by taking the length of the block at which the judgments 'longer' stop, and also of that at which the judgments 'shorter' stop, and taking their arithmetic mean. The difference between the two values which enter into such a mean is stated, under the heading 'zone of doubt.' The results thus obtained from each series of 13 'lifts' is put in the table as a separate entry; and at the bottom the average of these and the median. Under V are entered the variations of the 'equality point' from the average.

The following are the tables for all the subjects:

TABLE I.
Subject Ba.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>
49.5	.4	I	47.5	I.3	I	52.	2.45	2
49.5	.4	I	49.5	.7	I	50.5	.95	I
49.5	.4	I	47.5	I.3	I	50.5	.95	I
48.5	.6	I	50.5	I.7	I	49.5	.05	I
49.5	.4	I	47.5	I.3	I	48.5	I.05	I
49.5	.4	I	50.5	I.7	I	31.5	I.95	I
48.5	.6	I	50.5	I.7	I	46.5	3.05	I
46.5	2.6	I	48.5	.3	3	48.5	I.05	I
51.5	2.4	I	47.5	I.3	I	50.5	.95	I
48.5	.6	I	48.5	.3	I	47.5	2.05	I
Ave'ge 49.1	.88	I.	48.8	I.16	I.2	49.55	I.35	I.1
Median 49.5			48.5			50.		

TABLE II.
Subject Bu.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>
50.5	I.45	3	49.5	2.1	I	49.	.35	2
50.5	I.45	3	50.	2.6	2	50.	I.35	2
48.	I.05	2	49.	I.6	2	48.5	.15	3
49.5	.45	I	48.5	I.1	3	49.	.35	2
47.	2.05	2	47.	.4	2	49.	.35	2
47.	2.05	2	42.	5.4	4	47.5	I.15	I
49.5	.45	3	46.	I.4	2	48.5	.15	3
47.5	I.55	I	47.5	.1	I	48.5	.15	3
51.	I.95	2	47.	.4	2	47.5	I.15	2
50.	.95	2	47.5	.1	2	49.	.35	I
Ave'ge 49.05	I.34	2.1	47.4	I.52	2.1	48.65	.55	2.1
Median 49.5			47.5			48.75		

TABLE III.
Subject C.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>
48.	.1	4	47.	.45	2	49.5	.9	3
47.5	.4	3	46.	1.45	2	48.	.6	2
48.	.1	2	49.	1.55	2	49.	.4	2
49.	1.1	4	49.5	2.05	5	49.	.4	2
48.	.1	2	46.	1.45	2	48.5	.1	3
48.5	.6	7	47.5	.05	3	49.	.4	2
47.5	.4	3	48.5	1.05	3	47.5	1.1	3
48.	.1	4	45.5	1.95	3	47.5	1.1	3
47.	.9	4	47.	.45	4	49.	.4	2
47.5	.4	1	48.5	1.05	7	49.	.4	2
Ave'ge 47.9	.42	3.4	47.45	1.15	3.3	48.6	.58	2.4
Median 48.			47.25			49.		

TABLE IV.
Subject D.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>
52.	2.6	2	47.5	.35	9	50.5	.55	5
49.5	.1	5	45.5	2.35	7	49.	.95	4
48.5	.9	3	47.	.85	8	50.	.05	4
48.5	.9	1	49.	1.15	2	50.	.05	4
49.	.4	8	47.	.85	8	49.	.95	4
50.5	1.1	3	48.	.15	6	50.	.05	6
49.5	5.	3	50.	2.15	4	50.	.05	2
47.	2.4	6	49.5	1.65	5	50.5	.55	5
49.	.4	2	47.5	.35	5	51.5	1.55	1
50.5	1.1	1	47.5	.35	7	49.	.95	2
Ave'ge 49.4	1.	3.4	47.85	1.02	6.1	49.95	.57	3.7
Median 49.25			47.5			50.		

TABLE V.
Subject F.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>
52.	3.85	6	46.5	.6	3	50.5	1.8	9
48.	.15	4	47.	.1	2	47.5	1.2	5
49.	.85	6	45.	2.1	2	50.	1.3	6
46.5	1.65	3	46.	1.1	4	47.	1.7	6
50.	1.85	6	46.	1.1	2	48.5	.2	1
46.	2.15	2	49.5	2.4	1	45.5	3.2	3
48.5	.35	1	46.5	.6	1	46.5	2.2	1
47.5	.65	1	49.5	2.4	1	50.5	1.8	1
46.5	1.65	1	46.5	.6	1	51.5	2.8	1
47.5	.65	1	48.5	1.4	1	49.5	.8	1
Av'age 48.15	1.38	3.1	47.1	1.24	1.8	48.7	1.7	3.4
Median 47.75			46.5			49.		

TABLE VI.
Subject S.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>
50.	.0	2	54+	2.85	1	54+	3.05	1
50.5	.5	1	53+	1.85	2+	47.5	1.95	8
50.5	.5	3	50	1.15	2	51.5	.55	5
51.	1.	4	50	1.15	2	49.5	1.45	1
51.5	1.5	3	53	1.85	2	51.	.05	2
47.	3.	4	49.5	1.65	7	50.5	.45	1
50.	.0	2	53	1.85	2	52.5	1.55	1
47.5	2.5	3	51.5	.35	1	50.	.95	4
51.	1.	2	48.5	2.65	1	50.5	.45	1
51.	1.	4	49.	2.15	2	51.	.05	2
Av'age 50.	1.1	2.8	51.15	1.75	2.2	50.95	1.05	2.6
Median 50.5			50.75			50.75		

TABLE VII.

Subject W.

STANDARD 50 GR.			STANDARD 15 GR.			STANDARD 85 GR.		
Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.	Calculated equality point.	V.	Zone of doubt.
<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>	<i>mm.</i>		<i>mm.</i>
51.5	3.1	1	48.	.5	2	46.5	1.9	1
47.5	.9	3	48.5	.0	3	47.5	.9	1
47.5	.9	1	48.5	.0	1	48.5	.1	1
46.5	1.9	1	49.5	1.0	3	49.5	1.1	1
48.	.4	2	48.	.5	2	48.	.4	2
48.	.4	2	48.	.5	2	48.5	.1	1
46.5	1.9	1	48.	.5	2	49.5	1.1	3
48.5	.1	2	48.	.5	2	48.	.4	2
48.5	.1	1	49.	.5	4	50.	1.6	2
51.5	3.1	3	49.5	1.0	1	48.	.4	2
Av'age 48.4	1.28	1.7	48.5	.5	2.2	48.4	.8	1.6
Median 48.			48.25			48.25		

Tables Explained.

Subject Ba, Table I.

Taking the averages under 'calculated equality point,' the judgments of subject Ba were such that the block weighing 50 gr. (the weight of all in the series) appeared (as we may estimate) to be 49.1 mm. long, whereas in fact it was only 48 mm. long. That is, the estimated length made it 1.5 mm. longer than the block really was. This difference was not the effect of weight, for the standard and the other blocks were all of the same weight. It was rather the effect of the 'constant-error,' due to the make-up of the subject plus the influence of the order of the series, etc. Now take the effect of using standard 15 gr., 48 mm. long. The same subject estimated on the average that the standard was .8 mm. longer than it really was. The question is: Did the lightening up of the standard affect the estimate

of the length? If we suppose the 'constant-error' the same as before, namely, the tendency to make 49.1 mm. seem equal to 48 mm. when both weigh 50 gr., then the lightening of the standard to 15 gr. had the effect of making it seem 48.8 mm. long, whereas the 50 gr. standard made it seem 49.1 mm. long—a difference of .3 mm. That is, the lightening of the standard made this standard seem .3 mm. shorter than the length at which the 'constant-error' placed it.

When the 85 gr. block was used as the standard the average was 49.55 mm. That is, the standard seemed .45 mm. longer than the length at which the 'constant-error' placed it. By taking the median lengths instead of the average the differences of judgments became slightly more pronounced. For instance the median with the 50 gr. block as standard was 49.5 mm. That of the 15 gr. standard was 48.5 mm., a lengthening of .5 mm. The outcome is to the effect that to lighten an object seems to reduce its length, and to weight down an object seems to increase its length.

The results with subject Bu indicate that to lighten an object decreases its length and to weight down an object does the same thing. In the case of subject C, to lighten an object decreases the length and to weight down an object increases the length. Subject D's results show that to lighten an object seems to reduce its length and to weight down an object increases its length. The results of subject F indicate that to lighten an object decreases its length and to weight down an object increases its length. In the case of subject S, to lighten an object increases its length and to weight down an object does the same thing. Subject W's results show that to lighten an object perhaps slightly increases its length and to weight down an object has little or no effect upon the estimate of its length.

From this method—that of minimal changes—the following summary statement may be made, namely, that while the results of subjects S and W indicate that to lighten up an object seems to increase its length, and subject Ba's results show that to weight down an object seems to decrease its length, and the results of subject W indicate no effect at all from weighting, yet by taking all seven subjects together the aggregated aver-

ages show that to lighten up an object tends somewhat to decrease its length and to weight down an object slightly increases its length.

The following is the table of aggregated averages and medians:

AGGREGATED AVERAGES AND MEDIANS.

Subject.	STANDARD. WEIGHT, 50 GR. LENGTH, 48 MM.				STANDARD. WEIGHT, 15 GR. LENGTH, 48 MM.				STANDARD. WEIGHT, 85 GR. LENGTH, 48 MM.			
	Calculated equality point. (Average.)	M. V.	Zone of doubt.	Calculated equality point. (Median.)	Calculated equality point. (Average.)	M. V.	Zone of doubt.	Calculated equality point. (Median.)	Calculated equality point. (Average.)	M. V.	Zone of doubt.	Calculated equality point. (Median.)
Ba	49.1	.88	1.	49.5	48.8	1.16	1.2	48.5	49.55	1.35	1.1	50.
Bu	49.05	1.34	2.1	49.5	47.4	1.52	2.1	47.5	48.65	.55	2.1	48.75
C	47.9	.42	3.4	48.	47.45	1.15	3.3	47.25	48.6	.58	2.4	49.
D	49.4	1.	3.4	49.25	47.85	1.02	6.1	47.5	49.95	.57	3.7	50.
F	48.15	1.38	3.1	47.75	47.1	1.24	1.8	46.5	48.7	1.7	3.4	49.
S	50.	1.1	2.8	50.5	51.15	1.75	2.2	50.75	50.95	1.05	2.6	50.75
W	48.4	1.28	1.7	48.	48.5	.5	2.2	48.25	48.4	.8	1.6	48.25
	48.85	1.05	2.5	48.92 +	48.32 +	1.19 +	2.7	48.03 +	49.25 +	.94 +	2.4 +	49.39 +

Results by the Method of Right and Wrong Cases.

In order to check the results obtained by the method of minimal changes the method of right and wrong cases with some modification was used and the following four of the same group of subjects were chosen, namely, Bu, C, D and S. The same standards were employed and three blocks from the series were selected being respectively 50 mm., 48 mm., and 46 mm. in length. Nine cards were prepared with the following combinations written upon them; it being understood, of course, that the weight given is that of a block 48 mm. long:

50 gr. 46 mm.	15 gr. 46 mm.	85 gr. 46 mm.
50 gr. 48 mm.	15 gr. 48 mm.	85 gr. 48 mm.
50 gr. 50 mm.	15 gr. 50 mm.	85 gr. 50 mm.

These cards were shuffled; then the combinations were placed permanently in the record in the order in which they happened to come up. For instance in the case of C the following is an illustration of the order in one shuffling:

Gram. Wt. of standard 48 mm.	Mm. length of variable wt. 50 gr.
85	50
15	46
15	50
50	50
50	48
15	48
85	48
50	46
85	46

There were ten sets of these group combinations of nine, each set being arranged by an entirely independent shuffle, making in all ninety judgments which constituted one 'sitting'—all that was required in one day.

There were five 'sittings' for each subject, that is, four hundred and fifty judgments for each, or one thousand eight hundred judgments in all.

The blocks were offered to the subject as the shuffling of the cards called for. As before, the judgments of longer, shorter, equal, etc.—a judgment describing the second block in comparison with the first block as standard—were made after lifting in succession the two blocks.

For any one 'sitting' the following table will illustrate the distribution of judgments for the different combinations of blocks:

Pair Compared	Judgments 'Shorter'	Judgments 'Equal'	Judgments 'Longer'
50-46.....	8	1	1
50-48.....	1	7	2
50-50.....		1	9
<hr/>			
15-46.....	6	4	
15-48.....	2	6	2
15-50.....			10
<hr/>			
85-46.....	7	3	
85-48.....	3	7	
85-50.....	1	4	5

The following tables give the distribution of judgments for each subject for the five sittings:

Subject Bu.

	'Shorter'					'Equal'					'Longer'					'Equal or Longer'	'Equal or Shorter'
50-46...	8	9	7	8	9	1	1	3	2	1	1						
50-48...	1	1	2	1	4	7	8	6	7	6	2	1	2	2			
50-50...						1	1	4	2	6	9	9	6	8	4		
<hr/>																	
15-46...	6	9	7	4	7	4	1	2	4	2			1	2	1		
15-48...	2	2		1		6	8	8	4	8	2		2	5	2		
15-50...							2		3	1	10	8	10	7	9		
<hr/>																	
85-46...	7	10	8	9	8	3		2	1	2							
85-48...	3	3	3	6	3	7	6	7	4	7		1					
85-50...	1			1		4	5	6	5	9	5	5	4	4	1		

Subject C.

	'Shorter'					'Equal'					'Longer'					'Equal or Longer'	'Equal or Shorter'
50-46...	9	9	7	6	7		1	3	1	2	1			3	1		
50-48...	2	3		1	1	3	5	4	4	6	4	2	6	5	3	1	
50-50...						1	2	2		2	9	8	8	10	8		

15-46...	3	3	2	2	3	5	6	5	6	5	2	1	3	2	2		
15-48...	1		1		1	1	3	2		1	8	7	7	10	8		
15-50...	1						1		1		9	9	10	9	10		

85-46...	10	8	8	10	9		2	2		1							
85-48...	6	4		4	4	3	4	10	5	3	1	2		1	3		
85-50...	1	1		1		5	2	7	2	2	4	7	3	7	8		

Subject D.

	'Shorter'					'Equal'					'Longer'					'Equal or Longer'	'Equal or Shorter'
50-46...	10	8	9	7	7		1		2	2		1	1	1	1		
50-48...	8	3	4	3	5	1	2	3	3	4	1	5	3	4	1		
50-50...	5	2	1	2	2	1	1	1			4	7	8	8	8		

15-46...	5	2	7	4	5	2	2	3	2	1	3	6		4	4		
15-48...	3	1	4	2	4	1		1	3	3	6	9	5	5	3		
15-50...	2		2	2					1	1	8	10	8	7	9		

85-46...	10	8	9	8	10		1	1	1			1		1			
85-48...	9	9	7	4	6	1				2		1	3	6	2		
85-50...	7	6	3	5	2	1	2	3	1	4	2	2	4	4	4		

Subject S.

	'Shorter'					'Equal'					'Longer'					'Equal or Longer'	'Equal or Shorter'
50-46...	7	7	7	7	8	3	3	2	2	1					1	1	1
50-48...	5	8	5	4	4	4	1	3	5	3		1	2	1	3	1	
50-50...	3	2		1	1		4	3	4	2	6	4	7	5	6	2	
15-46...	8	10	9	8	10	2			2				1				
15-48...	9	7	4	8	7	1	2	2	2	3		1	4				
15-50...	5	1	1	3	3	1	3	5	3	3	4	6	4	4	4		
85-46...	9	9	8	7	5	1		1	3	5		1	1				
85-48...	7	9	5	5	3	3		4	4	7		1	1	1			
85-50...	5	3				3	6	4	3	4	1	1	6	7	6	1	

Combining them in summary the results are as follows:

SUBJECT BU.				SUBJECT C.				SUBJECT D.				SUBJECT S.						
	'Shorter'	'Equal'	'Longer'		'Shorter'	'Equal'	'Longer'	'Equal or Longer'		'Shorter'	'Equal'	'Longer'		'Shorter'	'Equal'	'Longer'	'Equal or Longer'	'Equal or Shorter'
50-46	41	8	1	50-46	38	7	5		50-46	41	5	4	50-46	36	11	2	1	
50-48	9	34	7	50-48	7	22	20	1	50-48	23	13	14	50-48	26	16	7	1	
50-50	0	14	36	50-50	0	7	43		50-50	12	3	35	50-50	7	13	28	2	
15-46	33	13	4	15-46	13	27	10		15-46	23	10	17	15-46	45	4	1		
15-48	5	34	11	15-48	3	7	40		15-48	14	8	28	15-48	35	10	5		
15-50	0	6	44	15-50	1	2	47		15-50	6	2	42	15-50	13	15	22		
85-46	42	8	0	85-46	45	5	0		85-46	45	3	2	85-46	38	10	2		
85-48	18	31	1	85-48	18	25	7		85-48	35	3	12	85-48	29	18	3		
85-50	2	29	19	85-50	3	18	29		85-50	23	11	16	85-50	8	20	21	1	

In order to estimate the results of this method let us examine the judgments of any subject from the above table, say subject S. Sixteen times he estimated block 48 mm. 'equal' in length to standard 50 gr. (48 mm.); twenty-six times he said it was 'shorter' than the standard, and seven times that it was 'longer,' and once he judged it either 'equal or longer'. It is evident that the 'constant-error' so influenced the estimate as to cause the subject to say twenty-six times that block 48 mm. was 'shorter' than the 50 gr. standard when they were actually 'equal' in length. Now take the effect of lightening the standard to 15 gr. If the lightening has no effect the result should approximate 26 'shorter,' 16 'equal,' 7 'longer' and 1 'equal or longer,' but as a matter of fact it stands 35 'shorter,' 10 'equal,' and 5 'longer.' What has been the effect of lightening? Evidently this, that nine judgments have been put into the 'shorter' column, and two have been taken from the 'longer' column. The lightening of the object thus seems to have had the effect of increasing its apparent length.

Now take the case where the standard is weighted down to 85 gr. If the weighting has no effect on the length we should expect to find the result approximately 26 'shorter,' 16 'equal,' 7 'longer' and 1 'equal or longer.' But in fact we find it 29 'shorter,' 18 'equal' and 3 'longer.' That is 4 'longer,' and 1 'equal or longer' have been distributed into the 'equal' and 'shorter' columns increasing these to 18 and 29 respectively. This result indicates that increasing the weight increased the apparent length.

Taking block 46 mm. and the 50 gr. standard, the 'constant-error' distributes the judgments as follows: 36 'shorter,' 11 'equal,' 2 'longer' and 1 'shorter or equal.' Comparing that with the result when the standard is lightened, the result stands 45 'shorter,' 4 'equal' and 1 'longer.' Nine judgments have been put in the 'shorter' column, indicating again that to lighten the object tends to increase its length.

For the other subjects it is only necessary to give the general conclusions. Subjects Bu, C and D, all show the same general result, namely, that to lighten an object is to decrease its apparent length, and to weight down an object is to increase

its apparent length. Subject S reversed this conclusion so far as lightening the standard is concerned, in this and the method of minimal changes also.

Subject Bu's judgments by this method of right and wrong cases come out somewhat different from those obtained by the method of minimal changes. By the method of minimal changes his judgments show that to weight down decreases the apparent length; but by this method—right and wrong cases—his results indicate that to weight down increases the length, *i. e.*, his judgment here accords with those of the main group of subjects.

The following is a summary of the results of all the subjects according to both methods:

METHOD OF MINIMAL CHANGES.

Subject.	Effect of decreasing the weight.	Effect of increasing the weight.
Ba	decrease of apparent size	increase of apparent size
Bu	" " "	decrease " "
C	" " "	increase " "
D	" " "	" " "
F	" " "	" " "
S	increase " "	" " "
W	" " "	seems not to affect size

METHOD OF RIGHT AND WRONG CASES.

Subject.	Effect of decreasing the weight.	Effect of increasing the weight.
Bu	decrease of apparent size	increase of apparent size
C	" " "	" " "
D	" " "	" " "
S	increase " "	" " "

It is evident, taking both methods together, that for most persons, and after making allowance for the constant error due to the succession in comparison, to lighten up an object seems to decrease its length and to weight down an object seems to

increase its length. We thus have an illusion in tactual space-perception that in some respects is, and in some respects is not, the converse of the weight-size illusion.

The exact converse of the weight-size illusion would be something as follows:

The illusion would start from the experience which the different weights arouse. Experience has taught us that in general the size of things increases with their weight. The actual size of the heavier box would be so much less than we anticipated and so much less than we had prepared the hand to grasp that by contrast it would seem smaller than it actually was. Its size would be underestimated and for like reasons the size of the lighter box would be overestimated.

We have said that the actual illusion found by these experiments in some respects is, and in some respects is not, the converse of the weight-size illusion. It is the converse of it in this, that the variation of weight has an influence upon the judgment of extent: whereas in the weight-size illusion the variation of size has an influence upon the judgment of weight. It is not the converse, however, with regard to the exact character of this influence. In the weight-size illusion it is found that to increase the size tends to the apparent decrease of weight, and that to decrease the size tends to an apparent increase of weight. The converse of this would be the apparent increase of size with the decrease of weight and the apparent decrease of size with the increase of weight. But we have found by these experiments that in most cases the decrease of weight apparently decreases the size and the increase of weight apparently increases the size. But in one case increase in weight seems to decrease the apparent size and in two other cases to decrease the weight seems to increase the apparent size; so that in three instances we have to a greater extent the converse of the weight size illusion; while in all the other cases the results are the converse only to the extent above stated.

Insofar as these results are not the exact converse of the weight-size illusion their departure is perhaps to be explained as follows: The method or way of judging here was unquestionably different from that employed normally in the weight-size illu-

sion. In the weight-size illusion the heavier box is estimated as larger, in advance of actual touch, and the hand is prepared to clasp it as such; but, as felt—in contrast with what was estimated as its size—it feels much smaller than it otherwise would; whereas in the illusion herein described the method is different. The estimate is not made in advance of touch but as its result. The heavier block is estimated as larger than it really is because it is ingrained in us that volume increases with weight. The lighter block is estimated as smaller for like reasons.

The reason for the illusion disclosed by the present experiments is that experience or custom has taught us that in general the weights of things decrease or increase with their volume. This has become so ingrained that we are influenced when the weight is increased to say that there is also an increase in the volume, although the volume remains constant; and when the weight is decreased, to say, that the volume is decreased also, the volume remaining constant.

In the cases of subjects S and W where an increase of apparent size tends to decrease the weight the probable explanation may be found in a failure to find in the object the size which was anticipated because of the decrease of weight; and the result by way of contrast and by a sort of disappointment was an apparent increase of size.

ON THE TRANSFORMATION OF MEMORY CONTENT IN THE COMPARISON OF LIGHTS.

BY T. A. LEWIS.

The object of the experiment was to determine how the comparative judgment of two successive light stimuli is affected by the time interval between the stimuli and by the intensity of the standard stimulus, i. e., by the absolute brightness.

G. M. Stratton¹ refers to work, done by students in the psychological laboratory of the University of California, indicating that a sound or light seems in memory to be now more and now less intense than a sound or light of actually equal intensity given afterwards.

J. H. Leuba² by his experiments with artificial stars, has made prominent the idea that the behavior of the memory content depends in part upon the quantity of the standard light, finding that there is a tendency for the subjects' estimates to approach the mean of all the different intensities previously used in the experiment; that the light used as standard when at an extreme—either over-strong or over-weak—is shorn of its excess in the judgment of comparison. If the standard was relatively too dim, the second light, to seem its equal, had to be somewhat brighter; and if relatively too bright, the second had to be somewhat less intense.

Baldwin, Warren, and Shaw,³ in experimenting with classes of college students by means of square figures, to determine the effect of time on memory for size, found that there was a continued increase in the memory estimate of the standard square directly with the increase in time.

¹ Experimental Psychology and its Bearing on Culture, p. 174.

² Amer. Jour. of Psy., Vol. V., p. 370.

³ Psychol. Rev., II, 236.

Lehmann¹ writing on sound found that the judgment of comparison between the absolute and the succeeding variable gave proof that the memory content was minimized by time; a weaker second sound being considered equal to an earlier sound with which it was compared. This experience of Lehmann's was paralleled in another sense-field by F. Kennedy, who, experimenting with pressure sensations having a time-interval of three seconds intervening between the two stimuli compared, found evidence that the quantitative change in memory estimate was towards the limen.

These references, representative of the effort put forth along the line of my investigation, give an idea of its motive and purpose.

In working at the problem here reported, the following apparatus was used: a gas-flame shut in by a sheet-iron hood having a square opening of 50 mm. in front, which allowed the light-rays to pass out and directly through a lens that focused them on a small plate of ground glass covering a hole 9 mm. in



Diagram of arrangement: F, gas flame; B, opaque box; L, lens; S', first screen; D, disk of episkotister; S'', second screen; E, eye of observer.

diameter in an opaque screen (S'). The size of the image focused on the screen was such that the 9 mm. opening occupied

¹ Phil. Studien VII, 169.

a position safely remote from the flickering edge and the unstable blue core. In this way the maximum of constancy of illumination of the translucent opening was obtained. The standard intensities were fixed by four diaphragms with square openings in size, respectively, 3 mm., 12 mm., 24 mm., and 48 mm. The standard in use was fitted to the lens. One meter from S' was a second opaque screen S'' that served to conceal from the subject the nature of the changes made and also to cut off from him scattering light rays. In the center of S'' was a hole 19 mm. in diameter, that could be opened or closed by means of a small square of card-board attached to one end of a pivoted rod within reach of the experimenter. In the path of light when it threaded the two screens was a Marbe Rotator arranged as an episkotister whose angular openings had a range from zero to one hundred and eighty degrees. The experiment was conducted in a room darkened except for scattering light from the apparatus. From fifteen to twenty minutes were allowed the subject for adaptation, and at the end of such time he could make out the profile of the experimenter and in a general way the surrounding objects. For some moments after entering the room the darkness seemed absolute. To give light for the operator, I used a single two-candle power electric light enclosed in a card-board box with an opening beneath, directly above the reading scale of the episkotister. An electric clock, connected with a 'second' pendulum, was used to count off time for the experimenter. The subject sat so that his left eye was one meter from S'' and he was fixed in position by a head-rest. The episkotister was always started long enough before exposing the light, to attain a speed exceeding the flicker limit. Whether the light was to be standard or variable was made known to the subject about six seconds before exposure; and then, just two seconds before giving the light, that the subject might get into a proper frame of mind, I gave the signal, 'ready.'

The episkotister was always set at 90° opening for the standard light. The tables show that there were twelve 'variable' lights, and—since two time intervals were used—twenty-four different judgments of comparison. Which vari-

able would be used and what interval—two minutes or two seconds—was decided entirely by lot. The subject was not told what the time-interval would be, and so always expected the second light two seconds after the first, knowing that in case it failed to appear in two seconds the interval would be two minutes.

The experiment was carried out on three subjects, and was therefore necessarily very slow. Only six complete determinations for each of the four standards were obtained from subjects D and B. From subject S two determinations were obtained for the two lowest standards (St. 1 and St. 2), and three and four for the other two (St. 3 and St. 4).

The results are given in Tables I, II and III, being in order the results from subjects D, B, and S. In these tables, directly beneath the heading representing the different standards (St. 1, St. 2, St. 3, St. 4, given in an order of increasing intensity) are the number of judgments, divided into two groups, 'dimmer' (less intense) and 'brighter' (more intense), given for each variable compared with each of the four standards and for the two different time-intervals. The column of judgments 'dimmer' is headed with L (less intensity) and the column of judgments 'brighter' with G (greater intensity). The judgments 'equal' or 'same' were divided between L and G.

The variables are given in degrees of angular opening of the episkotister.

The deliverance of this investigation on how the comparative judgment of two successive light stimuli is affected by the interval between the stimuli seems to be that the judgment is led to underestimate the light that sensation entrusted to memory. A line run through 90° in the table would cut all the variables of standard intensity. Accurate memory would have balanced at this line the judgments L and G for any one time-interval and standard intensity. But subject D makes the equal division (the transition line from a majority of L to a majority of G estimates) of five out of the eight columns below this line (as indicated by short black line); subject B of six out of eight; and subject S puts the transition point of all

eight below the 90° line. Subject S's results cannot be given as great weight as the other results, he having had scarcely half as many 'sittings' as subjects D and B.

In regard to the way the memory-estimate is affected by the intensity of the standard stimulus, the results seem to give some (but by no means uniform) evidence that accords with the theory proposed by Leuba, viz, that a light sensation passing through memory shortly after kindred sensations, is deflected toward

TABLE I.
Subject: Dr. Dunlap.

Variable	St. 1				St. 2				St. 3				St. 4			
	2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.	
	G	L	G	L	G	L	G	L	G	L	G	L	G	L	G	L
15°		6		6		6		6		6		6		6		6
30°		6		6		6		6		6		6		6		6
45°	$\frac{1}{2}$	$5\frac{1}{2}$		6	$\frac{1}{2}$	$5\frac{1}{2}$		6	$\frac{1}{2}$	$5\frac{1}{2}$		6	2	4		6
60°	1	5	$\frac{1}{2}$	$5\frac{1}{2}$	3	3		6		6		6	3	3		6
75°	2	4	2	4	4	2	$\frac{1}{2}$	$5\frac{1}{2}$	$2\frac{1}{2}$	$3\frac{1}{2}$	$1\frac{1}{2}$	$4\frac{1}{2}$	5	1	2	4
90°	4	2	2	4	5	1	$1\frac{1}{2}$	$4\frac{1}{2}$	$4\frac{1}{2}$	$1\frac{1}{2}$	2	4	4	2	$4\frac{1}{2}$	$1\frac{1}{2}$
105°	$5\frac{1}{2}$	$\frac{1}{2}$	$3\frac{1}{2}$	$2\frac{1}{2}$	$4\frac{1}{2}$	$1\frac{1}{2}$	$2\frac{1}{2}$	$3\frac{1}{2}$	5	1	$3\frac{1}{2}$	$2\frac{1}{2}$	5	1	5	1
120°	6		3	3	6		4	2	$3\frac{1}{2}$	$2\frac{1}{2}$	3	3	$4\frac{1}{2}$	$1\frac{1}{2}$	$4\frac{1}{2}$	$1\frac{1}{2}$
135°	5	1	$5\frac{1}{2}$	$\frac{1}{2}$	6		5	1	$4\frac{1}{2}$	$1\frac{1}{2}$	6		$5\frac{1}{2}$	$\frac{1}{2}$	$3\frac{1}{2}$	$2\frac{1}{2}$
150°	6		$5\frac{1}{2}$	$\frac{1}{2}$	$5\frac{1}{2}$	$\frac{1}{2}$	6		6		6		6		5	1
165°	6		$5\frac{1}{2}$	$\frac{1}{2}$	6		5	1	$4\frac{1}{2}$	$1\frac{1}{2}$	6		6		6	
180°	6		4	2	$5\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	$5\frac{1}{2}$	6		6		6		6	

the line of mean intensity of these earlier sensations. How my investigation agrees with this can be seen by comparing the points of transition or balance in the columns of the extreme lights St. 1 and St. 4. For 2-minute intervals subject D with St. 1 puts the balance-point at an intensity between 75° and 90° —thus below the standard height; and St. 4 is discounted for over-brightness by being estimated still lower, namely, as equal to a variable of 60° opening. Subject B for long intervals as

well as for short has the balance-point apparently a little higher for St. 1 than for St. 4; while subject D for short intervals puts the balance point for St. 1 well above the standard and for St. 4 below the standard. On the other hand, with both D and B, the estimates for St. 3 are about the same as for St. 1.

The point brought out by the students in the Psychological Laboratory of the University of California, namely, that in the

TABLE II.

Subject: Dr. Burrow.

Variable	St. 1				St. 2				St. 3				St. 4			
	2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.	
	G	L	G	L	G	L	G	L	G	L	G	L	G	L	G	L
15°		6		6		6		6		6		6		6		6
30°		6		6		6	I	5		6		6	$\frac{1}{2}$	5 $\frac{1}{2}$	$\frac{1}{2}$	5 $\frac{1}{2}$
45°		6		6	I	5	$\frac{1}{2}$	5 $\frac{1}{2}$	1 $\frac{1}{2}$	4 $\frac{1}{2}$		6	1 $\frac{1}{2}$	4 $\frac{1}{2}$		6
60°		6		6	2 $\frac{1}{2}$	3 $\frac{1}{2}$	1 $\frac{1}{2}$	4 $\frac{1}{2}$		6	$\frac{1}{2}$	5 $\frac{1}{2}$	3	3	1 $\frac{1}{2}$	4 $\frac{1}{2}$
75°	2 $\frac{1}{2}$	3 $\frac{1}{2}$	4	2	4	2	3	3	2	4	2	4	2 $\frac{1}{2}$	3 $\frac{1}{2}$	2	4
90°	3 $\frac{1}{2}$	2 $\frac{1}{2}$	3	3	3 $\frac{1}{2}$	2 $\frac{1}{2}$	2 $\frac{1}{2}$	3 $\frac{1}{2}$	4 $\frac{1}{2}$	1 $\frac{1}{2}$	4	2	3 $\frac{1}{2}$	2 $\frac{1}{2}$	5	I
105°	6		3	3	3 $\frac{1}{2}$	2 $\frac{1}{2}$	3 $\frac{1}{2}$	2 $\frac{1}{2}$	4 $\frac{1}{2}$	1 $\frac{1}{2}$	4	2	4 $\frac{1}{2}$	1 $\frac{1}{2}$	4	2
120°	5	I	3 $\frac{1}{2}$	2 $\frac{1}{2}$	5 $\frac{1}{2}$	$\frac{1}{2}$	4	2	5	I	4	2	5 $\frac{1}{2}$	$\frac{1}{2}$	4 $\frac{1}{2}$	1 $\frac{1}{2}$
135°	4 $\frac{1}{2}$	1 $\frac{1}{2}$	3 $\frac{1}{2}$	2 $\frac{1}{2}$	5 $\frac{1}{2}$	$\frac{1}{2}$	4 $\frac{1}{2}$	1 $\frac{1}{2}$	5 $\frac{1}{2}$	$\frac{1}{2}$	4	2	4 $\frac{1}{2}$	1 $\frac{1}{2}$	5 $\frac{1}{2}$	$\frac{1}{2}$
150°	4 $\frac{1}{2}$	1 $\frac{1}{2}$	5	I	5	I	5 $\frac{1}{2}$	$\frac{1}{2}$	5 $\frac{1}{2}$	$\frac{1}{2}$	5 $\frac{1}{2}$	$\frac{1}{2}$	5 $\frac{1}{2}$	$\frac{1}{2}$	6	0
165°	6		5	I	6		6		5 $\frac{1}{2}$	$\frac{1}{2}$	5	I	5 $\frac{1}{2}$	$\frac{1}{2}$	5 $\frac{1}{2}$	
180°	5	I	5	I	6		4	2	6		6		6		6	

case of sound and light there is a magnifying of the memory estimate for an interval of two seconds immediately following the sensation, finds a parallel in the results from but one observer in this experiment. Subject D's judgments for the short (2-seconds) interval show an elation of the memory content three times out of the four, the point of balance (where the variable light seems to equal the standard) being raised to 105° and above. But subject B's and subject S's judgments show no similar swelling of the memory estimate.

Finally, aside from this working along the investigations of others, there was another phase in memory whose nature I wished to investigate. This phase might be called the range of inconstancy and non-sustaining of earlier judgments (the same stimuli for comparison recurring); the place where the operator is never certain what the subject when comparing the variable with the earlier standard is going to answer. One would think that the memory after a long interval would upon

TABLE III.

Subject: Mr. South.

Variable.	Sr. 1.				Sr. 2.				Sr. 3.				Sr. 4.			
	2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.		2 min.		2 sec.	
	G	L	G	L	G	L	G	L	G	L	G	L	G	L	G	L
15°		2		2		2		2		3		3		4		4
30°		2		2		2		2		3		3		4		4
45°		2		2		2		2		3	I	2	I	3	I	3
60°	I½	½		2	½	I½	½	I½	I	2	I	2	3	I	½	3½
75°		2	½	I½	½	I½	½	I½	2	I	½	2½	I	3	2	2
90°	2		I½	½	I½	½	I½	½	2	I	2½	½	3	I	3	I
105°	2		2		I	I	I½	½	I½	I½	2	I	4		3	I
120°	2		2		I½	½	I½	½	3		2½	½	3½	½	2½	I½
135°	2		I½	½	2		I½	½	2	I	2½	½	3½	½	3½	½
150°	I½	½	2		I	I	I½	½	3		2	I	4		4	
165°	2		2		I	I	2		3		2½	½	4		4	
180°	2		I½	½	2		I½	½	3		3		4		4	

successive occasions be more apt to fluctuate in recalling a sensation than it would after a short interval, and would consequently extend farther the bounds within which the comparative judgment reverses earlier decisions.

But the data of my experiment seem to give no support to this conclusion, there being virtually the same width to the zone of variable judgment for the two-second interval as for the two-minute.

The number out of the twelve variables that each subject puts in the range of error are—giving the average for the four different standards:

Two-minute interval,	D 7	;	Two-second interval,	D $6\frac{1}{2}$
" " "	B 8	;	" " "	B $8\frac{1}{2}$
" " "	S $4\frac{3}{4}$;	" " "	S 7

THE ILLUSORY UNDULATION AND SHIMMER OF STRAIGHT LINES.

BY PROF. GEORGE M. STRATTON.

It has long been known that when fine parallel lines are viewed from a distance they appear to be irregularly beaded and bent.¹ In 1858 Bergmann, in his account of the matter, tells us that because of the illusory distribution of light and dark many of his observers mistook the direction of such lines, giving them a trend at right angles to their actual direction. And he attributed the whole phenomenon to the structure of the retina, whose elements acting as "visual units" (*Seheinheiten*) introduced an impurity into the spatial perception of the lines.²

Helmholtz's well known discussion of the facts³ is in essential harmony with that of Bergmann. Helmholtz felt that the wavy or beaded appearance of the lines depended upon bringing them close together, and his measurement of the visual angle subtended by two such lines corresponds fairly well with the diameter of the elements of the fovea. From this he concludes that the fine distortions of the parallels are due to the mosaic structure of the retina, wherein each element acts more or less as a distinct unit.⁴

Objections to Helmholtz's view have been adduced by Von Fleischl. He showed⁵ that the arrangement which Helmholtz regarded as an essential condition of the beading or distortion—namely, that the lines should be so close together as to fall upon adjacent retinal elements—was of no importance whatever;

¹ This was noticed at least as early as 1819, by Purkinje. Cf. Helmholtz: *Physiol. Optik.* 2d Ed., p. 258.

² Bergmann: "Anatomisches und Physiologisches über die Netzhaut des Auges," *Zeitsch. f. rat. Medicin*, Series III, Vol. II, p. 83.

³ *Physiol. Optik.*, 2d. Ed., p. 257.

⁴ See also Hering, in Hermann's *Handbuch*, Vol. III, p. 153.

⁵ Von Fleischl: "Physiol.-opt. Notizen, II Mitth." *Sitzungsb. d. kais. Akad. d. Wiss. z. Wien*, Math.-Naturw. Cl., Vol. LXXXVI, pt. III (1882), p. 8.

the waves occur quite as well with wide lines at a considerable distance apart as with fine lines close together. The most favorable condition for their occurrence is with lines moving across the retina—the eye looking fixedly at an immovable point past which the lines are being continually carried by the revolution of a drum. Careful measurement of the length and height of these subjective waves showed that they were many times too large to be caused by the mosaic formation, to which Helmholtz had ascribed them. Von Fleischl moreover showed that the aberration in the lines could hardly be due to the network of capillaries in the retina. But beyond this negative criticism, he offers no confident account of the phenomenon, although he suggests that the large epithelial cells might, after a generous allowance for error in his own measurement of the undulations, be large enough to cause indirectly the distortion of the lines.

It seems possible to advance beyond the point where Von Fleischl's excellent observations leave us, and to that end some farther experiments are described, with an attempt to indicate their meaning.

A most significant fact in connection with the sinuous appearance of straight lines¹—and one that, so far as I know, has hitherto escaped observation—is that the undulation is not visible upon first exposing the rested eye to the lines. It makes its appearance only after some moments of gazing at them, and gradually increases until a maximum effect is attained, this maximum effect continuing thereafter indefinitely. It is difficult to give exact numbers to express the time when this effect begins, but in my own case there seems to be no appreciable waviness until 7 to 10 seconds have elapsed; and often the effect is not fully developed until after two or three times such a duration. These measurements were taken with a group of seven black parallels on white paper, the lines being about 0.5

¹ There is probably some variety in the exact character of this phenomenon with different observers; with some it seems best described as 'undulation,' with others as 'beading.' One of my most careful observers describes the gradually on-coming effect as similar to the annular 'knots' on bamboo. For convenience' sake, then, let any one term stand for all these variants.

mm. wide and 15 cm. long, with centers 2 mm. apart, and observed from a distance of slightly over a meter.

This delay in the on-coming of the undulations may in another way be brought out perhaps more strikingly. If one looks at a system of parallel lines like those just described, continuing his view of them until the appearance of waviness is well developed, and if then the whole system of lines be suddenly revolved about its own center and in its own plane through an angle of 20 or 30 degrees, the waviness is noticeably absent from the lines as they reach their new position, and they must again be viewed for some moments before it returns. A like suppression of the wavy or tortuous appearance may be produced if, instead of revolving the paper, the observer simply turns his head slightly to one side—turning it upon an axis made by the line of sight. But when one says that in changing the relation between the eye and the system of lines the waviness suddenly subsides, this statement applies with entire exactness only under a slight reservation. The waviness entirely disappears only from that portion of the field of view corresponding to the parts of the retina unstimulated by the lines in their former position, but stimulated by them in their later position. Where the lines in their new direction cross the territory occupied by them before, an uncertainty and disturbance of their definiteness is still apparent, undoubtedly as a vestige of their former effect.

There are other circumstances under which the phenomenon here discussed appears or disappears; and these ought to be recounted, as helping in some measure to our better understanding. The phenomenon seems to have no intimate connection with movement of the eyes. It occurs with a free wandering of the line of sight over the lines. And yet the roving of the eyes to an appreciable extent over the lines does not appear to be necessary to produce the waviness; the aberrations occur when the ocular fixation is as perfect as is practicable by merely voluntary control,¹—although, as we know from mech-

¹ Herein I am running counter to Von Fleischl's statement that the waves disappear with strict fixation (see p. 11 of his article cited above). Can it be that upon strict fixation he slightly moved his head? This, as I have pointed out, would suppress the appearance temporarily.

anical records of the eye's behavior, such fixation is always in a measure wandering and unsuccessful. The phenomenon, occurs, however, when the gaze is fixed to such a degree that a sharp after-image of the lines is obtained. And it is interesting to note that the nicking, the fine distortion, of the lines, is apparent in such an after-image—indeed there is some temptation to say that it is here even more pronounced than in the original perception. Such an observation is perhaps connected with the fact observed by Von Fleischl, that exact accommodation of the eye to the lines is not needed to bring out the waviness—which comes out perhaps more clearly with imperfect accommodation. I find, too, that if ground glass is placed before the lines, but just near enough to permit the sight of them though blurred, the undulatory effect is, if anything, heightened. Some slight blurring, then, is perhaps the vital thing in a number of conditions otherwise so different—where the waviness appears with the interposition of ground glass or with inexact accommodation or in the after-image of the lines. There is no strong reason to think that the blurring is potent only—as one might suppose—by enlarging the opportunity for false observation and for suggestion. The observed effect seems to be too definite and sensuous and unmistakable to be so explained.

The nicking occurs in foveal vision, as Von Fleischl noted in his experiments to determine whether the retinal capillaries were anywise responsible. But this foveal occurrence proves, too, that the waves have no exclusive connection with the retinal rods. Nor is it necessary to view more than a single line in order to produce the undulation. The distortion with but a single line is less striking than when other lines are close at hand; and a longer time seems needed for full development of the waviness. This may be partly because the neighboring lines by their close proximity make more evident the distortion; or to some extent there may here be a psychic summation-effect, the distortion in a number of lines seeming to be a greater distortion in each line, by a very natural associative confusion. But in all probability a number of lines viewed at once act as reinforcing stimulants upon the retina, a given set of retinal elements being more frequently and more rapidly aroused by a

group of fine lines than by a single line, since in the eye's inevitable wandering it tends to pass rapidly from one to another. Yet this oncoming of the undulations with but a single line may the more readily escape an observer because now the same part of the retina must be held with some degree of exactness to the immediate neighborhood of the line. A slight shift of the direction of sight so that the after-image of the line no longer hovers on the border of the line itself but becomes slightly separated from it, causes an instant straightening of the line, while the return of the eye to its old position again brings out at once the nicking.

No markedly different result is obtained when, instead of having a black line on a white ground, the relation is reversed and a white line, or group of lines, is drawn on a black surface. Nor can I notice any decided change in the phenomenon when the black lines are drawn, now on white and now on gray paper of different degrees of darkness. Taking (to be more explicit) the four gray papers of the Hering series of fifty, marked respectively Nos. 1, 15, 30, and 45, where as the reader will recall, No. 1 is white and No. 45 a dark gray that approaches black (No. 50), and drawing upon each of these the group of parallels described earlier, the lapse of time before the waviness appears, is given in the following table:

No. in Hering Series	Nicking appears in about	
No. 45	14	Secs.
" 30	10	"
" 15	12-13	"
" 1	7	"
" 1	8	"
" 15	9	"
" 30	10	"
" 45	7	"

After each experiment in the above series the eyes were rested for two minutes, save that at the close of the series there was a rest of half an hour before beginning the series recorded

in the table following, where, it will be seen, the beginning and close is with No. 1 (white) instead of with No. 45, as in the earlier series:

No. in Hering Series	Nicking appears in about	
No. 1	14	Secs
" 15	11	"
" 30	8-9	"
" 45	6-7	"
" 45	6	"
" 30	7	"
" 15	13	"
" 1	12	"

The experiment indicates no clear difference between the white and the dark gray; indicates that the position in the series is of more consequence than the character of the ground upon which the lines are drawn, and that in all probability the effect of the preceding trial frequently outlasts the two-minute pause and is carried over into the trial following.

With moderate adaptation of the eyes to darkness (10-12 minutes) the waviness was still observed in looking at fine parallel lines—parallels produced by diffused daylight coming through finely slitted black bristol board inserted in a closely fitting window screen of an otherwise darkened room. And when fairly pure red light came to the eye by placing red glass before the lines—or green light (which, however, was far from spectral purity)—the waviness of the lines seemed nowise affected by the change. The failure to notice any marked influence from the exclusion of rays not red or from adapting the eye to darkness would be in keeping with the observation that the linear undulations occur in the fovea as well as in the eccentric regions of the retina, and consequently have no intimate and exclusive connection with the rods.

If but a single eye be exposed to a set of lines until the undulations are markedly present and then this eye be covered while the other eye is exposed to the lines, there is at the moment of

transition no such vivacity of undulation to be observed as there would have been had this second eye been uncovered while the first eye was exposed. Indeed it is at times exceedingly difficult to notice any transfer of effect. If any result whatever is produced in the second eye, it seems at most to be but a reduction of the time required to produce in full measure the linear undulations there, a noticeable interval still being necessary before the waviness is clearly present.

Passing now from the observed facts to the question of their origin, it is evident that the explanations hitherto offered are far from satisfactory. The earlier account—that the distortion was due to the mosaic arrangement of the retinal elements—would seem already to have been disposed of by the critical experiments of Von Fleischl. Yet Von Fleischl, by suggesting that the undulations were caused by the pigment epithelial cells, because their size roughly corresponded to the size of the linear waves, seems likewise to have been looking in not exactly the right direction, for he too seeks his explanation in some disturbance caused by the fixed structure of the eye's tissues. The present experiments, however, by making clear the *gradual rise* of the undulation indicate, it would seem to me, that the phenomenon is not due to some unchanging condition of the eye such as would be found in its permanent anatomical arrangement for were this the case the undulations would be present as soon as the lines were seen. There must be, rather, some obscure functional change, gradually brought on by continued linear stimulation; and not until this change has reached a certain degree of development does it seriously affect the spatial character of the visual field.

In ascribing this peculiar linear aberration to some functional disturbance of vision, it seems to me that perhaps all has been said that is possible at present with security. Yet beyond this come intimations both negative and positive that may be looked at until the weight of farther evidence seals their fate.

The clearness and distinctness of the undulations seems to me to dismiss almost at once the suggestion that the sinuous appearance is a mere vagueness due to flagging attention. Not only have these waves a direct sensuous vividness, but they are

noticeable only with close attention, and can hardly be due to a failure of careful notice. Nor does it seem probable that these undulations are due to some minute and irregular change in the refractive media of the eye, although changes of such a character might well account for the fact to be explained. Nor is it at all likely that the linear stimulation produces any transitory displacement of the retinal end-organs, after the manner of those more lasting disturbances which—for example, in choroiditis—give a broken and irregular appearance to straight lines. It is, however, distantly possible that a persistent stimulation such as has been described in the present experiment might produce a gradual and slight change in the local signs arising from certain elements or groups of elements either in the retina or in the cortex, and thus there be an altered localization of the dominant visual impression to which the signs were attached.

But rather than give much weight to a suggestion of this sort, it would perhaps seem preferable to believe that the functional disturbance involved in the present illusory undulations was to be described as an irregular and shifting distribution of areas of inhibition and heightened sensitivity. The unusually persistent and teasing stimulus from the juxtaposed lights and darks may perhaps bring on, with time, a momentary local blindness over certain minute areas, and an increased sensitivity in immediately adjacent parts, and for this cause the sensory response would not coincide with the exact outline of the image. Such a shifting distribution of minute induced areas of heightened and depressed sensitivity would in some respects have kinship with the phenomena of fatigue and the passing blindness from long fixation, but it would also in many ways be distinct from either group of facts.

Thus far the account has been confined to the behavior of stationary lines, and we might now consider the appearance of moving lines while the eyes themselves are motionless. The lines used in the experiments to be described were straight and were upon paper fitted to a revolving drum and lay parallel to the axis of rotation. Any particular set of lines had a uniform width and distance apart; but in different sets the width

chosen ranged from about $\frac{3}{4}$ of a mm. to 15 mm., the distances from center to center varying from 1 cm. to 1.5 cm. In general the lines were black, drawn upon a white ground, although white lines upon black, and black lines upon red, were also used. But unless an explicit statement to the contrary is made, it will be understood that the experiment was with black upon white.

If, now, the eyes be fixed upon a point just in front of the drum while this moves at a speed of one revolution in about 4 to 6 seconds, there is at first no marked effect; but with longer stimulation the lines begin to show those irregularities that appear with motionless lines. It was Von Fleischl's opinion that the movement of the lines while the eyes were fixed upon a motionless point, offered an even more favorable condition for observing the waviness of the lines than did the arrangement where the lines were not in motion; and there seems little reason to doubt the correctness of his statement. There appears here, however, the same delay in the oncoming of the disturbance, it reaching no clearness and force until the stimulus has played upon the eyes a few seconds. But once the eye has been exposed to the stimulus for a length of time sufficient to produce a decided undulation, then even after a moderate rest the effect returns almost instantly upon reëxposing the eye to the moving lines.

A most convincing demonstration of the delay in the coming of the waviness is obtained if one seats himself before the revolving drum so that the eyes are at a distance, say, of 1 m., and, after holding the eyes upon the fixation point for 15-20 secs., suddenly bring them to about 20 cm. from the drum, the ocular fixation being carefully maintained. There is then seen a small central area upon the drum—corresponding to the retinal area already stimulated when the eyes were a meter away—where the nicking and bending appear with great vivacity, whilst round about this central area the lines are still straight and sober. The fresh portions of the retina, suddenly brought to bear upon the lines as the eyes come close to the drum, receive and report them without distortion and give a view in strong contrast with that afforded by the adjacent regions that have

long been impressed by the passing lines. And as with stationary lines, so with moving: a slight rotation of the head upon an axis represented by the line of sight causes an instantaneous reduction, if not an entire suppression, of the undulations, by giving the movement of the lines an entirely new direction relative to the eye. By slowly pendulating the head upon this axis, and thus preventing a continued stimulation in any single direction, the linear waviness can be warded off almost indefinitely. Whether such reduction is quite so great when care is taken that the second motion of the lines, to which transition is made, be the *exact reverse* of the first—by glancing into a mirror that suitably reflects the original,—I am uncertain. But in any event it would seem that the mere interruption of light upon a given point, or movement *überhaupt* over such a point, were not of itself the sole and sufficient exciting factor in this phenomenon; but that the *direction* of movement had its peculiar influence upon the sensitive surface, and that the special retinal effect of a motion in one direction could not be built upon by a motion in some other direction, but this must work up its own peculiar sensory effect *de novo*.

But when the lines are moving before the motionless eye, there is an odd appearance distinct from anything I have described so far. The lines not only appear nicked and bent, but there is also a quiver or shimmer over all the lines; they seem to progress with an odd vibratory movement, that is pronounced enough in observation but is exceedingly difficult to describe. The nearest analogue to it in ordinary experience is perhaps the rhythmic and sometimes disagreeable commotion that runs over the face of a paling as we pass by, when behind it there is a second paling parallel with the first and not too far removed. The passage of the one system of lines over the other, due to the shifting projection as we move, seems to set the whole into a flutter not unlike what is noticeable with motionless eyes before a single system of moving lines.

And this shimmer or flutter, wherein often there is a slight appearance as though the lines, in moving forward, also periodically leapt backward each to the one following on its heels, requires usually some time to develop. With lines about $\frac{3}{4}$ mm.

in width, 1 cm. apart, and the drum revolving once in $7\frac{1}{2}$ secs. (so that 51 lines pass before the eye each second) the shimmer or shiver is usually apparent to me after 7 or 8 secs., and in the course of the next 3 or 4 secs., has reached a considerable vivacity. The time required for the rise of this appearance of vibration seems therefore to be at least roughly the same as that required to produce the distortion of moving lines. And, moreover, like this distortion, the shimmer or flutter is very much decreased, if not entirely suppressed by turning the head upon an axis represented by the line of sight, or by having a mirror beside the drum, into which the observer may glance and see the reflected lines moving in a direction different from that of their originals, without, however, being an exact reversal.

With regard to the influence which the speed of movement might have upon this shimmer, some observations were made. The black lines or stripes used were 5 to 10 mm. wide, 15 mm. from center to center, 33 stripes passing before the eyes—distant 60 cm.—with each revolution of the drum. When the drum made one revolution in 127 secs. I perceive neither distortion nor flicker of the lines, even after looking at the drum for a length of time that with higher speeds would be ample to provoke both of these phenomena. Increasing the speed so that a revolution takes place in 37 secs., it is doubtful if there is any flicker so long as the eyes preserve strict fixation. At 22 secs. a revolution there is a dim suggestion of flicker, with the undulatory distortion perhaps slightly more clear than the flicker. At 10 secs. a revolution the flutter is less dubious, but is still so vague and dull that in all probability it would escape the attention of one not schooled to observe it. Speeding the drum successively to 6 secs., to 5 secs., and to 4 secs., there is noticeable a marked superiority in the 6-sec. speed over that of 10 secs. There is now a clear though delicate flutter of the lines, while the bending is now perhaps less clear than this flicker. As between the 6-sec. and the 5-sec. speed, there is no great difference in their power to induce our phenomena, which here seem to have reached their acme; for with a speed of 4 secs., while the flicker and bending are vivid and are conjoined with brilliant color-effects well-known to observers of such move-

ments, yet with this higher speed there is no clear gain of vividness,¹ and perhaps some loss. With a speed of 1 revolution in slightly under 2 secs. it is far less easy to observe either the flicker or the bending, and there is a less clear effect also from suddenly turning the head. Yet even at this perplexing rate the old phenomenon is still traceable as an appearance of something *beating up against* the actual motion of the lines.

Some interesting facts come to the surface if, instead of using the lines or strips upon the revolving drum, a more powerful stimulation be obtained by having the light of the sky come to the observer through a large slitted disk of black revolved before a bright window. With slits cut radially in this disk and lying nearer to its center than to its periphery, so that the eye is well shaded from the light of the sky except as it comes through the moving slits, a flutter of the lines is soon observed similar to that noticed with the drum. Possibly it arises sooner than with the stripes upon the drum, perhaps because of the greater violence of the stimulation, but I cannot convince myself that there is any instantaneous appearance of the flutter even with the bright light; some moments of stimulation seem always to elapse before the phenomenon is present in force. But what is peculiar to the appearance under these new conditions is, that the flutter takes on the character of a movement of translation easily distinguishable from the actual movement of the slits themselves. It is as though, along with the circling slits and mingled with them, there were a secondary system of vague flickering bands revolving about the same center as the slits and

¹ At other times this 4-sec. rate has seemed more nearly optimal. When the system of lines, however, is completely changed—lines $\frac{1}{2}$ mm. wide, with centers 2 mm. apart—the flutter is excellent with 1 rev. in 33 secs., and seems to have reached its maximum with a rate of about 1 rev. in 24 secs. This latter rate makes the images of the lines pass a given retinal point at time-intervals of about $\frac{1}{16}$ of a second; the favorable 4-sec. rate mentioned earlier would make the lines (since they were farther separated spatially) follow each other about $\frac{1}{4}$ of a second apart. It is not improbable, therefore, that the *time-interval* between lines is of far more importance for the vividness of the phenomenon than is mere *speed of movement*. In the experiments with the lines spatially closer together the favorable speed was only about $\frac{1}{4}$ of the speed favorable for the lines more widely spaced; but the time-intervals between successive lines were, in the two cases, much the same.

at the same distance from that center and in the same direction, but at a slower speed than theirs. This movement of the fluttering parts is best seen when the eye is directed steadily to the center of the disk, although it does not disappear when the eye is directed to a stationary point against the slits themselves. And the forward direction of the fluttering movement is, with the special arrangement I used, noticeable when the slits themselves progress at a comparatively slow rate. When the speed of the disk is gradually increased, the apparent circling motion of the flutter becomes slower (although the fluttering itself does not cease, it must be understood); and finally, with still greater increase of the disk's speed, it changes its direction entirely, and now runs *counter* to the actual motion of the disk. This secondary system of back-moving, shadow-like, fluttering stripes is totally unlike the appearance of an after-image of reversed motion; for the apparent motion is, in the first place, more rapid than what is noticed in the after-image, and moreover is evident *along with* the rapid forward-movement of the slits themselves. It is similar in appearance to the shadowy movements that pass across two palings with shifting projection, or over the spokes of a revolving wheel at times when seen against the spokes of a wheel beyond. In general character it is, I think, like stroboscopic movement, and what is perhaps its explanation will later be suggested.

Returning now to the revolving drum with its stripes of dark and light, there seems to be no marked change in the phenomenon if the black stripes be considerably increased in breadth and the white stripes be correspondingly diminished, or if the black stripes be fixed upon a red ground instead of upon white, or be viewed through blue or red glasses, or through a pin-hole iris. And I find no unmistakable evidence that the exposure of one of the eyes to the moving lines until the flutter is clearly present does or does not arouse a flutter in the moving lines to which the other eye is then exposed,—or at least help to arouse this flutter earlier than it would otherwise occur. Experiments at one time seem decisive that such a transfer of influence does to some extent take place, while again the observation is unsettling. The following 'timings' are illustrative of what is to be found here:

Exp. 1. In the left eye flicker develops in 9 secs., when there has been no immediately preceding exposure of either eye to the lines.

Exp. 2. In the left eye, flicker develops in 6 secs. immediately after exposing the right eye to the moving lines for 20 secs.

Exp. 3. In the left eye, flicker develops in 7 secs. with no preceding stimulation of the right eye.

Exp. 4 (after a considerable pause). In the right eye, flutter develops in 9 secs. without preceding stimulation of the left eye.

Exp. 5. In the right eye, flutter develops in 7 secs. immediately after exposing left eye for 20 secs.

Exp. 6. In right eye, flutter develops in 10 secs. immediately after exposing left eye for 20 secs.

The time of the beginning of the flutter is under these conditions particularly difficult to fix with confidence, and an error of 2 secs. could easily be made. But on the whole the 'transfer' seems to be slight, if it occurs at all, and thus there is no sure ground as yet for assigning for this phenomenon either a purely retinal or a purely cerebral process as its physiological correlate.

There are a number of well-known facts with which it would seem theoretically attractive to connect, if possible, the shimmer of lines here described. The appearance known as the 'fluttering heart' might, by its very name, seem kindred to that for which I have so often used the expression 'flutter.' McDougall¹ has shown that there are two distinct classes of facts commonly included under the one term 'fluttering heart,'—a class probably due to the peculiar action of the rods when the eye is adapted to twilight vision, and a class observed in daylight and with foveal vision and consequently to be explained in a different way—perhaps (as McDougall, following Schapring, holds) by the different refrangibility of the rays for red and blue, with which colors the phenomenon is best observed. It is evi-

¹ "Illusion of the 'Fluttering Heart' and the Visual Function of the Rods of the Retina," *British Journal of Psychology*, I, 428.

dent that the shimmer of moving lines in the present experiment does not belong close to the first kind of fluttering heart, for it does not require adaptation to darkness nor paracentric retinal stimulation. And while there may be some connection with the other variety of fluttering heart—where there is a curious slipping of patches moved in daylight-vision, a slipping that occurs both within and without the foveal area of the field—yet this connection can seem reasonable only when we assume that McDougall's and Schapring's explanation is wrong and that this second variety of fluttering heart is *not* due to any such difference of refrangibility as has just been mentioned. For the shimmer of lines in the present experiment takes place strikingly well with stripes of black and white, where a difference of refrangibility of rays can play no important rôle. But since even the second class of phenomena of the fluttering heart can also be observed with colorless light in daylight-vision,¹ and therefore seem not entirely dependent on the use of colors of different refrangibility, some kind of kinship here is not excluded.

There is perhaps some intimacy between the flutter of moving lines and the appearance of slow reversal that continues for a time in vision after moving lines have long been regarded—the well-known after-image of motion. The great difference in the two kinds of phenomena, so far as their mere primary look is concerned, encourages caution in affirming any kinship. And this caution is further encouraged by the fact that it is possible to produce the after-image of motion without first arousing any noticeable flutter or undulation in the moving lines that provoke the after-image. Thus with very slow movements of the drum—with a speed, e.g., of one revolution in 37 secs.—I may be utterly in doubt as to the presence of any flicker in the moving lines, so long as my eyes themselves remain immovable. Nevertheless, upon stopping the drum, there is a patent and beautiful illusion of reversal. And even with a still slower motion of the drum—a revolution in 127 secs.—I perceive no flutter or bending of the lines; yet upon stopping the

¹ Cf. Sanford: *Course in Experim. Psychol.* I, 318 ff.

drum there is seen the old viscid flow of the lines contrary to their original motion.

Yet I am not sure that this should deter us from thinking that the moving lines, by their continued excitation of the retina, gradually call forth some process which, in its first intention and while the moving lines are still playing upon the eye, gives them a shimmering and sinuous look, but which later and perhaps in the presence of some obscure restorative process in the retina or in the cerebrum, when the lines have ceased their actual motion, makes them appear to move slowly backward. Some incentive to entertain such a belief is found in this: that if the eye be stimulated by the moving lines during short and controllable intervals of time, there is something like equality between the time of stimulation needed to induce the flutter and the time of stimulation needed to insure an after-image of motion. For example, after the drum has attained a speed of one revolution in $6\frac{1}{2}$ or 7 secs., I expose the eyes to the lines (black stripes 5 mm. wide, white stripes 10 mm. wide, 33 of each to the circumference of the drum which is 40 cm. from the eyes) for 1 sec., and stop the drum as nearly instantaneously as I can. During the exposure itself I notice no flutter of the lines nor bending, neither is there apparent any after-image of motion when the drum is stopped. After a rest of 5 minutes I repeat the experiment, now exposing the eye for 2 secs., and again there is no noticeable effect either during or after the movement. With a like rest each time, the exposure is lengthened successively by steps of one or two seconds, and the observations noted:

1 sec. exposure.	No flutter noticed.	No after-image of motion.
2 secs. "	No flutter noticed.	A mere ghost of after-motion (?) in excentric vision.
3 secs. "	Perhaps a slight suggestion of flutter.	Possibly a shade of after-motion in certain outlying parts of field.
4 secs. "	Slightest flutter in certain outlying parts of field.	Slightest after-image of motion in outlying regions.

5 secs. exposure	Clear, though slight, flutter. (The flutter and the after-image are clearest in approximately the same regions of the field.)	Clear, though slight, after-image of motion.
6 secs. "	Flutter clearer.	After-image clearer.
7 secs. "	Flutter clear, though perhaps no more so than with 6 secs.	After-image clear, though perhaps no more so than with 6 secs.
8 secs. "	Vivid flutter.	After-image excellent, though not so long-continued as with longer stimulations.
9 secs. "	Flutter fully developed, it would seem.	After-image excellent, but apparently confined to outer portions of field, and soon over.
10 secs. "	No noticeable difference from effect with 9 secs. exposure; flutter clearly spread over <i>both</i> center and periphery of vision.	No noticeable difference from after-image of 9-sec. exposure. After-image seems to avoid a considerable central area.
12 secs. "	General commotion; greater than before?	No noticeable increase in after-image.
14 secs. "	No noticeable difference in flutter.	No noticeable difference in after-image.

There would thus seem to be a certain similiarity in the external conditions that arouse the flutter and the after-image: they require approximately the same time for their appearance, and with longer stimulation run a fairly parallel course, reaching in times that are not far different their culmination. Yet the sense of their connection is somewhat weakened by the fact that while they make their initial appearance in approximately the same outlying parts of the field, yet the flutter seems more

ready to encroach upon the central region of the field than does the after-image; and too, by the fact already mentioned that with long-continued stimulation by exceedingly *slow-moving* lines an after-image is noticeable where no sign of flutter appears. Moreover it seems easier to reach conviction that there is at least a partial transfer of effect from one eye to the other in the case of the after-image of motion¹ than in the case of the flutter. If there is any connection at all, then, between the two orders of phenomena—other than that they may arise from the same stimulus and in the same organs—it is still obscure and calls for farther study before one can be sure even of its existence.

The evidence seems more conclusive that a close connection exists between the flutter apparent in a system of moving lines and the peculiar after-effect of a moving light noticed by Bidwell, Charpentier and others, and subjected more recently to careful study by McDougall.² Such an after-effect of a single bright moving light in my own case presents an appearance like that of a vaguely barred banner of light trailing behind the original impression. And this barred banner does of itself, too, suggest a certain flutter, doubtless because its extreme end is rapidly alternating between phases of dimmer and brighter intensities, and the fold-like series of lights and darks seems to pass over its surface, as in a flag rippled by the wind. But this is noticed only with a comparatively bright light, and not with any such mild stimulus as is given by a dark line upon white paper in moderate illumination. Yet it is highly probable that every patch of brightness, however faint, does when traveling across the retina, leave such an after-effect, either as a low and almost liminal banded sensation, or (if still weaker) as a mere trail of varying *sensitivity*, corresponding to the trail of varying *sensation* with stronger stimulus, but not actually developed into sensation.

Now in either case—whether the trailing after-effect of a

¹ Cf. Von Szily: "Bewegungsnachbild und Bewegungskontrast," *Zeitschrift f. Psychol. u. Physiol. d. Sinnesorg.* Vol. 38, p. 81.

² "Sensations Excited by a Single Momentary Stimulation of the Eye," *British Journal of Psychol.*, I, 78; Cf. also, Holt in *Psych. Bulletin*, II, 54, and Nagel: *Handbuch der Physiologie*, III, 222.

weak passing stimulus be a banded after-sensation, too faint and fugitive to be noticed, or be a banded region of contrasted sensitivity—a second weak stimulus in traversing territory so affected would find itself subject to a rapid alternation of reinforcement and inhibition. When the image of a white band falls upon a portion of the retina that (by reason of the stimulation from the preceding white band) is already giving forth a sensation of light or is predisposed to give such a sensation; there is a summation and the brightness is increased; and when the stimulus leaves this zone and passes into one that momentarily is in an opposite phase, there is a subtraction from the proper effect of the stimulus. And thus each line or stripe of the external stimulus used in the present experiment ripples through the wake of its predecessor, and there arises consequently that appearance of flutter over the whole system of lines, so often referred to in this paper.

This hypothesis—that the flutter in a system of moving lines is due to the periodic or vibratory interference between each line of the moving stimulus and the banded after-effect of the immediately preceding line—finds some difficulty in the fact already stated, that the linear shimmer or flutter requires an appreciable time for its development. The peculiar after-effect of a moving stimulus, however, is not known to arise gradually or to require time and something like coaxing, and therefore, the succeeding stimulus (we might expect) would at once find all the conditions needed to produce at once and without delay an appearance of vibration or of shimmer, if this shimmer were actually dependent upon an after-effect like that in Charpentier's phenomenon.

This difficulty is perhaps not fatal. Further observation may disclose the fact that the phenomenon of Charpentier is one whose vivacity increases with successive passage of the stimulus across the same retinal area. My own observations *ad hoc* are indecisive; for while it is true that I do not notice the characteristic banded appearance of a traveling slit until several revolutions of the disk have been completed, yet (since practice is of great importance here) the failure may be due to a certain initial confusion of the attention rather than to a delay

in the appearance of the phenomenon itself. Or even if with the stimulation usually employed in observing Charpentier's phenomenon this effect were to appear at once, this need not mean that with the stimulation afforded by black or white the banded after-effect must reach its acme upon the first passage of the stimulus across a given retinal area. It would be in accordance with other physiological facts if the first after-effect of one stimulation prepared the way for a still stronger after-effect of an immediately following stimulation—somewhat as the excitation of a muscle-nerve preparation not only produces a contraction but also brings on a state of heightened excitability, so that a second stimulation coming in proper relation to the first produces a greater effect than were otherwise possible.¹

That there is here an interplay of two systems—the images of the actual lines or stripes themselves crossing a retinal region already stirred into minute bands or stripes of excitement and inhibition or of contrasted degrees of excitability—this seems more reasonable when we remember the rate of excitation found to be most favorable for inducing the flutter of moving lines. It will be recalled that this flutter was found to reach its maximum when the lines followed each other at intervals of about $\frac{1}{8}$ to $\frac{1}{10}$ of a second;² and this should be compared with the times discovered for the after-pulsations of a single stimulation.

It is evident that the rate at which these pulses occur is various, differing with the strength of the original stimulation, and changing even in a single series of pulses as we go farther away from the initial pulse. With the lower intensities of stimulation by motionless light McDougall attempts no exact report of the time intervals involved, but finds higher up in the series, and with extra-foveal vision, four pulsations in about $\frac{1}{3}$ of a second, and elsewhere estimates that at the close of a series of pulsations from a light of moderate brightness, the rate is about 10 per second, and judges Young's estimate— $\frac{1}{4}$ sec. between the first and second pulse—to be too high.³ In his experiments with

¹ Cf. Wundt: *Physiologische Psychol.*, 5th Ed. I, 63 f.

² See p. 73.

³ McDougall: *British Journal of Psychology*, I, 85 ff. While the problem discussed above is not the same as that of plotting the curve of the varying inten-

moving lights McDougall found the entire series of primary pulsations to last about $\frac{2}{3}$ of a second, in which time a varying number of pulsations occurred, running at times as high as 18, according to one of his drawings.¹

Now the rate of movement at which the flutter is found to take place is such as to bring each successive line close enough upon its predecessor to come within its still-active 'wake,' as disclosed by these experiments of McDougall and others. When the lines crossed a given point of the retina at time-intervals of about 1 sec. or more, I was quite unable to be sure that any flutter occurred; while at intervals less than this the flutter gradually became evident—in barest suggestion when the time-interval between lines was $\frac{2}{3}$ of a second; clear at $\frac{1}{2}$ of a second; and clearest at intervals that, under varying circumstances, stretched from $\frac{1}{6}$ to $\frac{1}{10}$ of a second. Since the essential thing would seem to be, that each succeeding line reach the point occupied by its predecessor before the primary pulsating after-effect of that predecessor shall have died away (but hardly that it shall of necessity overtake the very *first* pulse of this series) the times here found seem to meet these conditions and to support the explanation I have suggested.

And this explanation would also cover those special and more occasional appearances—as of shadow-like movements across the face of the system of lines—taking a direction either with or against the motion of the lines themselves.² If the present account is correct these shadow-like movements are virtually stroboscopic in character, and are due to the peculiar relation

sity of a sensation arising with different durations of the stimulus, yet it is interesting to note that in this latter case, too, a pulsation of the wave of intensity is found whose first crest may come anywhere between $\frac{1}{6}$ and $\frac{1}{10}$ of a second after the sensation begins, and may be followed by crests at varying later intervals. (Büchner: "Ueber das Ansteigen der Helligkeitserregung," Wundt's *Psychol. Studien* (1906), II, 1; Cf. Berliner: "Der Anstieg der reinen Farben-erregung im Sehorgan," Wundt's *Psychol. Studien* (1907), III, pts. 2 and 3. It would be important if the rate of pulsation in my own experiments and in these others that seem in many ways kindred should be found to be perhaps connected with the 'refractory period' of the cortical cell, which, too, is about $\frac{1}{10}$ of a second (Schäfer: *Physiology*, II, 614 f).)

¹ See Plate I, Fig. 6, of McDougall's article just cited.

² See p. 80.

of the speed of the lines to the rate of the pulsing after effect. Thus the chief pulsation that intensifies a line as it approaches a given area may find this line occupying a position that is ahead of, or behind, or identical with, the position of its immediate predecessor when it underwent its flutter in that general region. And since this successive advance or retreat is not confined to any single line but is common to them all, there would thus occur at times, in addition to the mere flutter, a wraith-like progress or regression of the entire fluttering system.

SOME EXPERIMENTS ON THE PERCEPTION OF THE MOVEMENT, COLOR, AND DIRECTION OF LIGHTS, WITH SPECIAL REFERENCE TO RAILWAY SIGNALING.

BY PROF. GEORGE M. STRATTON.

The character of the night signals upon our railways has been discussed by the present writer in papers¹ wherein there was offered certain facts of psychology supported by evidence from sources that, it seemed to me, would be regarded as reliable by railway men. There will be no attempt here to repeat all that has thus been published, but since a number of the statements were based upon psychological experiments that could not be fully described without defeating in a large measure the purpose of the earlier papers, it has seemed well to set forth the facts here, so that anyone interested in following the matter may have them in clearer form.

¹ "Railway Disasters at Night," *Century Magazine*, May, 1907, Vol. LXXIV, pp. 118 ff.; "Railway Signals," *Science*, Aug. 23, 1907, N. S. Vol. XXVI, pp. 255 f.; "Railway Accidents and the Color Sense," *Popular Science Monthly*, Mch., 1908, Vol. LXXII, pp. 244 ff. It ought perhaps to be said that in preparing these articles assistance was obtained from two prominent signal engineers, who would doubtless prefer not to be thanked publicly; and use was made besides of such sources as the *Proceedings of the Railway Signal Association*, the *Railway Age*, the *Railway Gazette*, the *Annual Reports of the Board of Railroad Commissioners of Massachusetts*, and a number of other publications of like character. The *Accident Bulletins of the Interstate Commerce Commission* were in constant use, and I am indebted besides to the special assistance of the Secretary of the Commission, Mr. Mosely, and to the Statistician, Mr. Adams. The Pennsylvania Railroad, through Mr. Atterbury, the General Superintendent, very courteously permitted me to make observations from express engines both by day and by night. The experiments here reported would, besides, have been impossible without the kind coöperation of Dr. Dunlap, Dr. Burrow, Dr. Furry, Mr. Hershey, Mr. Barrett and Mr. Williams, who gave their time to tedious observations. Other obligations I shall have occasion to acknowledge in the course of the present paper.

I. As to the Perception of Stationary and Moving Lights.

In considering the possibility of improving the present signals, one readily thinks of using *movements* in the visual field, and especially of using the distinction between movement and rest. And this seems the more inviting since it is a mode of signaling already long resorted to in plain life, as when there is the wish to attract the attention of someone at a distance that cannot well be covered by voice, or when sound for any other reason would be ineffectual. The hand-lantern swung by the trainman shows a ready appeal to this form of signal. And moreover the behavior of animals, many of which are singularly responsive to movements in the visual field (being easily alarmed by moving objects, while utterly negligent of motionless things), seems to give a kind of biological warrant for high hope regarding movements as signs of danger.

Experiments were undertaken to discover how far this hope is justified with respect to very faint lights—to discover whether or in what degree, such lights when in motion are visible at an intensity below that needed for the perception of motionless lights.

In a dark room there was arranged a pendulum provided with a very light horizontal cross-rod running out from a point close to the center of motion. Upon this rod, and at a distance of about 35 cm. from the center of motion, there was carried a small circular white disk, $13\frac{1}{2}$ mm. in diameter, which, upon swinging the pendulum, moved up and down. This vertical movement of the disk was about 10 cm., and through this stretch it oscillated at a rate of 128 single vibrations per minute. The pendulating disk was illuminated from the front by a miniature electric lamp connected with a storage battery—a lamp enclosed in a small opaque chamber screened from the observer and provided with an opening toward the disk. White tissue paper covered this aperture of the chamber, cutting down and suitably diffusing the illumination; while the variation and control of the light, in order to discover the threshold of perception, was effected by an episkotister driven electrically. To give the observer the general direction in which he was to look for

the disk in the dark, there was set at each side of the disk, and distant from it about 8 cm., a solitary phosphorescent spot, immovable and dimly seen when once the eyes were partially accommodated to the darkness. The observer, whose eyes were a meter from the disk—a distance preserved by means of a Hering head-rest—remained in the dark room for at least ten minutes before the beginning of the day's experiments. Such a preliminary stay was also required in all subsequent experiments.

Since the aim, as I have said, was to determine whether a moving object could be seen at a degree of illumination less or greater than that needed to disclose this same object when stationary, the disk was at rest in certain series and in other series oscillating, and measurement was taken of the relative intensity of the light necessary for the two kinds of perception. The series involving motion were alternated in a given hour with those involving rest; and from day to day the order of these different series was changed to reduce the error that might

SERIES WITH MOVING DISK.		SERIES WITH STATIONARY DISK.	
Opening of epis- kotister, in degrees.	Observation.	Opening of epis- kotister, in degrees.	Observation.
10	'Nothing'	10	'Nothing'
20	"	20	'Some intimation, but did not last.'
30	"	30	'Seen for a moment, motionless.'
40	'Mere suggestion of movement.'	40	'Seen.'
50	'Seen.'	50	"
60	"	60	"
60	"	60	"
50	"	50	"
40	'Very indistinct.'	40	"
30	'Nothing.'	30	"
20	"	20	'Nothing.'
		10	"

arise from progressive adaptation in the course of the hour. The observer was left free to regard the disk with foveal or with extra-foveal vision, in the natural manner in which a signal would have to be observed. The table above illustrates the general procedure, and the character of the judgments passed.

The thresholds, expressed in degrees of angular opening of the episkotister, and set down in the order in which they were obtained for any one subject, are given in the following table:

Observer.	Thresholds for moving light.	Thresholds for stationary light.
W	30.	20.
	30.	20.
	50.	20.
	10.	10.
	10.	5.
	5.	15.
	15.	10.
	10.	5.
	5.	10.
	10.	5.
	Average: 17.5	Average: 12.
F	80.	20.
	70.	30.
	40.	20.
	40.	30.
	20.	20.
	20.	20.
	15.	5.
	10.	5.
	15.	10.
	15.	10.
	10.	10.
	10.	10.
	Average: 28.8	Average: 15.8

The table indicates some advantage, on the whole, for the motionless light; it was perceived at a degree of illumination

at which, on the average, the moving light was still invisible. But in the single determinations the motionless light is by no means universally favored; for observer W the *lowest* determinations for the one kind of perception are no lower than for the other, while with observer F the minimal thresholds are on the side of the stationary light. What difference there is thus leans to the side of the moveless stimulation. And in this very fact there is some surprise. The expectation won from the observation of strong lights, which in movement are so attractive to the attention, is not met by these weakest ones. We must say, therefore, that the advantage of moving lights is probably confined to those regions of intensity that are well above the threshold—where the sensation already has some *pou sto* from which to move the attention; their advantage does not affect the threshold of perception itself, the absolute threshold of sensation. Just how far the advantage possessed by the higher intensities is an offset to the want of such advantage, or even to the actual disadvantage, in the lower intensities, remains yet to be determined.

As to the cause of the higher absolute threshold for moving lights, it is probably connected with the wide fact, already recognized, that the threshold of sensation depends not alone on the absolute intensity of the stimulus, but also upon the duration and the area of its operation. Now in the present case the matter of area hardly enters, for at any instant the area of actual stimulation is the same in the two cases. But the motionless light plays upon any given retinal element a longer time than does the moving light, and thus supplements its own physical intensity by this increased duration. Accordingly the weaker intensity is enabled to attain a luminal value when the stimulus is stationary; but when moving, a greater strength is necessary to make good the loss due to the mere brevity of the stimulation upon any given retinal element.

II. *As to the Perception of Signal Colors.*

With regard to colored lights, the aim of the experiments was to discover the point at which the color used as a sign of danger

ceased to be perceptible, and to compare this with the corresponding point for the yellow light commonly called 'white,' that on many roads is used as a sign of safety. And since other roads use green as a sign of safety instead of using it as a caution signal, the color green was also to some extent included in the experiments. But particularly the purpose was to determine the advantage or disadvantage in the use of the present danger-signal, as compared with white, when for any reason the light is reduced to a very low intensity—by the distance, or the lamp's low burning, or by smoke, dust, snow, or fog. As with the moving and motionless lights, it was an experiment on the absolute threshold of sensitivity, and left out of account entirely the relative attractiveness for the attention of the impressions when once they are strong.

It must be remembered that in actual railway practice the effect of red as compared with white depends not merely on physiological and psychological differences, but on physical. In the red signal there is employed the same flame or other source of light as for white, and the same intensity, together with the same devices for reflection and for concentration of the beam. But for producing a red light, especially in the block system, there is ordinarily passed before the lantern at the proper time a red disk of glass, called a 'roundel,' that absorbs a large portion of the light emitted by the flame for the white signal, and permits the passage only of those rays that, either of themselves or in combination with other rays, give a strong impression of red. But this absorption greatly reduces the intensity of the light, so that the red signal thus at the very start is physically but about $\frac{1}{3}$ to $\frac{1}{4}$ as bright as the light that constitutes the white signal.¹ If one is to drive directly at the problem with which signaling is concerned, rather than at the one which would be of greater interest to abstract psychology, this physical difference must be included among the conditions of the experiment, and not excluded by first producing a red and a white whose physical intensities were as nearly as possible equal.

¹ See Churchill: "The Roundel Problem," presented at the Ninth Annual Meeting of the Railway Signal Association, 1905.

The experiments were conducted both within and without the laboratory, over long and short ranges, with light reflected from disks (as in the preceding experiment), and with light coming direct from lamp to eye. The colors, in all save the very first and preliminary experiments, were obtained by sending a beam of light through standard signal glasses used in the railway service.¹ The limit of perceptibility was found in several ways. In some of the preliminary experiments the miniature lamp with its windowed encasement already described was used to illuminate a small white disk that served as the object of observation. The distance between disk and lamp was varied until, with red and with white light, the just-perceptible intensity was reached. But more commonly the thresholds were found by placing the lamp and its red glass at such a distance from the disk as to make this just visible; and then, by means of an episkotister, after removing the red glass, the far lower intensity of the lamp was found at which the disk, under this 'white' light, could just be seen. But this procedure, for all its convenience, seemed to imitate too imperfectly the conditions of actual service, when the object seen is usually a small area of light having a visual angle far less than that of my disk, and moreover is not a reflecting surface but a beam coming direct from lamp to eye; so the apparatus was rearranged to produce this effect. By means of a system of dispersion lenses there was now obtained from an oil-lamp properly screened a point of light of an intensity so reduced that when the red roundel was placed before the lamp this point of light was barely visible. Then for comparison the roundel was removed; the (now 'white') light was, by means of an episkotister, reduced below the point of visibility; and the threshold was approached and crossed by gradual increase. In still other experiments the dispersion lenses were set aside and an opaque screen with an aperture of 0.75 mm. (0.03 in.) was placed before the sema-

¹ For these glasses I am indebted to the Corning Glass Works, and especially to the vice-president of the company, Mr. Houghton. The use of a railway semaphore-lantern with a special attachment permitting an easy interchange of roundels is due to the courtesy of the Adams and Westlake Company, and more particularly of Mr. Langworthy of their Eastern office.

phore lantern provided with a 'slow-burning' oil-lamp, prism reflector, and white glass lens. This aperture when viewed from a distance of 18 ft. was about equivalent, so far as the area of retinal stimulation is concerned, to the illumination from a signal-lantern having a 5 in. lens-opening observed from a distance of nearly 3000 ft.

In these experiments it soon became evident that the threshold for white and the threshold for red, bore to each other a relation far different from what we should expect from our knowledge of the physical relations of the two lights. Physically the presence of the red roundel of the style I used cut down the light to about $\frac{1}{8}$ of the intensity of the light when no such roundel intervened. From this cause alone, then, we should expect in reaching the threshold to have to cut down the white light by means of an episkotister or other device, to about $\frac{1}{8}$ of that intensity of light which, behind the red roundel, was just perceptible. In fact, however, the white light had to be cut down until the ratio between it and the light just seen through the red roundel was as follows:

Observer F. 1:36

S. 1:24

B. 1:18

These determinations were made within the laboratory and when the light was viewed direct. They do not indicate any thing other than the most favorable observations under the conditions; for with all the disparity between white and red, which these ratios indicate, there came times when the disparity was still greater: with one observer the red light was still invisible when the episkotister was opened 60 times more than was necessary for the white; with another observer an opening 70 times greater than for the white still failed to make the red perceptible. When the object to be observed was not the point of light viewed direct, but was the small white paper disk reflecting the light, Observer F did not see the disk under red illumination until the distance between light and disk indicated an intensity (neglecting the physical absorption

by the red roundel) 70 times greater than that needed for the white light.

A few trials were made at night in the open, the observer being distant about 4200 feet from the light. The air was more or less smoky from a neighboring railroad, thus supplying still more nearly the conditions frequent in actual service. This smoke of course affected the red light far less than it did the white, since smoke is less hostile to the passage of red rays than to those of other colors, and in consequence cuts off a greater proportion of the white light's full intensity. Under these circumstances the ratio of the lights just visible was, for myself 1:14—a reduction of the second term of the ratio below the lowest point found for any of the observers in the laboratory experiments. And yet this means that under conditions of atmosphere that tend to equalize the two kinds of light, the red signal disappeared at an intensity which, upon removal of the red roundel, was 14 times brighter than was called for to render visible the safety signal.

If, instead of white, green be used to indicate safety, this both does and does not materially change the relation between the danger signal and the safety signal in regard to their comparative perceptibility. With a brightness of light that at 10° opening of the episkotister the common warm light of the oil flame could be recognized as 'ordinary light' by one of my observers, the interposition of a green roundel did not prevent his having a sensation of light when the episkotister was open 15° , but the light seemed colorless; at 180° opening it was judged to be 'bluish green,' but again at 360° opening it was called 'bluish white,' though still faint. With another subject who at 20° saw the white light when no roundel was interposed, a 50° -opening of the episkotister made him see the light merely as 'light' when coming through the green roundel, while at 80° it still seemed mere 'light;' and even at an opening of 360° it appeared to him only a 'grayish blue.' Under the same circumstances, when a red roundel was substituted for the green, an opening of 360° gave an impression of red distinctly for the one observer, and doubtfully for the other. It is therefore clear that the relation of the red to the green signal can be stated in

two different ways, according as we consider the green in its chromatic or in its achromatic character. Considered in the latter way, the contrast between the perceptibility of the danger signal and of the safety still remains large; but considered in the former way—requiring the green to be recognized as *green*—the inequality between danger and safety almost, if not fully, disappears; indeed it may at times even be transformed into an inequality where the red is superior.

For practical purposes of signaling the green must be recognized in its true chromatic character, and therefore its use to indicate safety tends to reduce or cancel the relative disadvantage of the danger signal. But this simply means that *both* signals are now liable to fail from too low intensity, whereas before only one was peculiarly apt to disappear. The red itself however, has not thereby been made essentially more effective nor its inherent disadvantages overcome.

From our familiarity with the phenomenon of Purkinje, such a relation between red and green and white is not surprising. Yet a more concrete grasp of the facts as they appear under the conditions of color-production in the railway service does give its own stir and shock, and ought in the end to contribute to a better understanding and advance of practical signaling.

III. As to the Perception of the Luminosity and Direction of Lines.

The chief coadjutor of color for signal purposes is direction. By day the enginemen on most roads that use the block system are forewarned of the condition of that portion of the track they are about to enter, by the position of an arm or vane above or beside the way—pointing horizontal or vertical or at an angle somewhere between. Bearing in mind this common use of direction by day, the more distant question with which the present experiments were concerned was this: Would it be feasible to use by night a line of lights that by its direction of pointing would give different indications, in much the same way as that by which the present day-signals convey their meaning?

And toward this end two more immediate and particular queries were propounded:

1. How would the intensity at which a row of lights is perceptible in its proper direction compare with the intensity at which a single light in that row can just be seen?

2. Of what length must such a row of lights be to insure perception; and how does this minimum length compare with the distance from which the row is to be observed?

The first of these questions amounts to this: Would a signal given by the direction of several lights in a row be readily observed? Would its indication be more easily lost in fog or smoke or snow than is that of a single light, such as the present white safety signal, when of an intensity equal to one of the members of the row? The experiment was thus parallel in some measure to that by which the just-perceptible intensity of movement and rest and of the more prominent signal-colors had been studied.

A row of seven white disks, each $13\frac{1}{2}$ mm. in diameter, with centers 15 mm. apart, were mounted on black bristol and so arranged that their line might be at will either horizontal or vertical, and also that six of the disks might be covered, leaving only one exposed. The illumination was by means of the hooded miniature incandescent lamp described above, and the threshold was determined by minimal changes produced by an episkotister. The observer was about 2 m. from the disks, and was left free to look at them without constraint to purely foveal vision; the experiment was conducted in a dark room, and no observations were begun until after at least 10 minutes had elapsed for accommodation.

Thresholds, expressed in degrees of angular opening of the episkotister, were found as follows:

Subject.	Threshold for single disk.	Threshold for direction of series of disks.
S	60	20
	40	10
	Av. 50	Av. 15
F	35	15
	25	20
	10	10
	10	5
	40	25
	40	15
	Av. 27	Av. 15

For each of the subjects the perception of the row of lights and the detection of their proper direction was thus attained at an intensity of illumination far below that necessary for the perception of but a single one of the disks. The comparison of the thresholds indicates that such a series of lights has an advantage over the single light, of about 2 or 3 to 1.

The explanation of this advantage of the series of lights is doubtless to be found in two well-known principles of vision. In the first place the observation of the row is far less likely to be made by foveal vision exclusively than is that of the single spot; and any extra-foveal perception, under the conditions of the experiment, would of course permit a sensation to arise from a lower intensity of stimulation. In the second place the larger area of the retina stimulated, even regardless of any inherent differences of sensitivity, would help to reduce the threshold; since, other things being equal, a larger expanse of stimulus gives a more intense sensation. Moreover in the present observations the threshold for perceiving the *direction* of the line or row was not clearly separate—as might have been expected from observations in other fields¹—from the threshold for the

¹ For a single example out of many, see "Ueber die Wahrnehmung von Druckänderungen," *Philos. Studien*, XII, 538, where the *direction* of change has a higher threshold than mere change itself.

perception of mere brightness; the one seems to have been noticed almost if not quite as soon as the other. Since, then, our perception of the direction of a luminous band runs relatively so low, it is clear that a night-signal composed of a row of lights would have a far better chance of perception than has a single light of like brilliance. It would be possible to see a row of lights and to be certain of its true direction under conditions that would make a single light too faint to be seen at all—conditions that often come by reason of fog or the smoke of passing trains.

Passing now to the second question, as to the *length* of line needed for the clear perception of direction, it can readily be understood that this is of moment for practical signaling, since with the high speed of our fastest trains the meaning of the signal must be caught from a considerable distance—a distance sufficient to permit the engineman to bring his train to a stop, if necessary, at the signal post. Would the required length be so great that from practical considerations it would be inconvenient or even impossible to use a linear signal instead of the present chromatic signal by night?

The first attempts to answer the question were made crudely with a row of pin-pricks in an opaque circular screen set close upon the lens of a semaphore lantern, the direction of the row being variable simply by turning the circular screen. Under these conditions an observer might, with 4 pin-holes, each a millimeter from its neighbors, measuring from centers (so that the row had an extreme length of about 3 mm.), still detect the direction of the row when his distance from the screen was increased to 3 meters. With a similar row of points, but each point now 2 mm. from its neighbors (so that the row was now twice as long as before) the observer might, though with difficulty, give proper answer as to the direction of the row of points when his distance from the screen was as much as 8 meters.

The experiment was then tried *sub Jove*, in order to have more nearly the conditions of actual railway service. Stations upon opposite sides of a ravine or narrowish valley could conveniently be used, where the distance between the stations was found by triangulation to be approximately 4200 feet. Below

the air-line connecting the stations and along the floor of the ravine ran a railway of heavy and frequent trains that generously burdened the air with smoke. The lights to be observed were incandescent bulbs of 8 c.p., set in a row along a wooden vane, with a distance of 10 inches between the centers of any two neighboring bulbs. Six of these lights were on the vane, bringing the extreme length of their line to 50 inches.

The vane was held in either a horizontal or vertical or oblique direction in irregular sequence until a series containing each of these directions either 3 or 4 times had been completed. Another series was then carried out, with a different length of line, after the manner once described as the Method of Serial Groups.¹ For simply decreasing the length of the row one of the end lights was cut out; at other times, as will be indicated in several of the tables, intermediate lights were also omitted; both the length and the inner constitution of the series will be clear, therefore, if the lights be numbered consecutively from 1 to 6, and the serial numbers of the lights composing the series be actually given: 'Nos. 1, 3, 4, 6' will thus denote a row 50 inches long, with the second and the fifth lights of the series omitted. The judgment of the observer, after each display of the line of lights, was signaled back by means of a code of lantern flashes to the conductor of the experiment, and by him recorded. The following tables give the results, wherein the letters v, h, ob, and d stand respectively for 'vertical,' 'horizontal,' 'oblique' and 'doubtful.'

¹ *Psychological Review*, IX, 444.

OBSERVER H.

ACTUAL DIRECTION OF THE LINE OF LIGHTS.	JUDGMENT OF THE OBSERVER.				
	With 50'' line (Nos. 1-6)	With 40'' line (Nos. 1-5)	With 30'' line (Nos. 1-4)	With 20'' line (Nos. 1-3)	With 10'' line (Nos. 1-2)
v	v	v	v	v	ob
h	h	h	h	—	h
v	v	v	v	v	v
ob	ob	ob	ob	h	d
h	h	h	h	h	h
v	v	v	v	d	h
ob	ob	ob	ob	ob	d
h	h	h	h	h	d
ob	ob	ob	ob	h	d
No. of failures in the series	—	—	—	3	6

OBSERVER B.

ACTUAL DIRECTION OF LINE OF LIGHTS.	JUDGMENT OF THE OBSERVER.					
	With 50'' line (Nos. 1-6)	With 40'' line (Nos. 1-5)	With 50'' line (Nos. 1, 3, 4, 6)	With 50'' line (Nos. 1-6)	With 50'' line (Nos. 1, 3, 6)	With 40'' line (Nos. 1, 3, 5)
v	v	d	v	v	v	d
ob	d	ob	ob	ob	ob	ob
h	h	h	h	h	h	d
v	v	d	v	v	v	d
v	d	v	v	v	v	d
ob	ob	d	ob	ob	ob	ob
h	h	h	h	d	h	h
ob	ob	d	ob	d	ob	ob
h	h	d	h	d	h	d
v	d	v	v	v	v	v
No. of failures in the series	3	5	—	3	—	5

OBSERVER D.

ACTUAL DIRECTION OF LINE OF LIGHTS.	JUDGMENT OF THE OBSERVER.					
	With 50'' line (Nos. 1-6)	With 40'' line (Nos. 1-5)	With 50'' line (Nos. 1,3,4,6)	With 50'' line (Nos. 1-6)	With 50'' line (Nos. 1,3,6)	With 40'' line (Nos. 1, 3, 5)
v	v	d	v	v	v	v
ob	ob	ob	ob	ob	ob	ob
h	h	h	h	h	h	h
v	v	d	v	v	v	v
v	v	v	v	d	v	v
ob	ob	d	ob	ob	ob	ob
h	h	h	h	h	h	h
ob	ob	ob	ob	d	ob	ob
h	h	h	h	h	h	h
v	v	v	v	v	v	v
No. of failures in series	—	3	—	2	—	—

OBSERVER S.

ACTUAL DIRECTION OF LINE OF LIGHTS.	JUDGMENT OF THE OBSERVER.						
	With 50'' line (Nos. 1,3,6)	With 40'' line (Nos. 1,3,5)	With 30'' line (Nos. 1,3,4)	With 20'' line (Nos. 1-3)	With 30'' line (Nos. 1,3,4)	With 40'' line (Nos. 1,3,5)	With 50'' line (Nos. 1,3,6)
v	v	v	v	ob	ob	v	v
h	h	h	h	h	h	h	h
v	v	v	v	v	h	v	v
ob	ob	ob	ob	ob	ob	ob	ob
h	v	h	h	h	h	ob	h
v	v	v	v	ob	v	v	v
ob	ob	ob	ob	h	ob	ob	ob
h	h	h	h	h	h	h	h
ob	ob	ob	h	ob	ob	ob	ob
v	—	v	v	ob	v	v	v
h	h	h	h	h	h	h	h
ob	ob	ob	ob	ob	ob	ob	ob
No. of failures in the series.....	1	—	1	4	2	1	—

From the tables it is seen that observer H was able to judge correctly the direction of the line when it was reduced in length from 50 in. to 40 in. and even to 30 in.; only at 20 in. did failures begin to appear. In his case, therefore, the direction of the line was still perceptible when the length of the line was about 0.0006 of the distance between observer and signal, or when the extremities of the line subtended a visual angle of about $0^{\circ} 2'$. With no other observer, however, was there such nicety of perception; for the most part a shortening of the line to 40 in. was marked by the appearance of failures of judgment. But with a length of 50 in. (under certain conditions of illumination, of which more will be said later) it was always possible to run through a series without errors of judgment. Such a 50-in. line is approximately 0.001 of the distance between the signal and the observer's station, and subtends a visual angle of about $0^{\circ} 3' 20''$ —a result that roughly agrees with that obtained in the preliminary experiments in the laboratory: when a row of pin-holes 3 mm. in length could be correctly judged as to their direction from a distance of 3 m. the ratio is 1:1000—; when a row 6 mm. long could be judged from a distance of 8 m. the ratio is approximately 1:1300.

The angular measurement here is well above that obtained for the perception of difference of locality by means of adjacent points or parallels, where the space threshold descends from $60''$ to a value as low as $30''$,¹ or even to $7''$ and lower, under special conditions of experimentation.² But the higher threshold in the present experiment is hardly to be wondered at, when one considers how different are the conditions of perception here from those that give us the accepted values of the *minimum visibile*. Doubtless if discrimination of direction with the shortest possible extent of line were the one thing needful in a practical railway signal, far more favorable conditions for catching the direction could easily be devised—probably by using a weaker intensity of light than was here employed, and by reduc-

¹ Helmholtz: *Physiologische Optik*, 2d Ed., pp. 256 ff.

² "A New Determination of the Minimum Visible," *Psych. Review*, VII, 429; "Visible Motion and the Space Threshold," *Psych. Rev.* IX, 433. Cf. Bourdon: *La perception visuelle de l'espace*, 144 f.

ing the number of lights to two. But there would, in that event, be the disadvantage that with very weak intensities the signal might in many instances not be seen at all; and with but two lights instead of more, the lights might often appear discrete rather than linear, and the danger would be great of mistaking for a signal two meaningless lights that happened to be in proximity to each other in windows or on the street, especially when near some point where a signal was expected.

Yet to some extent a concession might well be made. Experiments with two of the subjects show that in general the same length of line can better be discerned when given with longer intervals between the constituent lights of the row. With observer B, when the 50-in. line was given by means of all six lights, three errors were made in each of two series. But when exactly the same length of line was given by means of fewer lights no errors were made.¹ That with a 40-in. line as many errors were made by this observer when the line was given by 3 lights as when given by 5, is perhaps due to the fact that for either arrangement the length seems to have been well below the threshold, and consequently no difference could appear.² With observer D the advantage of fewer lights is again apparent: in the 50-in. line with 6 lights one series shows no errors, while another shows 2; but when the 50-in. line is given by fewer lights no errors are in either of the two series. And with a line reduced in length to 40 in., 3 errors occur when given by 5 lights, but when given by 3 lights all errors disappear.

It is thus evident that one can over-reach in the very effort to make the line striking by multiplication of lights densely massed. The line then, because of retinal irradiation as well as of faulty refraction, loses nicety of form. The most favorable arrangement thus lies in a happy mean between lights so faint that they are in danger of remaining unseen at some critical time, or so far-spaced that they are seen discrete and not as a continuous line, and a crowded row that blinds the sense of direction by excess of light.

¹ See columns 4 and 6 in table on p. 99, and cp. with columns 2 and 5.

² See columns 3 and 7 in the same table, where it will be noticed the answers are only half correct.

IV. Résumé of Experimental Results.

The experiments here reviewed make it seem probable that:

1. A stationary object can be perceived at a lower intensity of illumination than is needed for a moving object. This at least is the case when there is more or less adaptation to darkness—and runs counter to our expectation based upon the evident influence which supra-liminal moving lights have over the attention.

2. The red [danger-signal requires for its perception an intensity of light far greater than is needed for a 'white' light, and moreover far greater than can be accounted for by the physical absorption-coefficient of the red glass. The intensities necessary for the red, range from 14 to 70 times that needed for white.

3. In comparison with green, the red danger signal is at no such disadvantage, although its absolute defects, of course, remain the same. Green is perceptible as mere light at an intensity of illumination far lower than that needed for red; but to recognize it as *green* the intensity must be increased until it is almost if not fully that required for red.

4. A number of lights in a row may be perceived, even as regards the *direction* of the row, at an intensity less than the threshold for any one of the constituent lights alone. The advantage of a row over a single light may be as high as 2 or 3 to 1.

5. A number of lights in a row may be perceived in their proper direction under circumstances approximating 'service conditions' when the length of the row is about 0.0006 to 0.001 of the distance of the observer from the row, or (to express it otherwise) when the extreme lights subtend a visual angle of approximately 2' to 3'. Prudence would of course suggest that in actual service this limit should not be too closely approached.

6. The optimal relation of lights for the perception of direction with a minimal length of line requires a certain moderation of the intensity of the row, either by reducing the number of lights (short of an appearance of discreteness) or by reducing their candle-power.

The bearing of the above facts upon the problem of night-signals for railways I have attempted to set forth in the articles cited near the beginning of the present paper. So that I shall not venture to repeat what has there been said. But apart from any personal interpretation or proposal, the experiments help to make clearer the plain facts that must be taken into account by those responsible for a system of railway signals.

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The Social Will

BY

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Dissertation presented to the University faculty of the University of
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INTRODUCTION.

The following thesis has grown out of general studies in sociology and psychology which I prosecuted under the direction of Professor Cooley and Professor Pillsbury. So far as anything new is offered in the thesis, I may say it consists in this: that the processes rather than the products of collective mental activity have been kept systematically in mind. Suggestion and imitation have received very little attention, in the belief that they contain practically nothing beyond what was already a matter of common possession in the doctrines of association and apperception. I believe that the conception of a social personality as a collective total organized out of mental systems that interact in definite ways, is of more fundamental significance.



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S. 9. A close interdependence exists between political and other social institutions.

THE SOCIAL WILL.

BY

EDWIN ANDREW HAYDEN.

CHAPTER I.

THE SOCIAL CONSCIOUSNESS.

1. In discussing the phenomena which distinguish social groups from others, Mr. Spencer writes of insect communities as follows: “. . . though insects exhibit a kind of evolution much higher than merely organic—though the aggregates they form simulate social aggregates in sundry ways; yet they are not true social aggregates. For each of them is in reality a large family. It is not a union among like individuals independent of one another in parentage and approximately equal in capacities; but it is a union among the offspring of one mother, carried on, in some cases for a single generation, and in some cases for more; and from this community of parentage *arises the possibility of unlike classes having unlike structures and consequent unlike functions.* Instead of being allied to the specialization which arises in a society, properly so-called, the specialization which arises in one of these large and complicated insect families is allied to that which arises between the sexes. Instead of two kinds of individuals descending from the same parents, there are several kinds of individuals descending from the same parents; and instead of a simple coöperation between two differentiated individuals in the rearing of offspring, there is an involved coöperation among sundry differentiated classes of individuals in the rearing of offspring.”¹

2. Spencer has in the foregoing indicated some distinctions of fundamental importance, especially in the idea that insect groups are more like an expanded family than a society strictly

¹ *Principles of Sociology*, vol. i, pp. 5-6.

so-called. Wundt has expressed the same idea more forcibly by saying that the so-called "animal states" are "sexual communities, in which the social impulse that unites the individuals, as well as the common protective impulse, are modifications of the reproduction impulse."¹ But the fact that in most communities differentiation of function is based upon a thoroughgoing differentiation of structure, so that individuals performing unlike services are physically unlike, possesses no more than a biological significance. The important thing, so far as these and other animal groups are taken to be the precursors of real societies, is the way in which the individual comes to share in the group life—to what extent the group life modifies his life and how far the group life is plastic; that is, how far it is capable of change through the organization of its own economy. Defining society as an organized total of thought, feeling and volition, we find only human groups to be truly social; so that we cannot take mere reciprocity of services, as is likely to be done in a narrow economic view, to be the measure of social development.² We do not count the slaves of Athens an integral part of the Athenian society, but rather look upon them as the physical or economic background of the small group of free citizens, among whom there went on an active exchange of thought and feeling. A society is animated by a common consciousness of historic events, of traditions religious, political and industrial; and it is chiefly in so far as economic relations modify these contents of the social mind that exchange of services is a matter of importance to the sociologist. So long as foreign merchants at Rome were mere traders with the Roman people, they did not constitute an integral part of Roman society; but when through the business relations created, and more particularly through other relations arising from the marriages which they contracted with Roman citizens, it became necessary for the courts of Rome to deal with cases between the stranger and the citizen, and thus to gradually develop a system of legal rules defining their rights, there was going on a process of assimilation, changing something which was in its inception merely economic, into something to

¹ *Outlines of Psychology*, 2d ed., p. 311.

² See Tarde: *Laws of Imitation*, p. 64.

a degree social.¹ The foreigner was then beginning to share something of the desires, thoughts, and feelings of the citizen. Reciprocity of thought, of feeling, of desire, of motive to action, rather than reciprocity of service is the real index of social relations.

3. In respect to the mental characters displayed, communities may be divided into two widely contrasted types, the instinctive and the social. Insect communities are the best examples of instinctive groups, while a modern state, as a highly organized purposive association, is the best example of the social. The mentality of an individual in an instinctive group is largely predetermined at birth. The individual inherits a reflex nervous mechanism in which a perfected correlation exists between certain sensory stimuli and certain movements essential to its welfare. Many naturalists are inclined to regard the instinctive acts of wasps, bees and ants as pure mechanical reflexes, the most successful attempt to show this being probably that of Bethé.² If this view is true the only psychical attributes which can be attributed to these animals are, in accordance with the usual postulates of physiological psychology, a certain amount of sensation and feeling due to the sensory stimulation, either from external objects or from the movements themselves. Use does not seem to modify to any appreciable extent the acts of these individuals: so that the particular act is performed about as well the first time as ever and then only in response to immediate excitation. Whenever the excitation occurs, the response follows, no matter if the present conditions under which it occurs involve collateral results of a detrimental nature. It is a matter of frequent occurrence that the same instinct is found in various degrees of perfection among allied species; and this fact with other evidence, strongly supports the view that slow mutations do occur in the mental constitution of instinctive groups. This change is brought about by the selection of inborn variations, and not through the transmission of habits, which latter means some capacity for learning on the part of the individual. Nat-

¹ For a brief account of the Roman Law of the Nations, above referred to, see Bryce, *Studies in History and Jurisprudence*, p. 570 ff.

² Pfliiger's *Archiv. f. Physiol.*, vol. 70.

ural selection at once reconciles the apparent contradiction of individual rigidity and specific plasticity in a thoroughly satisfactory manner.

4. When we pass to other animals, we do not find instincts so highly specialized nor so mature at birth. Habit steps in to perfect the instinct; that is to say, repetition improves the inherited coördination between stimulus and movement. In consequence of this less perfect inborn connection, instincts are in these cases both more adaptable and transitory. It is a well known fact that the instincts of many wild animals are somewhat modified by domestication and further, in the absence of proper conditions to excite the instinctive act, the instinct may die out. The gain in having instincts vague and general lies in the accommodation to circumstances which is thus secured: the loss lies in the lack of definiteness and precision of coördination. To the extent that instinct is highly specialized, is the mental life of the individual limited to processes directly connected with the instinct. This limitation is especially evident in the case of animals whose life shows an organization of mental processes standing in immediate relation to two dominant instincts—the sexual and the alimentative.¹ It is doubtful, however, whether, even in the case of animals which profit most by their experiences, free ideas or representations are to any extent present; and if present, they are probably aroused chiefly “on the spur of the immediate practical advantage,”² i. e., upon peripheral stimulation. Such associations as animals do form seem to be chiefly between sense impressions and impulses to activity. Representations of various sorts are peculiarly a human possession. In the form of memory images of either the remote or resident sensations of an act, they appear at a certain level of mental development as a matter of utility to control action in some cases as peripheral stimulation had done before. “With the rise of language, experience became conventionalized, and set rules replaced the less reliable images. These still persist, however, (1) where arrangement and previsions do not per-

¹ See Wundt: *Human and Animal Psychology*, secs. 23, 24, 27 and 28.

² See monograph by Thorndike on Animal Intelligence.

mit linguistic statement, and (2) as phantasy images.”¹ Briefly, the processes of the mental life of animals do not extend much beyond what Wundt terms passive apperception, and at best, there can be but faint glimmerings of the mental processes which we know in the human mind as understanding and imagination.

5. In the foregoing description of the animal mind, much has, by implication, been said of the human or social mind. It is not a mind in which there exist reason and instinct as parallel psychic processes: it is a mind in which instinct still remains as impulses or tendencies to action which are perfected by training, and which are controlled by a coördinated adaptation of means to ends. Rationality, in other words, consists, to a large degree, in the harmonious synthesis of the perfected impulses in a unified life of thought, feeling and action. But purposive control, exercised through a knowledge of results, exists in all degrees of perfection; so that we must take the difference between the social and the sub-social to be one of degree rather than of quality. Instinctive communities like those of ants and bees, have, in one respect, the necessary basis for the development of a social consciousness, viz, a continuous group-life, which some of the higher species of solitary habits, like the gorilla, do not possess. It is only in human groups that we find both requisites to a collective consciousness, viz, continuity of the group life and plasticity of the individual mind. Here the individual mind gets a content and organization through its contact with other minds; in fact in consequence of the rudimentary state of its instincts, such contact and control is necessary to the perfection of its powers. The function of an organized social life in the development of the individual mind is to furnish a definite and continuous set of impressions and to organize these into fundamental mental systems. In the case of unstable groups like the Fuegians, the group life is too discontinuous, too intermittent to permit the individual's mind to come in contact with impressions sufficiently definite and varied for the organization of a complex mentality. The most that we can expect in cases

¹ Bentley, The Memory Image, *American Journal Psychology*, vol. xi, no. 1, p. 25.

where individual capacities are not much amplified by social discipline beyond what they are at birth, is an acuteness of sense discrimination,—a conclusion which the facts of comparative human psychology support.¹ I have intentionally used the phrase, “acuteness of sense discrimination,” rather than the words used by Burton in writing of the Bedouins, viz, a “high organization of the perceptive faculties,”² for the reason that perception, in a high degree of organization, implies apperceptive activities which only a social state can make possible. A developed world of sense implies something more than the mere capacity to discriminate impressions: it implies further the power to compare them, and thus to discern deeper unity than the mere contiguity of space or time. So far as thinking goes, the uncultivated mind is content with the reproduction of concrete individual experiences, and is unconcerned with the causal relations of phenomena the perception of which requires much analysis and abstraction. Hence in the classification of the objects of experience such a mind seizes upon the obvious and superficial attributes. The scientific mind, however, by the aid of delicate instruments, which have been perfected by the combined thought and effort of many generations, amplifies its powers of sensible discrimination infinitely beyond those of the savage, and by means of the capacity of analysis and abstraction which arises in the social order, coördinates the impressions of sense into an orderly world of cause and effect. In other words, the social life has given him an apperceptive basis for the interpretation of sensations, and this basis becomes more and more the condition of the way in which he perceives the external world.³ This fact has well been developed by Professor Dewey⁴ in the following passage: “Every higher analytic stage influences immediately the lower process, rendering it more definite. It is synthetically combined with it. Every process of reasoning expands a judgment; every judgment enlarges a concept; every concept adds new meaning to a percept. As we universalize, we also

¹ See Spencer: *Principles of Sociology*, vol. i, ch. vii.

² Quoted by Spencer, *op. cit.*, p. 78.

³ See Pearson's *Grammar of Science*, ch. v, sec. II.

⁴ *Psychology*, ch. ix, Intuition. Consult the entire chapter.

see the particular more in the light of the universal, and thus make it more significant and more definite. . . . The more the element of reasoning is involved, the more does the percept mean, or tell us of the object. There is a complete implication of every stage of self-development in every other. The scale from perception to systematization looks at the development as an analytic process of growing universality; the scale from systematization back, looks at it as a synthetic process of growing definiteness. As a matter of actual psychological fact, there is no separation of ascending and descending movement, but every concrete act of mind is an act both of perception and reasoning, and each because of and through the other." In brief, stimulation arouses in the cultivated mind apperceptive processes by means of which external objects are cognized in their general relations, but it arouses in the savage mind only discrete existences. The necessity therefore of social life for the full realization of the capacities of the mind, even in the domain of sensation, is abundantly evident; and still more so when we come to the higher phases of mental life and the more delicate and subtle feelings and emotions. These latter arise only in some social situation, involving the relation of self to others, and presuppose some highly developed means of communication. This we have in language.

6. The so-called language of animals can scarcely be called a language, for signs discharge no further function than arousing certain responses through a connection, largely inborn, between certain sensations and certain movements. Gesture language, which is so extensively employed by savages that have a scanty vocabulary of articulate speech, can become a true language, i. e., an expressive one, because it has in itself the possibility of becoming an instrument in some degree of conceptual thought. Gesture can express concepts of a low degree of generality, i. e., "universals comprehending particular objects as their subordinate elements; but they can only to a very limited extent fix attention on universals having as their subordinate elements other universals."¹ It is only in speech, however,

¹ Stout: *Analytical Psychology*, vol. ii, p. 226.

either oral or written, that we find an instrument of expression capable of responding to the complex needs of the civilized mind. It is unnecessary here to enter into the psychology of thought and language farther than to remark that words come to have significance through contiguous association with the various apperceptive systems of the mind. What I desire to do is to specify a little more carefully how speech functions as a social process. First of all, conventional language is a system of signs of "extreme flexibility,"¹ to borrow Kuelpe's term. Composed of a few articulate elements, it is capable of combining these into an indefinite number of word complexes that can express all varieties of objects, qualities and conditions. Linguistic invention, which at certain stages of social culture, is as much an impulse or desire as other kinds of social invention, forms in accordance with the general laws of the language new words and phrases which are readily assimilated with the old stock. In this way language soon becomes an instrument perfectly subservient to the will of man. Being a temporal rather than a spatial complex, it is more adequate to deal with the continuity of our mental life than other forms of expressions, such as painting and sculpture. A statue may portray a single emotion or situation in a manner that surpasses verbal description; but in the complex relations of life, where time relations are important elements, and where for practical volition it is necessary that mental systems should be unfolded in their details, language is alone adequate to the task. Again, language fixes permanently our systems of conceptual thoughts. I do not mean by this that concepts do not change with the progress of civilization, but rather verbal description enables us to understand a conceptual system, no matter how long since it may have been expressed. We have no trouble in understanding Newton's views on the nature of light, although the corpuscular theory is no longer held. Provided we have had the requisite experience and have competent powers of understanding, we can, by saturating ourselves with his writings, organize in our own minds to a considerable degree, the apperceptive systems acting in his, and thus come to understand something of the nature of his achievements.

¹ *Outlines of Psychology*, p. 14.

Language thus makes possible the transmission of acquired knowledge from one generation to the next; and in this way the coöperation of individual minds extends in point of time far beyond the allotted span of life.

7. Thus the cultured mind bears in its type or quality unmistakable evidence of its social origin; and it is equally true that its contents, or in other words, the ideas with which it is stocked, are but individual totals interconnected in a collective mental process. Not only does the child's hereditary equipment in the way of instincts and capacities imply a social history, but the discipline which transforms this heritage into a reality is social to the last degree. The pedagogical romanticism which holds that the human mind can be developed by the indiscriminate and fortuitous play of physical forces upon the senses is a delusion from top to bottom. The child's earliest experiences with physical things is under the guidance of persons, and so habitual is the association of persons with things that he at first views physical objects almost entirely from the standpoint of social utilities. It is a long time before he is able to make complete abstraction of the personal element in the way in which it is done in such objective sciences as chemistry and physics. A horse for instance does not mean an object of certain anatomical and physiological characteristics, but something on which he can ride, which his father owns, etc. His attitude is rather that of primitive man in the stage of mythologic thought. The savage reads into nature his own thoughts and feelings, and sees in natural forces personalities of various kinds. So predominant in fact is the personal bias in the savage theory of things that it has been given the significant title of "personalism." And the general features of this mode of thought are found to be so uniform among people in a certain grade of mental development, even in the entire absence of historic contact, that the tendency of the human mind to think in personal terms must be classed as one of its fundamental traits. So the child, in recapitulating the broad features of race history, shows the same tendency toward a personal view of things. The whole system of education through which he passes is largely a procedure clarifying the relations which the immediate social facts, viz,

personal ideas, sustain to each other or to things. Aside from a few theorems respecting the combination of numbers, arithmetic is a quantitative exploitation of social relations in the business world. Historically it is an evolution from a primitive concrete social arithmetic which existed long before any theory of pure number was thought of. Geography, too, is a social discipline, being in fact the study of the earth as modified by human action. And of course history in the political sense of the term is preëminently a study of social relations and activities. The personal element, however much crowded into the marginal regions of consciousness, remains an inseparable part of the totality of conditions which make possible a study of natural forces. In fact, the psychologist whose province of study is the human personality in all phases of its manifestation, is not dealing with an experience different as a manifold from that which the physicist studies. The difference lies in the point of view. The physicist makes abstraction of the personal variation or event, or better treats it as a constant factor, by giving the limits of error to his results, whereas the law of personal variation is precisely the thing of interest to the psychologist.

8. In the period of mythologic thought, the consciousness of the individual self is beginning to emerge, as is evident in the fact that primitive man projects his own thoughts and feelings into the process of nature. Although he is for the most part unaware of the fact of projection, yet the image of self, thus reflected in nature, serves with tribal experience, as a nucleus around which the feeling of self clusters. There is now felt a kinship for nature which is wanting in the latter stages of scientific thought. As soon as scientific investigation begins to discover regularity in the flow of natural events, the personalities of nature are metamorphosed into natural processes, entirely unconscious in themselves, and totally objective to the human mind—processes which may thus become a matter of universal apperception. But even then the personal feeling for nature still remains in the religious and poetic consciousness, which feels in the unity of nature the presence of an immanent reason.

CHAPTER II.

THE SOCIAL PERSONALITY.

A. Desire.

Tarde has analyzed the mental life of society into two fundamental processes, desire and belief.¹ I have accepted this analysis as probably the most rational one in the present state of the analytical psychology of the social mind.

1. Reaction, or movement in response to external stimulation, is shown by all systems of energy. With some the stimulation, although very slight, results in the immediate disintegration of the system; with others the stimulation produces a temporary change in the configuration of the system, after which the system regains its former condition. In the words of mechanics, the first are said to be unstable, the second, stable systems of energy. Living aggregates are the most complex specimens of the second kind. We see both plants and animals going through a coördinated series of activities which from the outside, seem to be under the guidance of some purpose or end. Examined more closely, however, most of these activities are found to lack the one essential feature of purposive control, which is this: responding to new situations by an activity that is due to an internal development and organization of impressions. If an adaptation has been developed to an external element A, which uniformly in the history of the species has been associated with B and C, both harmless to the organism; and if A is now connected in a concrete total consisting of D and E, both harmful to the organism, but for which no inhibitive adjustment exists, then the reaction to A will occur with the same certainty as before, with perhaps death as a consequence, owing to the harmful results coming from D and E. A survey of all the facts relating to activity of this sort precludes the supposition

¹ *The Laws of Imitation*, New York: Henry Holt and Company, p. 145.

that it has anything in it analogous to what we know in ourselves as purposive or voluntary control. We describe behavior of this sort in a negative way by saying that it is unlearned or native; or, in other words, that it is the immediate expression of the inherited constitution. It is the most widespread and basic form of behavior in the animal kingdom. But in the higher vertebrates, as has been already noted, these instinctive, or native connections, are changed in the course of the individual's life, such modification being especially pronounced in the case of man, with whom instinctive actions produce experiences which, organized into memories, result in forming an idea of the end or object toward which the striving is directed.

2. An impeded activity manifests itself in consciousness as a longing or craving—a feeling of unrest, which, when processes of attention and knowledge are developed, sets going a series of mental changes that terminate in removing the feeling from consciousness. These feelings are parts of an interconnected process of ideational and affective elements which form a total with clearly distinguished features. Such interconnection changes the vague feeling into a definite one, conscious of the means of satisfaction, that is, into a desire. The attachment of the feeling to a specific ideational content marks the rise of voluntary control. In the course of time a definite means of attaining the object which satisfies a desire is selected. With this selection comes the repetition of a definite set of experiences to which the feeling adheres. The desire stretches over all the component parts of the process of realization, and thus includes a representation of the means as well as of the end. In fact the means as something existing apart from the end is an abstraction. In so far as any part of the process of realization is unforeseen the desire is vague and ill-defined. Conversely, to the extent that any part of the process of realization becomes better defined is the desire less impulsive. As the organization of experience goes on a desire whose satisfaction involves the willing of a complex act is likely to split up into a number of reciprocally limiting and hence more specific desires, forming a system in which there exist various degrees of subordination and ascendancy. Anything that arrests the process of realization at any

particular point, emphasizes that part and tends to disengage it from the total activity, and raise it to the status of an independent volition. The detachment of a partial volition is also facilitated by its functioning as a component of several volitional processes. Again the particular experiences connected with a partial volition may be a source of satisfaction in themselves, independent of their connection in a more comprehensive process. The ideal representation of this satisfaction is equivalent to the formation of a new desire. The ascendancy which a desire may attain is limited in two ways: 1st, by the relation of the desire to the self, as being a member of the individual's entire system of desires; and 2d, by the fact that the volitional process through which a desire is realized, tends to become automatic. Perfect coincidence between the appearance of a want and its means of satisfaction would to a large extent do away with that ideal representation which is the very essence of desire. The only thing that will keep a periodic desire, receiving full satisfaction from passing to the marginal regions of attention, is some change of the circumstances in which it recurs. Such change means a modification of the desire, so that complete satisfaction is no longer obtained. The desire is then again able to command the attention, and lead to the devising of new means of satisfaction.

3. Purely practical desires are far from constituting the entire system of desires even in the case of primitive man. Specific desires of a non-utilitarian character soon appear in connection with mythological systems. As soon as the mental progress of a people has reached a point where wants are to some extent anticipated, the mental life begins to expand beyond the immediate present, and the imagination on the basis of certain social experiences constructs an ideal world, where desires quite remote from economic wants receive satisfaction. In mythological systems, as is well known, we have a blending of science, religion and philosophy, not only as regards the concepts, but also the desires and beliefs peculiar to each. Out of this complex the more intellectual desires of science and philosophy detach themselves and culminate in the pure love of knowledge for its own sake, while the other desires less directly con-

nected to the order of sensible experience, become interconnected in a separate system of moral and religious desire.

4. Knowledge, however, remains for a long time subordinated to practical ends, that is, it remains imbedded in volitional processes that aim to effect some change in the world of things. The development of perception and images of memory is the psychic side of a process which has for its other side the definition of motor activity. The attainment of an object of desire, as already pointed out, necessitates the ideal representation of a series of partial acts, and the more detailed such construction the more perfect the satisfaction of the desire. Thus the selection of the best means of attaining a given end involves an increasing amount of intellectual activity as experience expands. Hence it happens that the apperceptive activities of relating and comparing, of analyzing the mental aggregates formed by association out of the cognitive elements of motor experiences, may arise in the course of practical activities. But deliberation on the choice of means postpones the satisfaction by the amount of time which it occupies, and thus conflicts in one way with the realization of desire. The checking of a motor tendency weakens it so that deliberation may be protracted to a point where it defeats its own purpose. This antagonism between thinking and doing is a matter of common observation. The practical man seldom takes interest in theoretical questions, rarely engages in mental activities that have no aim beyond affecting some change in the world of ideas. It is by means of the social heredity that a reconciliation of this conflict is secured. Individual wills enter into a more comprehensive psychic process, the social will, by which interconnection society is enabled to attain it sends by a mental division of labor. In this way activities may be going on simultaneously in the social mind that would be somewhat incompatible in the individual mind. Mental activity of a theoretical sort is limited to one social group, and practical endeavor to another. The final expression of some epoch-making conception is invariably the work of some thinkers of transcendent genius. Once the idea becomes articulate it spreads through the social medium, and after its incorporation into the fabric of social thought, it becomes a source of common desire.

5. While we can speak with truth of the interaction of individual wills in a higher or collective will, we should not forget that the individual will is part of a concrete personality, and that these personalities interact more or less in their entirety. Out of this interaction arise the personal ideas and feelings which in the period of primitive culture enter into the apperception of the external world. Personal ideas and feelings are gradually detached from the objective ideas and their related feelings, because of the difference in the relation in which the two sets stand to the will. This detachment does not extinguish the system of personal ideas and feelings, for they continue to exist in a world of their own, the world of social relations. The separation between these worlds, however, is never complete, for as a matter of history the two have ever interacted. Though the universe of personal ideas and feelings ceases, in the course of time, to influence immediate sensible experience, yet in the larger and broader aspects of life, where religious faith finds play, it ever remains of sovereign importance. When sensible experience comes to check or suppress the fundamental impulses of man, the will transforms the elements of social experience into "an existence that fully corresponds to the wishes and requirements of the human mind,"¹ forming the universe of moral and religious belief.

6. The specific content which desire assumes, is a matter of national history. Says Tarde: "Every organic want is experienced in the characteristic form which has been sanctioned by surrounding example. The social environment, in defining and actualizing this form has, in truth, appropriated it. Even desires for nutrition and reproduction have been transformed, so to speak, into national products. Sexual desire is changed into a desire to be married according to the different religious rites of different localities. Desire for food is expressed in one place as a desire for a certain kind of bread or meat; in another, for a certain kind of grain or vegetable."² Desires are thus refined by the social experience coming from the volitions which they themselves create. It may be said with Tarde that the means of

¹ Wundt, *Ethics*, vol. i, p. 59.

² *Laws of Imitation*, p. 44.

satisfaction in a measure create the desire, for in defining and particularizing it, they limit the lines along which volition moves in its effort to satisfy the desire. This statement seems to be contradicted by the fact that the means do not always lie in the order of sensible experience. But in answer to this, it should be noted, that desires attaching to a universe which the individual regards as imaginary, are, as a rule, rather evanescent, arising in connection with complex modes in the cognitive synthesis of social experience, and further that imaginary creations bear a close resemblance to the objects of sensible experience so far as conditions of space, time and the general qualities of human nature are concerned. Persistent desires, however, fail to receive full satisfaction in the imaginary creation of the individual, for there remains beneath all an abiding sense of the unreality from which there is no escape, except in those cases where the imaginary creation has been produced by the transformation of historical experience and has thus acquired the status of a belief.

B. Belief.

7. Another mode of consciousness in which the ideational and affective compounds of experience are connected, is belief. The representation of the satisfaction of desire involves a series of specific affirmations which are not detached judgments but interdependent parts of a total process connecting means and end. Such interconnection is belief. Belief involves knowledge, but is not identical with it, for we have perfectly clear and distinct ideas about many things that we disbelieve, as on the other hand we believe some things of which our conceptions are by no means the clearest. The multitudinous creations of fancy recorded in the literature of the world, are things that do not arouse in us the sense or feeling of reality. A cognition in order to become a belief, must have some "fringe of consciousness" added to it. It must be able to evoke in the mind an emotional color in addition to the feelings of meaning and relationship which, though evanescent, form the staple of the cognitive consciousness. Belief stands in intimate relation to the self on the affective side, and to the objective world on the cognitive

side. Things which are a matter of common consent, being acted upon from day to day as habit, are usually not regarded in the light of belief, because the feeling of assurance, or the "emotion of conviction," as it has been called by Bagehot, is absent.¹ When, however, some obstruction to our practical or theoretical endeavor arises, leading to the postponement of gratification, the mind is confronted with a situation in response to which the feeling of the reality or unreality of some particular thing emerges. Deep conviction is associated with strong feelings. When such conviction is challenged the whole self recoils: "Men in these intense states of mind have altered all history," writes Bagehot, "changed for better or worse the creeds of myriads, and desolated or redeemed provinces and ages. Nor is this intensity a sign of truth, for it is precisely strongest in those points in which men differ most from each other. John Knox felt it in his anti-Catholicism; Ignatius Loyola in his anti-Protestantism; and both, I suppose, felt it as much as it was possible to feel it."² Both Knox and Loyola were one with the cause for which they stood; the emotion of conviction assumed a fanatical ascendancy because of the intense self-feeling involved, the belief in both cases commanding the whole resources of the will. The rationality of a belief as measured by the criteria of reality set up by science, is in general a matter of subordinate importance, as the social function of belief is to organize the fundamental desires of humanity, and to do this a belief must possess strong affective elements. Religious and moral, and in some instances political beliefs, possess these characteristics.

8. It may be said in a general way that the world of sensible experience is taken commonly to be the ultimate universe of reality. This attitude of strong conviction toward the reality of the external world, arises, as Stout has clearly shown,² from the limitations which are imposed from without upon the activity of the will. We find ourselves unable to manipulate the objects of perception just to suit our fancy; in the effort to do so the feeling of their reality clearly emerges. So too, when

¹ *Literary Studies*, The Emotion of Conviction.

² *Analytical Psychology*, vol. ii, pp. 239-243.

we run across some stubborn fact of experience which blocks all our processes of thought and refuses to be explained away, we become painfully aware of the frailty of our ideal creations. Thus thought combinations in proportion to the facility with which they can voluntarily be altered lose the moments of reality. It is especially for this reason that the mind is usually alive to the unreality of the images of the imagination. In other cases where the alteration has occurred independent of the will as a matter of mere association or assimilation the mind is likely to accept the modifications as real. The illusions due to preperception are examples of this sort. Frequent repetition of a thing makes us strongly disposed to believe in its reality: the more often a psychic process is recollected, the less is the effort necessary to restore it, so that while it is becoming more firmly fixed in the memory, the mental experiences connected with its invention are being forgotten. The fictitious idea, when thus freed from all such associations, may find lodgment among the true memories, and thus seem to the individual to refer to some part of his past life. Association has brought the idea within the circle of remembered sensible experience. The rate at which the transformation of the contents of memory goes on has much to do with the extent to which the feeling of unreality is aroused by a fictitious idea. Frequently the memories of history become changed in the course of centuries into unrealities rivalling the boldest creations of fiction; yet so gradual has been the change, that the mutations have escaped detection. Such alteration of the memories of history frequently serves the high purpose of allowing greater scope to the sway of social ideals. In the period of mythologic thought sensible experience does not conflict with monstrous beliefs in ghosts, demons, and other supernatural agents, for the reason that nature then mirrors the capricious impulses of the savage, but as soon as perceptions are brought under some concept of order, these beliefs or those into which they are changed, are likely to be held valid, not of the present order of things, but of some past or future state of existence. If, however, such beliefs still remain potent in their influence on human conduct, it is through the fact that they still retain a stable connection with some system of sensible experience.

9. The native attitude of the mind toward its thought combinations is one of belief, an attitude which is checked through painful experience. A successful issue, whether a matter of accident or not, confirms the belief, and a few successful trials, when disproof is difficult, are sufficient to firmly implant the belief in the mind of the race. Something of this pristine faith is necessary to the mental health of a people. Though many of the cosmological beliefs of primitive man have disappeared with the rise of science, yet no void has been left, for the reason that science has brought to light innumerable uniformities in natural phenomena. In fact, science has increased the sum of faith in the objective order, and at the same time has expanded the sphere of religious feeling. It is only when the impersonal attitude of science is assumed toward all the departments of life that human faith, which finds its most adequate expression in religious faith, is on the way to extinction. The skepticism which is present in a decadent civilization, is the result not so much of its science and philosophy, as the moral disorder of the social life, which makes the individual mind the theater of discordant and distracting impulses. Individual or social faith is but an expression of life, after all. An harmonious, expansive life has an abounding faith in the essential truth and goodness of the world, while a life tormented by conflicting passions accepts the same order with misgiving and doubt.¹ But at the same time faith reacts upon life to expand or contract it. A people outgrows some of its beliefs, just as an individual does. Many beliefs which at one time in the history of a culture were real organizing forces, come later to be obstacles to progress. The belief in the divine right of kings, by the halo with which it surrounded the regal head and the obedience it inspired, was a powerful cementing political force in the more unenlightened periods of social development, but the conception was certainly obstructive in French history at the time of Louis XIV, when impulses of democracy were beginning to express themselves that were later lashed into the fury of the Revolution by the stubborn resistance which they encountered. So monasticism, though it did noble service in bringing hope and consolation to

¹ See Paulsen: *A System of Ethics*, p. 421 ff; also James: *Varieties of Religious Experience*, p. 41.

minds distracted by the confusion and disorder following the fall of Rome, was too narrow for the new life of the Renaissance. In a similar way the political and economic conceptions had to expand in order to compass the new social vitality in European life produced by the discovery of the possibilities of the New World.

The tendency of social development is to make the sphere of desire and belief coincident. Every desire which remains unsatisfied, that is, detached from the affirmation of the means of satisfaction, soon dies, as, on the other hand, no belief acquires a permanent social ascendancy, which is not intimately connected with the needs of humanity.

C. Desire and Belief in Relation to the Will.

10. Desire and belief are brought into systematic coördination through the activity of the will. Many desires and the corresponding beliefs are different phases of the same total psychic process in which impulsive acts become purposive through the effects of memory. Other desires and beliefs are independent of each other in origin except in so far as they have a general connection in the same will or personality, and are brought into coördination through apperception. It is in the activity of the will that consciousness alters its contents in a definite direction. Out of the ideational and affective experience connected with such control arises the notion and feeling of self. As a result, desire and belief, through their interconnection in volitional processes, stand in intimate relation to the self. The universe of desire represents "recognition in feeling of the distinction between the actual and the unrealized self,"¹ while the universe of belief stands for the habitual attitude of the self in affirming or denying the possibility of realizing desires. The unification of desire and belief through the activity of the theoretical or practical will, is eminently a social process. Those phases of experience which are not directly modified by the will form a total which the mind regards as independent of itself: while

¹ Dewey: *Psychology*, p. 364.

the other phases subject to volitional control, form the contrasting total of the self. Now in the early history of civilization, the separation of self and object is imperfect both in thought and feeling: the increased coöperation of individual wills which comes with social experience widens the sphere of sensible experience on the one hand and on the other the sphere of internal experience connected with self-activity. The final result is that complete detachment in thought and feeling of the self from the manifold of perceptual experience, which is seen in its best estate in the scientific consciousness. Out of the internal experience develops not an isolated personality, but a consciousness of a plurality of like selves, sharing a common life of thought, feeling and action.

11. The external expression of the social will is the activity of social life. Individual wills are linked in associations of various degrees of complexity, each association having interests, desires and beliefs, in a word, a life peculiar to itself. Within each group of individual wills are to be found common motives to volition, with the result that group ends are achieved in a more or less rational way. Individual wills are not, however, of equal importance in the organization of motives. Within certain limits the statement is true that the greater the number of individual wills interacting the less deliberative, the less rational, the resulting action. The final expression of a great conception is as a rule the work of a few minds; the organization of this into a social impulse is partly the work of suggestion and partly of choice. Some writers, like Le Bon, seem to imply that a collective mind really exists only at the moment when a group of individual minds are simultaneously affecting each other as in a crowd. We do not restrict the individual will to the complex of ideas and feelings that happen to be above the threshold of attention at a particular instant: and in an analogous way, there seems to be no good reason for restricting the social will to the sum of ideas and feelings appearing simultaneously in a group of interacting minds.

12. While impassioned discussion upsets for the time being all rational deliberation, nevertheless more temperate discussion enables the individual to get glimpses of new and important aspects of a subject which will assist him in the calm of his

private moments to reach a more satisfactory solution of the problem. While social deliberation does not attain the degree of rationality and control which the processes of thought in a highly endowed individual mind possess, yet it is far from receiving justice at the hands of the theory of suggestion.¹ The most transcendent genius is connected through a graduated series of capacities to the average mediocrity, so that the organization of public opinion is by no means a process consisting first of all in the invention of an idea by one supreme mind, and the subsequent incorporation of this idea in other minds by mere association. Frequently the acceptance of an idea appears to be automatic, when in reality the acceptance of the idea marks the completion of a mental process whose development involved complex apperceptive activities. Still it remains true that collective thinking is in general less controlled than individual thinking. An idea which has been perfected in one mind cannot be communicated in its final form to another mind; the second mind must repeat to some extent the process of development which the idea underwent in the first mind, and in so doing gives some play to association to bring in irrelevant ideas. Today natural selection is a datum in reasoning on biological matters; yet a quarter of a century nearly passed before the idea became apperceptive in the collective mind of biologists. So far as the net gain in positive knowledge is concerned, much of the thinking and feeling of that period was sheer waste of mental energy. The collective thinking of society would be far less efficient than that of a single mind, were it not that a multiplicity of coöperating minds permits a division of mental labor. This division extends to all phases of the collective mental life. Society has as its disposal a vast fund of knowledge which it turns to account through various associations, each of which has more or less clearly defined aims and within the limits of these aims a will of its own. That is to say, the various associations have a range of motives outside of which choice cannot be made without its prescribed character being violated. But with one exception, the state, the range of motives is far from being

¹ See Baldwin: *Social and Ethical Interpretations*, ch. vi, Sec. 5. Also Giddings: *Principles of Sociology*, ed. of 1896. 150 ff.

exhaustive. The social will expressed in the state is free in the sense that the only limit to the range of motives which actuate it is its own psychic constitution, and in the further sense that it is capable of making a rational choice of motives. That the social will exerts a directive force is unmistakable. The state has made notable achievement in the domain of education, industry and politics: and the part which it is destined to play in the future gives fair promise of being greater than in the past. Mr. James Bryce puts the matter fairly when he says:

"Modern civilization, in becoming more complex and refined, has become more exacting. It discerns more benefits which the organized power of government can secure, and grows more anxious to attain them. Men live fast, and are impatient of the slow working of natural laws. The triumphs of physical science have enlarged their desires for comfort, and have shown them how many things may be accomplished by the application of collective skill and large funds which are beyond the reach of individual effort."¹

13. The ends of the state are thus more comprehensive than those of any other association; while the autonomy of its will and the indefinite range of its motives give it a unique psychic character. The state reflects upon its past and plans for remote future ends. It has one supreme end, the welfare of society, which it strives to attain through a series of particular volitions, in which it evaluates, to some degree, its motives according to the mode in which they modify its character. The voluntary control exercised by associations within the state is chiefly prudential; the choice of motives being largely made from the standpoint of interest or advantage. These associations seldom scrutinize their motives from a moral point of view; such evaluation as they do make, is limited to those negative cases in which some question arises as to the prescribed (legal) limits of their authority. The individual will may be subordinated to a number of partial or group-wills; but as a rule there is one particular group-will in which this subordination is completest. This corresponds to the dominant universe of the individual

¹ *American Commonwealth*, 3d ed., vol. ii, p. 539.

mind. But to each social group to which the individual sustains organic relations there is a related universe of desire and belief, forming a small social world within itself. When the individual will comes into relation with the total will of society, the point of contact is in some one of these particular universes; or, in other words, the contact is not between individuals as members of society in its entirety, but between individuals as members of the same or different particular groups. At the same time the desires and volitions of the various social groups are subordinated to a still higher psychic unity, the personality of society.

14. That the concept of a social personality stands for a reality is evident from a variety of considerations. If we take an historic survey of the mental life of a people, we invariably find in the existing fabric of its civilization elements coming from a remote past. Its religious, moral and political beliefs have resulted from the combined thought, feeling and action of many generations. Thus a civilization is a psychic synthesis of the past experiences of a society. While the civilizations of the earth have many broad traits in common, yet each has a content and organization peculiar to itself, forming a genius or temperament that binds into a delicate unity the most diverse products of its activity. These differences of national genius are not things of a day merely, but characterize a nation throughout its growth and decay. The social mind has a character or disposition as truly as the individual mind, founded upon certain fundamental desires and beliefs and correlative modes of action. They are the elements which give stability to the psychic life of society. They are of course rooted in the habits of the individual personality. A certain way of thinking and acting spreads through a community; repeated again and again, it becomes a mechanized process whose unfoldment is more or less independent of attention. The mechanization of the original attentive process has been effected through social discipline, and for this reason there is an interconnection of individual dispositions in a wider mechanism which we may fitly term the social disposition. But these mechanized processes possess something higher than a merely vital or biological significance, even in the stage

of complete formation, because as Stout remarks, they "may enter as component parts into a total process which as a whole is very far from being automatic. The inverse of this is seen in habits of thinking and willing. Here a comprehensive habitual tendency realizes itself on special occasions by means of special processes which are not habitual."¹ Hence it is that social habit is never a closed automatic series functioning independently of the will, and that custom, the external expression of the social disposition, never sinks to the level of instinctive control.

15. The extent to which habitual tendencies enter into the volitional acts of a community varies with the stage of civilization. The despotism of custom in the period of primitive culture is a notorious fact. Habit then forms the chief ingredient of motives, while at the same time the range of individual variation in habit is narrow. Contiguous adhesion plays an important part in the unfoldment of volitional processes. In this stage of mental development the individual will does not organize experiences into complex apperceptive systems, and thus return to modify the social will in a serious way. With the growth of civilization, the range of variation in habit is increased; habits are multiplied, but at the same time, as already noted, they function in processes having a degree of conscious control. The development of the social will involves at the same time the differentiation of the individual will; so that in a state of advanced culture, the individual will functions in some ways as a tendency more or less complete in itself. Here the individual will has certain ends and purposes which are purely personal, as in particular interests relating to private property. As a complex social environment requires the readaptation of volitional processes to new circumstances, some component parts whose unfoldment was previously more or less automatic, now demand attention, and in consequence become to some extent independent volitions, so that in this way a continuous enlargement as well as particularization of the universe of motives goes on. The individualization is not to be conceived, however, as the segregation of some particular will from the whole social will, but as the infoldment of the social will, out

¹ *Analytical Psychology*, vol. i, p. 262.

of which arises a number of reciprocally limiting and partial wills, externally manifest in the corresponding lives of the social groups concerned. The extent to which the infoldment modifies preëxisting social habits varies with the different social groups and with the habits themselves. Some groups retain more than others the character of the primordial society, while some habits are but slightly modified in any of the social groups. The latter are the basis of the national culture, forming the stable elements of the social personality which enable it to withstand the profound shocks of political revolutions.

16. When the growth of civilization has reached a stage in which the individual will is enabled to organize social experiences in a manner peculiar to itself, volitions may then speedily mature in a single mind and spread to the other minds of the community. Such volitions are in relation to the entire history of a culture somewhat ephemeral. They are produced by a mental activity which, to a considerable extent, operates independently of the apperceptive control exercised by the permanent beliefs of the race. They gain a temporary ascendancy owing to the action of highly special and accidental causes; but they do not persist for any length of time, not because they lack cohesion of parts, but because springing up at various parts of the social medium, they act as mutually inhibiting motives in the social mind. A general condition which favors the appearance of these transitory beliefs is a skepticism resulting from the weakening of old beliefs. In a period of social anarchy, the times are ripe, owing to the excited condition of the public mind, with a multitude of beliefs, "which appear first here and then there, only to disappear, until the advent of some clear formula or some suitable mechanism which throws all the others into the background and which serves thenceforward as the fixed basis for future improvements and developments."¹ We have here an instance of the general law of apperception that when for any reason the systematic control exercised by any mental system is temporarily suspended, the forces of association may come into play to fill the mind with a multitude of disconnected ideas until some new system supervenes. These unstable beliefs

¹ Tarde: *op. cit.*, p. 148.

function as more or less independent units. If they have strong affective components, they act with great vigor and energy, and thus set going external events that destroy the condition of their existence. They are thus likely to pass into action at once though a long course of action is out of the question. We have a record of their work in the violent, turbulent periods of history. But extreme mobility of opinion is possible only as the terminal phase in the mental evolution of a people, in which the foundations for a civilization and a national culture have been laid in the ground-work of a few fixed beliefs.

CHAPTER III.

THE SYSTEMATIZATION OF BELIEF.

1. In the early history of the social mind the apperceptive control is relatively simple, association or contiguous adhesion being the chief form of the interconnection of psychic processes. The lax interdependence of associative systems makes it possible for somewhat contradictory beliefs to be held. The absence of unifying principles of experience permits an indefinite extension of associative systems through mere accretion; so that we find in the history of the human mind a period whose most salient feature is the accumulation rather than the systematization of belief. There is, however, an evident limit to the extension of belief, notwithstanding a freedom from apperceptive restraint, viz., the uninventiveness of the primitive mind. There is too much solidarity of the individual and social will in the early stages of civilization for the former to elaborate social experiences in a fashion peculiar to itself; but the very conditions which free the individual will at the same time extend the sphere of apperceptive control. The creations of the developed individual will are notably more numerous and at the same time more coherent than those of the primitive will. Early inventions are largely modifications of memory contents; later inventions are a combination of the elements of experience under the motive of a purpose or end. The latter involve a more detailed analysis of experience and a more comprehensive and systematic synthesis of its elements.

2. (a) The mental systems into which historic experiences are organized, are never in the social consciousness in their fullness at one time. A system is able to exert an influence on the stream of consciousness without the necessity of its parts being explicit, i. e., it may act as a total tendency from the fact that there is a common trend in all its parts. Much of the social control existing before the collective mind has reached a high

degree of self-consciousness, is in mental systems, the fundamental unities of which in the ordinary run of things remain largely implicit. But when new conditions confront society, the likelihood exists that the various parts of a mental system are not excited with equal force owing to the unusual modes of stimulation, and as a result some, perhaps all, of the parts become explicit. Times of profound political disturbances are preëminently the periods in the history of a nation when social dispositions are unfolded through the excitation of their components. The unfoldment may at times be violent, but even then the fundamental beliefs of the race come in to give a more or less definite trend to the outburst.

2. (b) The systematization of belief means in its individual aspect the specialization of the universe of intercourse. As the interaction of mental systems becomes more definite in the individual mind, so the interaction of individual minds becomes correspondingly more controlled. The fundamental systems into which social experience becomes in the course of time organized, display certain special tendencies which they did not have in earlier times, although they remain interconnected in the general mental fabric of the civilization. Thus in the modern secular state, we find changes in political ideas spreading through society without involving to any serious extent the religious beliefs; while in the old Hebrew theocracy, religious ideas were so closely interwoven with political, that the utterances of the prophets as the acknowledged oracles of God, seriously modified at times the affairs of state. To each of these fundamental systems of belief corresponds a conative tendency of the social will. The transference of an idea or mental element from one mental system to another frequently occurs in the history of a culture. Men's views of the world and life change: which means not so much that the facts of common experience are different as that the mental systems into which they are incorporated are different. But a belief cannot function in two distinct mental systems at the same time. The æsthetic attitude toward an impression is incompatible at the time of its existence with the scientific. Social beliefs are coördinated through their interconnection in the social personality; but when this coördi-

nation is disturbed, the ascendancy and isolated action of one system ensues. Under such circumstances the belief is likely to incite acts which the interconnection of the belief had previously inhibited. In this way social indignation acting independent of legal sentiment may result in the avenging of wrong through mob-violence; so, too, religious zeal, freed from other social emotions, may result in a fanaticism that counts its victims by the score. On the other hand the union of independent systems restricts each in a manner corresponding with the principle of combination, as we see in the modification of the political ideas and institutions of a state when it is united with others in an empire.

3. There are two widely contrasted types of mental systems. In one the unfoldment depends upon contiguous adhesion, i. e., a given fact emerges into consciousness largely through its dependence upon the immediately preceding facts. In the other there is a central principle of control: a part emerges not because of its relation to the preceding part alone, but to all other parts of the system as well. Where there is such solidarity in the interaction of the individual wills that any one of them but slightly changes the collective activity, such changes as do occur in the social mind are largely associative. Associational changes stretch over psychic processes ranging from sense-perceptions to the interconnection of mental objects in a temporal process. Modification of sense perceptions have an indirect interest to the social psychologist in the fact that the sensory product may be combined along with other ideas through memory in a belief. Thus illusions acquire special significance if they become incorporated in a mythological system. Of direct importance are the changes occurring in a temporal succession of ideas. One of the factors at work is the natural effacement which mental objects undergo through the failure of memory. Details are forgotten, only certain features remaining permanent mental possessions. The permanence of an impression depends not only upon certain qualities of its own, but upon a group of highly variable subjective conditions. As a result there is an uneven fading in the contents of a memory process, with the result that reproduction is always an imperfect reinstatement

of the experience. An associative system may thus break up passively through internal dissolution into its component parts, which then become attached again through association to other systems. A similar phenomenon is observed in the social mind. Distinct streams of thought become confluent in the course of history when the circumstances of their origin are forgotten. A striking historic personality serves as a center of attraction for myths and legends derived from independent sources. Another source of unconscious modification is found in the rôle which associative systems frequently play in being parts of a more comprehensive system which as a whole is apperceptive. We may take in illustration of this what Wundt calls "the change of purpose in custom."¹ Speaking of the funeral feast he says: "In its earliest form the funeral feast is a sacrificial feast. Primitive man offers sacrifices to the gods at every important occasion of his life, and will certainly make an offering at the burial of a kinsman. In part he desires to obtain the divine favor for his dead, but in part—and this is probably the more ancient idea of the two—the dead man is himself an object of worship. . . . A second motive, which came into operation at a later date, but may gradually have ousted the original worship of the dead, lies in the symbolic meaning of a feast eaten *in common*. The common enjoyment of meat and drink is for primitive man a religious symbol of brotherhood; more especially if the feast have anything of solemnity about it, if it be sanctioned, so to speak, by the presence of the gods. . . . It is this final form of the funeral feast whose traces have been longest preserved. With its passage from a sensible to a symbolic meaning it has gradually lost its religious reference. The funeral feast, that is, becomes simply a *commemorative* feast, at which mention is made in conversation and discourse of the virtues of the dead." The changes in the feelings and ideas which were associated with the given custom are unintentional modifications brought about by the confluence of mental systems. The funeral feast and the commemorative feast were alike in this respect, that in both there was the enjoyment of meat and drink in common; and in consequence of this likeness it

¹ *Ethics*, vol. i, p. 139 ff.

was but natural that the funeral feast should attract to itself the feelings and sentiments associated with the commemorative feast.

4. In apperception the changes produced in consciousness take place in a state of attention according to some motive which controls and preconditions the change. Society adjusts itself to new conditions by the conscious adaptation of old stores of knowledge. The adaptation is generally effected first at one center and then spreads throughout the social medium. Social theory which finds in the interaction of individual minds the essential phenomena of hypnotism only, is inclined to draw a radical distinction between the mental processes going on at the social center where the adaptation is first effected and the mental processes going on at the centers which repeat this adaptation. The first have been dignified by the name of invention while the latter have been forced to acknowledge the impeachment of mediocrity in the name of imitation. Now the distinction is of profound importance for a theory of social progress which is directly concerned with mental products, but is of less importance for social psychology, which aims to study the interconnection of individual mental processes in collective mental processes. Invention marks the termination of an apperceptive process in which a determinate psychic compound, image or conception is produced. In imitation an image is communicated to the mind in a more or less completed form. Through whatever medium the communication occurs we have contiguous association between the verbal symbols and mental systems, by means of which certain mental systems are brought into conjunction that would have forever remained isolated in that particular mind. In this way association can be of material assistance in producing favorable conditions for an apperceptive action between two mental systems: but its action can extend to nothing beyond bringing the two systems together in consciousness, and exciting the partial system upon which apperceptive interaction depends. If the mental systems are relatively simple, the apperception is of the ordinary degree of complexity; so that to superficial observation nothing seems to be involved beyond the mere lodgment of a communicated idea in the par-

ticular mind. Now in invention the same two features of association and apperception are at work, though not in the same relative proportion. In invention the conjunction of mental systems is less externally determined by social suggestion, while the interaction is likely to be more prolonged and persistent. Invention implies more comprehensive mental systems and more sustained attentional control. But there is no case of invention in which social contact, or what this amounts to, psychologically speaking, viz, association, has not played an important part. The history of the theory of natural selection sheds considerable light upon the psychology of invention. I take the following short account from Morgan:¹ "Charles Darwin and Dr. Alfred Russell Wallace both reached the conception of Natural Selection on reading Malthus's work on Population. Both had acquired a system of knowledge concerning the relationships of animals and plants. In both the net results were constantly in mind. As they ranged in thought over the system, now one and now another factor was in the focus of attention, with a rearrangement of the other factors around it. They read Malthus. Unless some factor in the Malthusian universe of discourse coincided or was congruous with some factor in the universe of biological thought, the two could not come into fruitful relation. But there was a mediating factor common to both—over-production of offspring. There were other features sufficiently congruent to enable the Malthusian discussion to throw light on the problems of biology. Hence arose the suggestion . . . of Natural Selection through the elimination of the unfit." Here social suggestion communicated to the minds of the two distinguished naturalists a mental system, which was combined in an apperceptive way with the systems of biologic relationships already formed, into a more comprehensive system the fundamental unity of which was the concept of natural selection. But the same mental processes are involved in the humbler achievements of everyday life.

5. Owing to the interconnection of individual wills in a wider volitional process, it happens that a change, originating in an individual mind, spreads through the social medium in

¹ *Psychology for Teachers*, p. 87, new ed.

some determinate fashion. A change in a given mental system spreads to other mental systems in the order of the degree of relatedness of the latter to the former. In so far as the mental systems are more thoroughly organized in the minds of respectively different social groups, we have, corresponding to the order of psychic excitation, an order in the modification of the life of social groups. Social changes begin as changes in the desires and beliefs of a certain group, followed by like mental changes in the groups whose interests are most closely identified with those of the first. But to the extent that other desires and beliefs happen at the time to be ascendant in the minds of the other social groups, the disturbance originating in the mind of the first group meets a corresponding resistance to its spread. The excitation is in its earlier stages of a general nature and becomes more specific as the infoldment proceeds. Accordingly we find deep social changes beginning as vague mental tendencies, which are nothing more than feelings of unrest and dissatisfaction with some existing institution, and which continue for some time in this merely negative attitude of protest. Later a plan emerges that seeks to remove the cause of dissatisfaction by substituting some other arrangement that will realize the needs in this direction in a better manner. The plan becomes a motive to a series of volitions that may have profound and revolutionary changes as a consequence. The earlier phases of such social movements involve the excitation of some universe of belief in its entirety, with a corresponding indefiniteness in the reaction of the social will. The component systems of the particular universe are all equally aroused, so that none of them can become appercipient in preference to another; but later on one of these gains some ascendancy, and events now take a definite turn, owing to the resulting univocal nature of the motives to social volition. The period of incubation of a social movement is thus one in which component mental systems of some universe of beliefs are struggling each against the other to become explicit in the public mind. The issue depends upon a variety of conditions, which lie partly in the mode or circumstances of stimulation and partly in the nature of the mental system itself.

6. The recency of its activity conditions the ascendancy of a mental system. A system out of use is undergoing continual decay. Soon the parts begin to function independent of each other, and to restore the system to its former degree of efficiency requires a process of recollection that is discursive in a degree proportionate to the time during which the system has been dormant. A mental system that has been in recent action may function more efficiently than one which has been out of use for some time although, in the event of continued disuse, it would soon fall into a greater degree of incoherence than the later system. Besides reproduction of an incoherent system generally brings in through association irrelevant ideas that delay the apperceptive activity of the system. At times these are incorporated within the system, as already noted in the case where memory images become changed into images of the imagination. An interesting example of how recency conditions the efficiency of a mental system is given in the memory of the late seismic disturbances. The news of the South American earthquake brought to mind in even considerable detail, the facts of the California disturbance, but only vaguely those of the Charleston.

7. Intensity and vividness of the elements of a mental system are important conditions of its strength. It is chiefly on account of the intensity and vividness of its elements that the world of sensible experience is taken to be the ultimate universe of reality and that the creations of fancy never command belief until they find lodgment in the memory series. Intense and vivid experiences such as are incident to political revolutions are more deeply engraved on the social memory, although the latter have been repeated many times. Ihering advances the proposition, in opposition to the Savigny-Puchta theory, that all great legal principles have been established by what he calls the "struggle for right." Undoubtedly the intensity of the experiences incident to a struggle in which some legal principle is born, is an important factor in helping to maintain the assertion of the right involved, before it has crystallized into a social sentiment. Le Bon has well described the effect which startling events produce on the public mind: "A hundred petty crimes

or petty accidents will not strike the imagination of crowds in the least, whereas a single great crime or a single great accident will profoundly impress them, even though the results be infinitely less disastrous than those of the hundred small accidents put together. . . . The probable loss of a transatlantic steamer that was supposed, in the absence of news, to have gone down in mid-ocean, profoundly impressed the imagination of a crowd for a whole week. Yet official statistics show that 850 sailing vessels and 203 steamers were lost in the year 1894 alone. The crowd was never for a moment concerned with these successive losses, much more important though they were as far as regards the destruction of life and property, than the loss of the Atlantic liner in question could possibly have been."¹

8. The support which a mental system can command from the other systems with which it is connected, is of material assistance in the maintenance of its ascendancy. Now we have already seen how association may bring into relation two mental systems that might otherwise remain disconnected; and if such conjunction occurs when the mind is especially active, the two will probably unite in a more comprehensive system whose total energy is greater than that of either. Under such conditions a given mental system, with its associate, in their joint activity, can effectually oust from consciousness another system, although considered in itself it may have less inner stability than its rival. What is called the social opportuneness of an idea or invention, depends upon such associative conjunction. An idea that is harmonious with the general set of the public mind, rallies to its support a whole mental array, while another idea, equally meritorious but lacking such support, fails to command general attention. Not that the mental systems are wanting which under other circumstances would yield the latter support, but that for the time being, they are prevented from acting. With a change of ideas in the public mind, the defeated invention may later gain a speedy acceptance. The great leaders of mankind have well understood these facts, and before trying to put their plans into execution, have either waited till times became ripe through the natural course of events, or have

¹ LeBon, *The Crowd*, pp. 78-79, London, 1900.

sought by direct instruction to develop in the public mind, mental systems that would support their plans.

9. The most important factor in institutional heredity is none of those above discussed, but another, viz, repetition—unceasing repetition with, of course, full command of attention. The groundwork of a civilization is a few fixed beliefs which have been thoroughly wrought into the mental constitution of a people by incessant repetition. The stress which has been laid upon imitation as one of the most fundamental facts of social life, does but enforce in particular words the importance of repetition in giving stability to the ideas, concepts and beliefs of a race. Society maintains a vast disciplinary agency whose sole purpose is to instill into the minds of the young the fundamental facts and values of its culture as data upon which immediate action is demanded. In this routine of the common, oft-repeated experiences, lies all that is most vital to the welfare of a people. Art has largely drawn its themes from the realms of common experience and in this fact lies its suggestiveness. The experiences repeated from the earliest years of childhood, organize into mental systems that require a minimum of stimulation to arouse them; they form a delicate consensus in the way of a sensitivity to the genius of one's civilization which a foreigner never fully acquires.

10. The degree to which feelings of relationship interpenetrate a mental system, has much to do in determining its strength. In a highly organized system, each part reflects and supports the other; so that one part is never in the focus of attention without there being an excitation to some extent of the others. Owing to the reciprocal action going on between the parts, the system is kept from dissolving and in readiness to function as a unit. A series held together by mere contiguous adhesion has no more strength as a total system than the weakest bond existing between any two members; nor does the increase in strength in the connection between any two members improve that between the others. On the other hand, in a system which is a manifold of numerous relations, it is impossible to modify the connection of any two parts without involving the others to an extent proportionate to the number of inner relations. The

best examples of these systems are those expressing quantitative relations as mathematical demonstrations, mathematical theories of physical phenomena, etc. The theories of mathematical physics may fall or stand with a single fact, there being at times a delicate dependence upon quantitative relations within very small limits of error. The corpuscular theory of light gives an admirable general explanation of refraction, but fails when quantitative relations (the index of refraction), are taken into account.

11. In general we may divide mental systems into two distinct classes so far as their cognitive elements are concerned, the group of perceptual data constituting the so-called 'facts' of a science, and the system of concepts by means of which the mind apprehends the facts. The concepts are in reality the laws or principles of the particular science. Now the progress of science means not only the multiplication and more exact determination of perceptual data, but the extension and deeper organization of theory as well. While the theories of a science are conditioned by its data, theory returns to condition the discovery of new facts; for as science develops, the discovery of new data is less a matter of accident, and more a matter of rational procedure based upon existing knowledge. Notwithstanding the intimate relation between fact and theory, their elaboration represents partially independent historic movements: that is to say, the accumulation of facts may go on for a considerable length of time before any need of the revision of hypothesis or theory is felt, just as a further improvement of a theory is possible with reference to the sphere of existing fact. Both the sphere of fact and theory become more coherent with the progress of science. A fact before it is admitted as a datum in the body of existing knowledge must be repeatedly verified; while a theory before it can gain an ascendancy, must submit to critical experimentation devised for the specific purpose of testing the theory.

12. Conceptual systems are not however limited to branches of knowledge with which the idea of science is especially connected. We find the great fund of social knowledge, religious, political, economic, arranged in more or less articulate schemes,

based upon some principle or concept. Experience combines into systematic totals long before the plan of combination becomes explicit in the public mind. In such cases the universal elements of experience do not exist apart from the concrete totals whose plan of combination they determine. What is termed practical sagacity or wisdom consists of mental systems organized in this way. Later the universal elements are disengaged from their concrete embodiments and are explicitly stated in rules. We find for instance in the universality of custom the operation of general factors or tendencies but dimly comprehended, which emerge later in special moral precepts of the practical understanding and again in more fundamental principles of ethical science in the way of certain norms. In the sphere of industrial activities, at times new practices spread through the social medium by the imitation of a particular model, and in this way certain general factors are at work, causing concrete elements to combine into similar wholes. Later these universal factors become explicit in a new concept as in the case of the capital concept in modern times. While the multiplication of concrete social acts goes on with the growth of civilization and the increase of population, the number of distinct universal principles serving for the organization of experience, does not exceed a certain small number. This universalizing activity of the mind corresponds to what is, objectively considered, the discovery of laws of greater generality. In truth, within any sphere of fact already a matter of social acquisition, the growth of culture means the replacing through combination and substitution of empirical formulæ by a smaller number of laws possessing a correspondingly higher generality. Progress in the evolution of conceptual systems is partly a matter of combination and partly a matter of substitution. Many theories which have appeared in the history of science are mutually exclusive; others have resulted from the synthesis of empirical generalizations that have covered partial phases of a group of phenomena; still others have resulted from a more precise quantitative statement or detailed application of an idea already developed in its general features. The later part of the history of events that led up to the discovery of the Newtonian

theory of gravitation well illustrates the progress of generalization through the combination of preceding hypotheses; while progress through substitution is seen in the replacement of the corpuscular theory by the wave theory of light. Substitution then occurs in the case of theories covering the same range of phenomena; combination occurs where theories formulating the order in the component parts of a system, are united in one more general theory valid for the whole system.

13. Conceptual systems comprehending special determinations and having numerous inter-relations, are difficult to displace for the reason that the mutual excitation of parts multiplies the amount of mental energy available at any particular moment. But too high articulateness is at times a source of weakness. No theory ever does full justice to the facts, which must be pruned here and expanded there to fit into the ideal limits of a formula. It thus may happen that a theory is expressed with too much mathematical exactness, and gains so great an ascendancy over the mind because of its logical symmetry that really significant facts are ignored or transformed by such prepossession. The mental system, because of its completeness resists modification, and like a group of physical particles under high internal stress may fly to pieces when exposed to the repeated shocks which the progress of discovery causes. A lower degree of articulateness at times insures a higher degree of of vitality, because room is afforded for growth and expansion. Nothing enforces this point better than the inductive philosophy associated with the name of Darwin. Stated with circumspection and a manifest desire to do full justice to all the facts concerned, it never aimed at finalities but only tendencies highly probable, with the result that it has quietly assimilated the facts gathered in so many lines during the last half-century. So, too, in Roman and English law, we have two instances of legal systems whose universal fitness for defining the rights of man, has been due in a considerable measure to the absence of a certain degree of logical refinement.

14. The spread of an idea through the social medium is checked by indifference or opposition. Where there is lacking the mental system with which an idea has some points in con-

tact, it fails to command the attention. Scientific conceptions which formulate highly specialized experience, do not enjoy a currency beyond a small social group, for the reason that in the minds of the generality, the mental systems are wanting which can incorporate the idea. Owing to the superficial contact the mental processes aroused by the idea are very transitory. In the case of opposition, however, the idea stimulates the mind to vigorous action, in calling forth mental systems which have elements that resist the incorporation of the belief into the context of social thought. The various conditions affecting the stability of social groups are so numerous and in their joint action so complex that we find as a matter of history that very few beliefs are uniformly organized in the individual minds of society, but rather that corresponding to the external division of labor in social activity, there is an internal division of thought and feeling, making the social mind a complex of different component mental systems. As a result an idea encounters in its spread through the social medium a resistance varying with the stability of the social groups which antagonize it. In all minds some struggle goes on before the idea is assimilated: in some the assimilation is comparatively speedy; in others somewhat tardy. If the mental conflict terminates in each mind in practically the same way, in the incorporation or rejection of the idea, the struggle has been an individual affair. The transmission of an idea under these circumstances is like the onward movement of a wave in a homogeneous medium: as the wave retains throughout the same form, so each mind repeats the apperceptive activity. What we have here is the repetition of an individual process forming a total process of nearly identical parts. The total process corresponds to an aggregate idea of the individual mind. It is only in rare cases, however, that the transformation of social belief is accomplished by the quiet spread of an idea from one individual mind to another. Such peaceful solution occurs only with matters of obvious utility, where sentiment, habit and prejudice are of minor importance. Conflicts involving beliefs deeply rooted in social history, which because of their fitness to express the fundamental needs of humanity, have strong affective components, are of a more

bitter and violent nature. In proportion as an idea involves emotional interests, it is destined to encounter somewhere in its course firm and obstinate resistance. As soon as there is a division in society between those who oppose the idea and those who affirm it, the seat of conflict has passed from the individual to the social mind. In the minds of one party to the conflict the idea has been incorporated into the dominant universe of belief; in the minds of the other, the idea has aroused an antagonistic mental system. Now mental conflicts of a social nature may be settled in either of two ways: by discussion or by force. Solution by discussion occurs under a variety of forms, depending in part upon the particular nature of the conflict or opposition. The opposition may arise from ignorance. In this case the mental systems which can assimilate the idea, are wanting or imperfectly formed. The idea is sufficiently grasped to touch some universe of belief, but owing to its vagueness it discharges no further function than arousing and keeping ascendant the particular belief. If the belief has strong affective components it leads to practical endeavors that resist the spread of the idea. The removal of the conflict is a matter of education. The idea must be presented as a mental system in the process of unfoldment, like a scientific exposition or judicial opinion,—not as a total: for the component parts, representing a less complex mental synthesis than the whole idea, are more readily assimilated, and their interconnection in a belief follows, once they are firmly established. The mental systems on which the assimilation of the idea depends vary all the way from mere aggregates of general experience to organic combination of concepts in still higher universals.

15. The conflict may arise from the indeterminateness of the mental systems engaged, as between two rival theories that derive equal support from the rather meager data. The discovery of some pertinent fact puts an end to the struggle by suppressing one theory and confirming the other. Such contests are not likely to be spirited in an age of speculative caution when the scientific ideal of suspended judgment on matters not yet adequately investigated is being realized, though in the early history of science, when superstition formed the staple of

its pretences, disputation rather than investigation was the rule.

Other conflicts occur between mental systems through their connection with other systems which are antagonistic. The conflicts of moral precepts in particular cases are frequently of this sort. Moral conviction, for instance, may lead to refusal of a gift of money intended for some worthy end, if it comes from a fortune dishonestly accumulated. There is no conflict of a moral nature between the desire to use money for a worthy end and the desire to be honest. The conflict in the present instance arises from the peculiar concrete circumstances under which the two desires are conjoined. The removal of the conflict in a way that suppresses neither desire, is by setting them free from this particular conjunction and uniting them again as parts of other concrete systems.

16. Free discussion, however, is a mode of solution successful with only a portion of the public issues. Disputes which involve matters deeply connected with social welfare, are subject to legal control, being decided by a body constituted for that purpose. The psychology of prestige and obedience explains the mental processes leading to the solution of these conflicts. Lastly the conflict may attain such a degree of intensity through the feeling engendered that a peaceful solution is impossible and nothing short of an armed struggle can remove the division in the public mind. We have here the intrusion of a physical factor in the domain of psychological causation. The victorious idea in this more than in the preceding case gains an ascendancy through external constraint; and though not becoming an integral part of the mental life of the defeated party, does yet secure an outward conformity. The opinions and sentiments which only violence could suppress, are still secretly cherished; but they weaken as time changes the outward conformity into second nature. The consciousness of force owes its strength to the objective circumstances of its excitation: it does not represent an apperceptive synthesis, and hence does not reflect the inner constitution of the personality.

17. (a) A conflict of any social importance marks a stage in the collective mental life clearly separated from what precedes

and from what follows. In its external aspects, it forms a turning point in the history of nations. Even when the idea fails to be incorporated in the minds of one party to the conflict, society being reconciled to a permanent division of opinion, the conflict has not left the mental systems in the minds of that party in their former condition, for the points of contrariety have been emphasized and rendered more suggestible. In subsequent issues in which this belief is again concerned, either by itself or as a part of a more comprehensive movement, the heightened suggestibility of the points of conflict is destined to play a part in the trend of social thought.

17. (b) A conflict causes social thought to return upon itself. Without some object to arrest the flow of thought, mental life would move on under the inhibitions and reinforcements coming from the play of the forces of association. Opposition causes a backward movement of the social mind to the earlier phases of the struggle; and on this follow mental processes working toward a removal of the conflict, either in the way of the repetition of the mental history of the conflict, or along lines outside of the historic development of the conflict. Much more in the social than in the individual mind is a mental conflict likely to extend to related mental systems; so that along with the fundamental issue usually go many collateral ones.

18. At times the deadlock in the social mind is relieved as noted above, not by an apperceptive activity, but by a moderation of the ardor of the strife, which permits a division of public opinion on the matter at issue. What differences of opinion are tolerated, depends upon what society regards as essential to its welfare. A theocracy punishes blasphemy as the gravest of offenses, because it feels the religious faith of its people to be the strongest social bond. Intolerance arises from the ascendancy of an idea which has considerable internal strength but which is under no systematic control through an interconnection with other ideas in a more comprehensive mental process. Not until the idea encounters resistance so that its immediate command of the will is checked, does the intolerance abate to any extent. Mental conflict raises the status of an idea above that of a mere impulse to action, by bringing it into connection

with other ideas, and thus marks the beginning of rational, apperceptive control. Then the mental changes are manifest on the social side in the weakening of the authority of custom. "As far as it goes, the mere putting up of a subject to discussion, . . . is a clear admission that that subject is in no degree settled by established rule, and that men are free to choose in it. It is an admission too that there is no sacred authority—no one transcendent and divinely appointed man whom in that matter the community is bound to obey."¹

19. In mental conflict the mind becomes aware of its own activity. In customs and usages are embodied unities of experience which the social mind does not clearly apprehend, though they exert a control on social thought through the felt similarity of one individual case to another. When some particular circumstance arises which because of its novelty, does not readily assimilate with existing customs or usages, mental conflict ensues, resulting in the synthesis of concrete social experiences into higher systems. It is here that emerge the general principles of experiences, moral, political, religious and utilitarian.

Now there are certain general limitations connected with apperceptive action that should be noted. History is irreversible. It is absolutely impossible to restore any past social epoch. The outward arrangements and appointments may be much the same, but the inner sentiments and feelings which clustered around the old régime, and which are the really vital elements, are gone forever. The experiences which have come with the intervening years, have produced a new background in the social mind.

20. Sentiments and ideas which express what is distinctive in the mental life of a people, cannot be bodily transferred from one culture to another. They must be transfigured into harmony with the national genius, if they are to be anything more than floating ideas or mere facts of memory. Hence a belief which represents a long social growth, cannot be assimilated by a foreign civilization in its subtler and more transitive phases. Scientific and mathematical conceptions are the least subject to national limitations, for the reason that they relate to a domain

¹ Bagehot, *Physics and Politics*, p. 161.

of experience which is apprehended by the least variable of human faculties, that of sensible cognition; but the delicate sentiments of social life which find their most adequate expression in the great works of poetic genius, are in a considerable degree the exclusive property of the particular culture.

CHAPTER IV.

THE CONSCIOUSNESS OF MORAL RIGHT.

1. The interaction of individual minds takes place with varying degrees of intimacy in relation to the self. As already pointed out, experience organizes in the individual personality into two widely contrasted universes, the physical or impersonal and the social or personal. The feelings which in concrete mental experience, are attached to these two universes, differ widely in their nature. The attitude of the self toward experience regarded in the mere light of fact, is one of disinterestedness. All facts then stand upon the same level so far as their value for the self is concerned: and if the self does exercise any selective preference, it is on account of the feeling which arises from the relation of one fact to another as members of a conceptual system. We have examples of these impersonal feelings of relationship in the feeling of harmony which arises when some conception dawns upon the mind that injects order and system into a mass of disconnected facts; in the feeling of scientific curiosity which impels the mind to seek further knowledge along some particular line, as well as in the feeling of wonder in the presence of something that contravenes the usual order of experience. The two attitudes are mutually exclusive, though alternation between them, at times even somewhat rapid, is in the general run of things possible.

2. When in the contact of one mind with another the point of orientation in the universe of intercourse lies almost exclusively within some cognitive system, the consciousness of self both in the way of idea and feeling, shrinks to a minimum. As the collective mental activity moves within the domain of processes of knowledge, ideas and feelings of relationship form the content of the psychic material communicated. The plane of communication may vary all the way from the bare excitation of mental images up to elaborate interaction of conceptual sys-

tems. In these cases the mental contact is at points on the universal side of experience, sharable by all individual minds. But even here that particular center of mental energy which we call the self is to some extent excited, since the impersonal feelings which color the given ideational processes, represent the reaction of the self as a totality, without involving an unfoldment of its parts. If under conditions of mental conflict an unfoldment of the parts of the mental system which constitutes the self becomes necessary, self-consciousness becomes explicit: the idea of self in its relation to other selves together with the various related feelings, comes into full view. Moral situations in particular are favorable to the evolution of the consciousness of self.

3. So long as mental life is such as arises in connection with responses to present stimulation, it stands on a plane of organization no higher than that of animal want; mental changes are then merely an incident in the mutations of experience resulting from variations in the objective order itself. When, however the reinstatement of former experience is possible from central excitation, under any form that will lead to the same practical result as did the original sense stimulation, animal want is passing into a higher mental organization of desire. And later desire becomes still farther removed from immediate practical volition when the reflective analysis of experience and the combination of the resulting elements into products similar to perceptual realities, take place. Desire then arises in connection with ideal universes. Many of these universes remain merely floating systems of the mind, in more or less complete detachment from a group of habits necessary to volition; others are brought into intimate relation with the will, and come thus to exert a direct influence on the turn which the pursuit of practical ends is likely to take. Now among these ideal universes is one of particular importance in the change which brings in the relation of the self to others: viz, that universe in which some type of personality is imagined and desired. The self is here viewed with respect to its inner organization, as composed of certain dominant desires and motives in partial abstraction of the external conditions which surround the self, whether these

circumstances are a matter of hereditary accident or produced by the will of the individual himself. It is not meant by this that the self sets the circumstances of its volitions over against itself as a mechanical opposite, but rather that it sees ramifying through the material changes which its volitions effect, a certain type of personality, the actual or realized self. If some other type of personality is preferred to that which the practical activities of the self reveal, to that which is really immanent in volition, a division exists in the personality on account of the presence of an unsatisfied desire in the form of an ethical ideal. Moral progress consists in the successive incorporation of such ideals into the universe of practical desire as they arise in the course of individual development. In other words, the mental universe in which some type of the self is imagined, must, in order to be an ethical ideal, command that practical assent of the mind which has already been discussed under the title of belief.

4. The universe of the ideal self is social in the sense that it does not contemplate an isolated self, but a self united by definite ties to other like personalities—ties which reflect very clearly the relations obtaining in the existing social order. The extent, however, to which the ideal universe is likely to deviate from the actual social world, varies considerably in the history of culture. Where there is great solidarity of the individual and the social will—where, in other words, the mental life of society is made up largely of processes of association, the deviation is slight. The social will constrains the individual will into a narrow conformity to a type, since individual and social aims are not clearly distinguished. The moral character consists largely of certain fixed habits of thought and action with their related feelings, being thus a will developed out of responses to concrete moral situations in which personal example has served largely as the guide. The morality of such an age is not a morality of reflection that comprehends broad humanitarian ends: it is tribal and sectional, yet withal a mechanism that responds with wonderful delicacy to the demands of that particular social life, serving besides as the indispensable basis of further moral growth. When the individual will comes to function in some degree as an independent volitional process, as it

does with the appearance of imagination, understanding and reason, the universe of self-consciousness, as already noted, includes as one of its ideational elements, the conception of an ideal self. To the extent that this representation is an integral part of that universe: that is, to the extent that it receives the support of self-feeling and its affiliated mechanism of well-formed habits, does it become a force in the transformation of social life. Conduct is then motivated by an end more or less clearly conceived, which serves as a unifying principle of mental life—unifying not merely in the logical sense of securing consistency within some group of ideas or concepts, but in the far deeper psychological sense of permanently satisfying the most urgent and fundamental desires of man. The moral consciousness at this stage of its development presupposes a complex social life in which there is considerable multiformity of experience. Out of a reflective analysis of the complex social experience, through which the individual is enabled to apprehend the universal elements of his civilization, grows the ideal self—ideal, and yet imagined as achieving its career under the special historic conditions of the social order in which the individual exists. The ideal self has, however, become a far more complex and fluid creation than was its predecessor of primitive times.

5. The social universe, actual and ideal, within which the realized and ideal self exists—the realized self in the practical conations of the mind and the ideal self in the imaginary universe of unsatisfied social desires—is the universe of moral consciousness. The ideational content of this universe is not its distinguishing trait although the general proposition is true that the ultimate psychologic fact of the moral consciousness is a personal idea. Moral action, in other words, is not action defined by a particular physical content, but action defined by the attitude of the self. The self here reacts to other selves as concrete totals rather than as individualized aspects of the general processes of social life. The self is truly moral so far as the welfare and experience of other personalities is included within its universe of practical motives. Whatever in the way of mental cultivation, material possessions and other externals

contributes to the efficiency of the self in willing and helping to realize the welfare of others, comes thus to have a moral significance. So far as the ends of the self include the welfare of others merely as an incident, the plane of behavior sinks to the level of prudential conduct; so far as the injury to others is object of direct or indirect volition conduct becomes immoral. In matters of merely prudential content the agent accepts other personalities as a psychologic fact and makes abstraction of all thought whether his relation to them makes for the attainment of the ideal in their lives. Conduct solicitous of the welfare of others is felt to possess, because of its psychic inclusiveness, the higher moral worth, securing as it does in the long run, greater breadth, richness and stability of individual life. It is in conduct based on the perception of an ideal that the self is most frequently thrown into the condition of mental conflict; and yet notwithstanding the pain and worry incident to this, the appearance of inclusive ideals as motives to volition is felt to mark a higher stage of moral development for the reason that the experience out of which the ideal is abstracted and which the ideal returns to illumine and unify, has then a deeper social significance. The relation between the individual and the social will, which heretofore had existed in concrete in particular volitions, now becomes a definite object of cognition, enabling the individual to enter sympathetically into hopes, sorrows, ambitions and disappointments of others. Aside from its foundation in a mechanism of firmly rooted habits, the moral will is largely a matter of the sympathetic imaginations. A life guided by rationally perceived motives is likely to be more stable and harmonious than one based upon immediate feeling; for in the latter case the consciousness of the unity of the self with the other is in the form of an isolated impulse with a corresponding lack of apperceptive control. The outcome then depends upon the adaptation of a preformed mechanism of native and acquired disposition to the particular situation.

6. Moral evil is essentially a condition of affairs in which individual and social life is narrow, restricted, unstable and inharmonious. It implies a condition of mental conflict,—a conflict, however, in which mental systems instead of uniting

into higher and more comprehensive groups, are dissolving into minor and fragmentary ones. Mental conflict, as we have already seen, is the fundamental condition of psychic development. It becomes more pronounced—not necessarily more turbulent or violent—as the complexity of the social personality increases. The partial wills through which the desires of social groups express themselves, are not of equal importance in the determination of the total will of society: so that it frequently happens that the will of a particular social group encroaches upon the wills of other groups, and in various ways makes its own interests ascendant in the collective mind. In proportion as these interests are simply group interests, the ascendancy has the effect of shrinking the general volume of social life, and thus perverts the course of moral progress, except in those early stages of social growth when the most important condition of mental development is the formation of definite habits of obedience to some authority. At times the ascendancy of a partial will means the dominance of an ideal capable of serving as the basis of more comprehensive social organization, and although imposed by force upon the antagonistic social groups, lifts them to a higher plane of social life if they are capable of assimilating the ideal. Conflict of this sort is evidence of an overflowing vitality, struggling to embody itself in new forms. On the other hand the conflict may grow out of the dissolution of social bonds that have previously restrained and coördinated the activities of the various social groups. Thus freed from subordination in any collective activity, the impulses of each group begin to assert themselves in a turbulent fashion, with much energy, perhaps, but with the energy coming from the dissolution of an unstable system.

7. Aberrations of the individual will which are a reflex of social disorder, must be carefully distinguished from the immoral tendencies of the individual will that are private. The individual caught in the maelstrom of social revolution, will give his sanction to deeds of violence which in times of ordinary peace and security he would blush to think of. The usual balance within the universe of motives has been upset by objective conditions. But even in times of social unity and concord,

there is a considerable number of individual wills which stand out in more or less conflict with the social will. Under these circumstances we have order and uniformity in the peripheral stimulations that the individual mind receives, along with a lack of harmony of thought and feeling. The conflict evidently results from some peculiarity in the inner constitution of the individual mind. In some cases the aberrant will is systematically controlled by a universe of motive which the social will unreservedly seeks to inhibit. This universe may be of considerable complexity, so far as mere intellectual relations are concerned, but in respect to personal relations and feelings it is poverty stricken. The purposes of the self are more or less coherent, conflicting, however, with the ends which society deems vital to its welfare. In other cases the individual will does not come into systematic conflict with the social will, and yet may considerably disturb the moral consciousness of the community. A will of this type is not actuated by a permanent universe of immoral motives—in fact it may possess a considerable wealth of social feeling: its deficiency lies in its impulsiveness. Irritating circumstances seriously disturb the mental balance, setting free an isolated impulse that does violence to the objective moral order. In a final category may be put those cases of delinquency which result from a general weakness of the mental organization. They do not come into contact with the deeper currents of social life to any serious extent; the resistance which they offer to the disciplinary agents of society is of a negative kind, consisting in a failure to return to the social fund any contribution for the energy which society expends in their care and maintenance.

8. Thus the immoral life reflects but a fragment of the social life or reflects it in an irregular and riotous manner. In moral crises through which the individual mind may pass, there is profound emotional disturbance; and yet in spite of this, numerous mental systems are excited whose mutual restraint prevents precipitate action. In the excitement of crime there is lacking such reciprocal inhibitory action: the emotional disturbance runs a serial course in which oftentimes a successive accumulation goes on terminating in a violent outburst of passion that accom-

plishes the act willed in such a horrible fashion as to surprise the agent himself.

9. Returning to the consideration of the ethical ideal as a variant of realized conceptions of conduct, we naturally encounter the question, What limit exists to the deviation of the ideal from the practical morality of the times? What is there to guarantee that the ideal which a nation sets before itself is something more than a chimera, perhaps luring it on to ruin and destruction? There is no *à priori* guarantee, although there are some conditions which help to predetermine the variability of ideals, or what amounts to the same thing, to make extreme forms relatively infrequent. In the first place, as explained above, the ethical ideal is a psychologic fact resulting from the normal workings of the human mind upon personal and social data. Any new moral principle or precept, or more inclusive application of some old principle, is first of all conceived by some mind of superior moral insight. The personality of the moral seer is an outgrowth of the common social life both in the way of hereditary equipment and acquired content: so that its idealizing activities are constrained to move within a definite circle of experience. Man's physical heredity guarantees on the average a native disposition adapted to a social economy, so that response to social situations is possible long before any intellectual apprehension of social relations is reached. Instinct finds expression in conscious processes in the form of desire, first in connection with purely practical acts concerned with vital ends, then in connection with apperceptive processes that aim at the removal of some conflict in the world of things, and still later at the removal of conflict within the world of ideas. But even in the universe of abstract relations, the desires of our common humanity come in to direct the imagination and the understanding in the construction of an ideal through which these desires are to receive ampler satisfaction. The moral reason views existing imperfections in their relation to an infinite process of development, and in this way satisfies in part the desire for harmony and order in the objective moral world. But it does not rest content with merely doing this. Though the intellectual processes excited by conflict in the universe of ideal desires are

in some measure independent of practical volition, the primitive tendency of the mind toward action never completely disappears. The moral will is preëminently a practical will, striving after something more fundamental than consistency of ideation—striving in fact after a consistency co-extensive with the personality in its thought, feeling and action. Again, an ideal far removed from the existing moral sentiment of the community, although it is a logical development of that, cannot command a passionate devotion from the people because of its lack of apperceptive contact with the social mind. The impractical moral ideal thus fails to find serious lodgment in the public mind, not only because it fails as a postulate of the practical will to unify the desires already ascendant in a more satisfactory manner than at present, but also because its pursuit involves too much pain and effort. The ideal must fit into the preformed mechanism of the social disposition and be able to organize the practical interests of life. There is continually going on a selection of ideals, and even at times a selection of the idealist. Preoccupation with ideal interests, to the neglect of vital conditions, brings into play the forces of physical selection. Competition between races with the resulting selection maintains a certain harmony between the national ideal and the national character. If the practical activities excited by the ideal are uniformly unsuccessful, the ideal soon loses its ascendancy. The Roman ideal of universal empire feeds on the success of Roman arms.

10. The utility of an ideal lies not alone in the harmony and consistency of life which it makes possible, but also in the hope and courage and through them the vigor of life which it inspires. One of the peculiar traits of moral feeling is the permanent satisfaction which it brings the individual, no matter what the external accidents of his career may be. The finely constituted moral nature feels that, no matter what obstacles have prevented it from achieving a career rich in material content, it has still nobly fulfilled its destiny in the world by putting forth every effort to live the moral law as it conceived the same.

11. (a) The individual cherishes an ideal not only of himself but of the social order of which he is a part. The interaction of these common desires of individual minds forms a collective

process that is a social ideal. The social ideal is generally much vaguer in its lineaments than the individual ideal, yet is frequently a powerful stimulus to action because of the social emotion which stands back of it. In the social ideal the collective mind expresses as clearly as it can the desires which it has in regard to its own constitution. The social ideal is generally most clearly understood by the ruling class, who are the chief organs in the selection of means and ends in the actual historic process in which the ideal strives to embody itself. But almost every individual consciousness feels to some extent national sentiments, no matter how lowly its organization may be. History, literature, myth, folk-lore, in short, all tradition relating to the deeper emotional interests, bring the ideal into more or less clear expression in the individual mind. We may thus speak with propriety of a national ideal so far as there exists in individual minds a common motive to volitions that aim at realizing a particular type of social personality. Usually the social personality moves on a much lower ethical plane than the more exalted individual wills. Nations, in their dealings with each other, have been actuated largely by prudential motives, for the reason that the national safety has been regarded as of supreme value. The broader sympathy which has followed from commercial intercourse and the ascendancy of a religion preaching the brotherhood of man, has mitigated somewhat this national egoism. We get evidence of a movement toward a higher plane of collective ethics in some matters of international concern in the idea that the lives of all nations have a moral worth in part relative to a collective process that embraces all humanity.

11. (b) A higher form of collective ethics means more comprehensive apperceptive control in the social mind. Moral conflict involves the mutual restraint as well as the mutual assistance of mental systems which are components of the personality. In a healthy condition of public morals, when rights are carefully protected through the strict and impartial enforcement of the law, the desires and volitions of the various social groups are harmoniously combined in the social will. There is a full and complete synthesis of the claims and interests of the various groups according to accepted standards of right. On the other

hand when there is a systematic violation of the rights of any class, we have a partial and incomplete synthesis of mental processes in the social mind, due to the suppression of the desires and volitions of the social group. The suppressed volitions assert themselves as soon as the restraint is withdrawn, causing a renewal of the mental conflict. In such periods of public disorder, motives of the social will do not restrain each other but add their energy one to another along the line of violence and confusion.

12. The moral springs of action are kept in a healthy condition only by effort. The social will in its effort to restrain the individual will into harmony with itself, strengthens the habits on which its character is founded and invigorates its emotional life. All social institutions rest upon conflict in the processes of the social mind, since through conflict the social will is spurred on to constant endeavor, and thus escapes the penalty of idleness,—extinction and decay. The resistance of the physical gives us our industrial economy: of ignorance, our educational economy: of evil, our moral economy. I speak of these as economy: for running through them is the law of rational effort which aims at a maximum of achievement for a given expenditure.

13. The primary ethical feelings of love, friendship, duty, obligation, attach most firmly to the ascendant personal universe of the individual. What particular group of persons shall constitute the personnel of this universe, depends upon the factors which determine the strength of mental systems. In the early stages of social growth, the ascendancy lies in the tribal association. Other forms of association like the family are too unstable to furnish a definite set of experiences that will organize into a permanent universe of personal or social relations. The tribal union is the instrument most concerned in the preservation of social life in the early struggle for existence; and round it are associated the most vivid, most intense and most frequent of social experiences. The result is that the consciousness of tribal ends forms the most stable mental system in the individual mind, attracting to itself the moral feelings that are the first to appear in social progress. Further mental develop-

ment of the social personality brings changes tending in two directions: one which is intensive, permeated by the intense feeling of natural affection, resulting in the partial will of the family group; the other extensive, embracing a multitude of personalities within its scope, the will of the state. The will of the family group is the first form which appears in that process of infoldment in the social will that gives us ultimately an individual will which has ends of its own, and which enables the social will to find self-conscious expression. The collective result of this process of infoldment is a rational social will declaring itself through the agency of the state. Corresponding to these two wills of the family and the state, are two systems of moral motives in the individual mind, differing somewhat in their constitution. In the sphere of family relations the most fundamental motive is the feeling of natural affection, the tendency of which is toward a complete obliteration of individual and group interests, as separable factors, while in the sphere of civic relations the sentiment of law, or the feeling of legal right, is the basic motive—a motive of the understanding in which more or less complete abstraction is made of the concrete determinations of personality, and the individual is viewed simply as the subject of certain rights and reciprocal duties.

14. Certain ceremonies, in particular the ceremony of adoption, helped to extend the rigid limits which custom imposed on the morality of primitive times. It is but natural that the stranger should be practically without rights according to the standards of primitive morality, for the reason that the savage sees the individual only in the light of tribal ends, and so excludes the stranger from the scope of social feeling. The thought of the stranger as the member of some unknown or hostile tribe arouses in the mind of the savage a real mental conflict, just as soon as the feelings of common humanity begin to prompt him to extend to the stranger the privileges of his family or clan. The device of adoption according to the primitive way of thinking changes the personality of the stranger in a manner satisfying to the demands of tribal safety, and creates between the stranger and the savage a system of reciprocal rights and duties sustained by the common impulses which the tribal will aroused in their

minds. Adoption extends the sphere of obligation without impairing the strength of the feeling, since the ceremony itself is a solemn affair by which the sanction of the tribal gods is obtained for a more intimate personal relation. It makes the limits of social feeling more elastic, but could evidently work only little change in the mental disposition of the tribe, founded as it is upon rigid customs. Far more powerful for change is the contact brought about by war or commerce; especially if political union under a single head is the result, provided the two cultures are equally virile, or nearly enough so, in order that one may not completely extinguish the other. With the thorough intermixture of the two races, goes on a corresponding incorporation of their cultures, resulting in a more complex civilization, and a greater openness to foreign influences. The individual moral universe has been both broadened and deepened: the virtues common to the two cultures have now a wider social validity, while each culture has contributed some virtues peculiar to itself.

15. Besides the rules of conduct valid for the entire social order, there are special rules valid only within some particular social group. This necessarily results from the fact that social groups have desires and sentiments of their own. Certain virtues are more fundamental to the life of one group than they are to another. The differentiation in class morality is for obvious reasons pronounced when social classes are rigidly separated from each other by hereditary lines. The desire of the superior caste to maintain their ascendancy leads to the adoption of elaborate ceremonials and usages which make the difference in rank plain to the eye and so accustom all to the thought of rank as a just and necessary principle of social organization. Moral respectability lies chiefly in meeting the ceremonial exactions of one's caste. A socialization of caste morality begins when an interchange of thought and feeling between the different castes sets in. The movement of ideas is more rapid from above downward, as the intellectual fermentation in the upper social ranks is the more vigorous; but in compensation there is a slower and more massive flow of social feeling upward from the heart of the multitude which suffuses the whole social life.

The combined result is a system of national virtues that serve as a groundwork for the more special virtues of the various social classes.

16. A social life of manifold activities, characterized by a rapid interchange of ideas, is governed less by habit and more by a consensus of motive resulting from an openness to suggestions and impressions along numerous lines. Such openness is due to the increased range of individual experience which makes possible the elaboration of mental systems of varied content. The resulting increase in the breadth of sympathy leads to a change in moral values. The past no longer commands the obedience and loyalty that it once did, for its sanctions lose something of their former force, owing to a growing disposition to accept the institutions of the past for what they are worth as contributing much or little toward realizing the dominant ideals of the present. While the individual will has now vastly extended its realm of social relation, it has been to some extent forced to make abstraction of those details of personality which were formerly the chief source of inspiration and strength. Though the old feelings which clustered around the narrow but deep personal experience of family life have been somewhat weakened by the change, in their stead have come others which may even more constrain the individual will to self-sacrifice. The individual mind is assailed by an indefinitely larger number of impressions, and so far as these have a common tendency they organize in a cumulative way into motives of considerable strength. But the relaxing of the bonds of traditionary constraint which results from the openness of the individual mind to social impressions of numerous orders, makes possible a great number of ideas which may be operative in determining the activity of the imagination to the construction of a corresponding number of ideals. Some confusion then results in the matter of moral values. This invariably happens in the opening up of new lines of social endeavor. It becomes at times under such conditions difficult to subsume all the concrete acts of practical conduct under the old moral principles. The difficulty continues until through the mutual inhibition of antagonistic motives and the rational synthesis of harmonious incen-

tives, the will is supplied with some definite moral concept that sobers and tempers the imagination. Once that confusion exists as to moral values within a particular domain of social life egoism asserts itself in the more vigorous natures and causes self-deception in regard to the moral quality of actual achievement. When some conception emerges capable of harmonizing some of the new practices with the great body of accumulated moral precepts, the accidental and the transient, which are essentially the immoral complications in the situation, disappear and the social mind goes back in thought and feeling to the permanent ends which are the indispensable basis of all development.

17. The principles of morality thus seem to stand in vital relation to the mental health of the individual and social will. Certain disturbances of feeling are symptomatic of mental disorder: melancholia, for instance, indicates the severance of the natural relation between feeling and action: the mind becomes suspicious of itself, suspecting the sincerity of its own motives; minutely attentive to its feelings in themselves for no ulterior reason. In the excitement of mania, on the other hand, the mental life expands: projects enter the mind with astonishing rapidity, in utter disregard of physical possibility or of moral obligation. But out of the fury nothing permanent emerges, for the very violence of the emotive processes suspends the apperceptive activity necessary to efficient mental work. Contrast with these two conditions that of a mind sound in its moral constitution. In the latter we see a general hopefulness, faith, respect for self and others urging the will on to systematic and controlled endeavor. Much the same may be said of the social mind. Faith in itself, hopefulness of the future, reverence for the past, a sentiment of honor and of law—these in moderation are the indispensable conditions of high efficiency in the social will. Wide-spread corruption, especially if it occurs with a general state of social apathy and indifference, is an unmistakable sign of social decay. These conditions are of course reflected in the individual life; but such demoralization of individual life differs both in its origin and in its nature from personal degeneracy. The former is due to a lack of proper discipline from

the social medium; the latter to an internal derangement of the personality resting on an impaired physical heredity. But there are some cases where the two merge imperceptibly into each other: many succumb to temptation in times of profound social disorder and excitement who under less exacting conditions would round out a career of decency and respectability. There may be no impairment in the abstract conceptions of right, duty, law, justice, but the blunted moral feelings cause an impairment of the practical moral judgment, so that the moral identity of concrete acts that belong to the same moral category is not perceived. Moral conceptions become detached from moral attitudes and form a sort of floating mental system. Moral feeling "is a function of organization," writes Maudsley, "and is essentially dependent upon the integrity of that part of the nervous system which ministers to its manifestations as is any other display of mental function. Its sanction is given to such actions as are conducive to the well-being and the progress of the race, and its prohibitions fall upon such actions as would, if freely indulged in, lead to degeneration if not extinction of mankind." Moral feeling then is the most sensitive index of the mental integrity of the individual who has received a thorough training in a social environment enforcing rigorous standards of conduct. A delusion may seize a community and lead to official acts that violate all canons of justice and mercy, as did the witchcraft delusion at Salem; but seizures of this sort are usually ephemeral, as history testifies. They do not excite the apprehension that a delusion of the individual mind does, because they are largely a matter of the intellect, aroused by external excitation, while a delusion of the individual mind exists in spite of the innumerable impressions of the social environment which tend to inhibit it. With this goes a derangement of the affective life which does not accompany a belief merely erroneous. But in spite of these exceptions, the general proposition remains that the immoral life both public and private is the life of disorder, confusion, violence and weakness.

CHAPTER V.

THE CONSCIOUSNESS OF LEGAL RIGHT.

1. The social will, in its composite organization, lacks the unity of the individual will in action, though motivated by more numerous and more comprehensive ends. As a mental system it is composed of minor groups that can function in far greater independence of each other than is the case with the contents of the individual will. The affective processes are likewise much more delicately balanced in the individual personality than in the social personality. We find, for instance, a frenzy of excitement spreading through a community which in the case of the individual would indicate grave mental disorder in the way of systematic mania. An emotion in spreading through the social medium undergoes considerable mutation as it spreads from one social stratum to another and even from one individual to another within the same social stratum. Take the instance of a piece of legislation which affects certain property rights. The social classes who possess no property view the matter with comparative indifference, while the particular group whose interests are threatened oppose in anger and indignation the proposed legislation. The emotive attitude of the individual mind toward a situation is for the moment unitary, but emotive processes in the social mind may at a particular moment possess all shades and variations. Now we have seen that the moral consciousness is essentially the personal, the personality in some form or other being the end of action. It is then evident that the very condition for moral contact in its higher aspects, is an intimacy of individual contact by which each comes to have detailed knowledge of the personality of the other. As the sphere of contact widens, the finer and more delicate adjustments of friendship cease, partly because of a lack of intimate knowledge of other people, and partly because of the absence of certain emotive processes, instinctive in their nature, which

the bare notion of social relations would not excite to any extent. Intimacy sometimes arises from a community of interests, as in the case of certain voluntary associations; but this is generally feeble in comparison with that which arises out of the instinctive needs of humanity. Still more attenuated is the personal feeling when we pass to economic organization, where men enter into coördinated activities largely from a desire for certain physical objects capable of satisfying human want. The contact there is reduced down to a sphere of ideas relating to physical processes. In so far, however, as in any of these spheres of contact, the personality of the agent comes in as a conscious factor or condition, rights and duties are created. Obligation attaches to the contract between the employer and the laborer, as to the rate of wages, hours of work, etc., not because of the laborer's position in the industrial organization as an economic force, but because in and through the terms of the contract ramifies the personality of each. Now it is evident that an emotion which arises from intimate personal knowledge and relation can never be a motive to the social will in the full sense of the term. In order to rise to the level of social motivation, an idea must arouse those mental systems which are organized with approximate equality in each individual mind. The social will, in so far as it is truly social, aims at ends valid for the whole social organization, and never makes the individual as such the direct object of volition, or, in other words, the personality of the individual comes in merely as an incident in the pursuance of general ends. The juristic personality is merely an abstract conception, a construct of the understanding and reason, and is a convenient fiction by means of which legal relations are systematically thought of. Thus the law specifies certain mental qualifications necessary for testamentary competency, and in doing so takes account of certain well-known facts of human nature: but even in the judicial application of the law, where the law comes in contact with the concrete personality, the history of that personality is a matter of concern only in so far as it helps to determine whether the given individual possesses the specified soundness of mind. The law is interested in maintaining some and suppressing other systems of personal relationship more than it is

in the instrument of these relationships, the concrete personality, although it is constrained to operate through the latter in doing so. This, of course, is the ideal of what the law should be, rather than the practice which actually obtains, though it is a psychologic fact like any other ideal.

2. The first way in which the social will acts upon the individual will is in the way of restraint, through the mechanism of the law. It arises out of conflict between individual wills which disturbs the consciousness of right of the community and especially the consciousness of right as organized in the government. The psychologic difference between the consciousness of law as it exists in the mind of the community and in the mind of the government may be expressed by saying that in the first case it has more of the nature of a sentiment and in the second more that of volition. The fundamental condition for this inhibitive reaction of the social will is the externalization of the idea in the individual mind in the form of some act, since this is the only way in which an idea in the individual mind can set going a collective process. The reaction of the social will has a double effect: one on the individual will and the other on the social will itself. The individual will has the consciousness of its guilt brought before it, and at the same time through the special discipline to which it is subjected, may undergo an educative process that leads to the formation of a character as harmonious, at least, to the social will as the condition of personal liberty demands. In other words, the excesses of the individual will are brought within the limits of tolerance of the social will. The crime conflicts with the consciousness of law in the public mind. If the crime goes unpunished, there is a blunting of the feeling of legal right. As soon as a community habituates itself to letting crimes go unpunished, the feeling of hostility which the infraction of right arouses, weakens, as no feeling remains intact which becomes permanently disjoined from the corresponding act. Punishment maintains the integrity of the feeling of right in the social mind, because it furnishes a definite channel for the expression of the feeling and thus avoids the paralyzing effect of what would be otherwise a mere emotional fermentation, besides emphasizing the concept of the right.

The social will also comes into contact with the individual will in a more positive fashion, performing functions that are more creative than those of mere inhibitive supervision. The social will as organized in the state sets before itself certain specific ends that aim at realizing a certain type of social personality. The state appears as an undertaker in various enterprises whose magnitude is so great and the return for which is so remote and imperceptible that private associations are not disposed to assume the risk. The most powerful motive in private association, outside of religious impulse, is the economic one. As economic value rests upon scarcity, implying limitations in the powers of production, there are many things desirable because of their social utility whose pursuit cannot be brought within the scope of this motive. Many lines of endeavor which from the standpoint of immediate economic return, appear insane and delusive, like the chemical tinkering of the alchemists, do yield in the course of centuries enormous returns on the energy invested. Back of all economic activity must be a sentiment of patriotism, of religion and of kinship. A high industrial economy comes only after centuries of effort during which an intense race spirit and consciousness has been fabricated. An ardent race feeling is an absolute necessity for survival in the struggle for existence going on among competing social groups. Let a nation lose faith in itself; let it become indifferent to its own past achievements, and view without emotion the sacrifices of its martyrs and heroes; let mutual trust and confidence, sympathy and virtue pass away and its doom is sealed. A national song or hymn, a work of great poetic genius—in short, art in all its varied forms gives articulate expression to the deepest hopes and aspirations of a people and so converts a vague emotional tendency into a social volition that molds the national life and raises the plane of collective endeavor both in the way of its spiritual and material achievements. In the encouragement, direction and control of the social agencies making for the higher life—even in some cases directly supervising them—the state appears as the most efficient instrument of the social will.

3. The contrast between the legal and the moral in regard

to personal relations, pointed out in the opening paragraph of this chapter, calls for further consideration. We may speak of the impersonal character of certain social relations, not in the sense that they are relations holding of impersonal, i. e., physical things, but in the sense that the universe of intercourse between the minds concerned is a mental system in which abstraction is made of the specific determinations of personality. When a judge, for example, renders a decision in strict accord with the merits of the case, as we say, he is treating the matter in a way as objective as a scientist does the facts of nature. His personality is simply the instrument through which the social will expresses itself in respect to the point in question. Of course self-feeling may be present but not in the way of prejudice against or sympathy for the party to the case: it is present in connection with the ideal he has of himself as the impartial administrator of the law. But in accordance with the fact that any mental system shares in some degree the unity of the personality, there is reflected into the decision to some extent the personal bias of the judge, either unconsciously through the limitation of habit and temperament, or consciously with a knowledge that such is the case. The result is dependent upon the special circumstances. In the case of the intended departure from the strict demands of the law, if it is done from prejudice, passion, hatred, and other egoistic motives of no moral worth, positive mischief is the probable result; if the departure is made from a consideration of the higher moral ends of social welfare, and if no injustice is done thereby to any particular person, undisputed gain follows in the way of making the law as a whole a more efficient instrument of social progress. The unintended modification of the law goes on through judicial interpretation. The law at best can only deal with classes of acts, with only very general reference to the material conditions of the act: so that acts are continually occurring which differ so much in their material circumstances from anything previously coming under judicial cognizance, that the highest legal insight is demanded in dealing with them. Right here is an opportunity for the development of professional and technical opinion through the collaboration of decisions that grow

and expand in the course of time to such an extent that a new right has been created. In this collaboration of judicial opinion are acting also those general social influences modifying the spirit of the entire legal system. But it is the manifest ideal of justice to reduce the personal factor to a minimum, and to do this certain checks and balances are introduced in the judicial machinery. In some cases appeal is permitted; in other cases the verdict is a collective decision of a court consisting of several judges. A collective verdict is held to minimize personal bias by broadening the view of the case through the interchange of judicial opinion and the cancellation of irrational or chance factors. The law should correspond with the general sentiment of justice in the community, though it frequently happens that a law securing a fair average of justice in former times no longer expresses in many of its details the existing social sentiment. Judicial interpretation plays an important part in maintaining the vitality of the law by a process of slow but continuous adjustment to social changes. The legal sentiment of a community is in a healthy condition when it does not consist of the mere motive to preserve the legal system, but of a willingness to obey it because it is felt to be the most important objective condition in the moral progress of mankind.

4. The consciousness of law varies in its organization in the minds of the various social groups. It is most sensitively constituted with respect to those rights the maintenance of which is most necessary to the ends and purposes of the group life, as determined by the psychological conditions governing the strength and stability of mental systems. Out of the experiences which enter into the composition of the dominant universe in the mind of the group, imagination develops an ideal which the group seeks to impose upon its members—the ideal through which the group-conscience declares itself: nonconformity to which on the part of the individual will calls forth the reaction of the group will, sometimes in the way of personal disapproval, at other times in the way of compulsion exercised through the channels of the law. It is in those fundamental points round which the self-feeling of the group flows, that the individual is likely to have the clearest consciousness of right as defended

and protected by the law. The consciousness of legal right, if it really figures as a motive to the will, is something more than a mere system of abstract concepts of the rights and duties enforced by the law: back of these must be the self-feeling which arises when something is felt to be an important condition of the welfare of the self. It is especially on the side of feeling that the vigorous enforcement of the law strengthens the consciousness of legal right. If officials are derelict in their sworn duty, allowing an open violation of the law, the injury done extends far beyond the incidents of the particular case; the feeling and respect for all law is weakened in the public mind. Not only is the strict enforcement of the law in those cases in which the state takes the initiative in punishing, of fundamental importance in keeping alive the feeling for the law in the public mind, but perhaps equally important is the action of the individual in asserting his right in cases where the state leaves the enforcement of the right optional with the individual. Where the individual puts forth his own effort to assert the supremacy of the law, although it may be from a purely egoistic motive to pain his assailant or recover material damages, and not for the sake of the law itself, still his own consciousness of law is strengthened and through his example that of others. In fact the failure to assert the right impairs to some extent the moral vigor of the individual concerned, for his self-feeling has been aroused without issuing into appropriate action. The willingness to abandon one's own right, even if the law views it with indifference, is incompatible with that sturdy and manly sense of justice and of law demanded of every citizen of a democracy. A virile sense of right in the minds of the great mass of people is the best safeguard against the prostitution of public interests by corrupt officials, for it makes demands of them in the way of rectitude and honesty that a timid spirit, hesitating to demand its dues, does not.

5. The energy with which the feeling is aroused in a conflict of right, depends upon the conditions governing the interaction of mental systems. The invasion of a legal right is a special instance of conflict in the social mind. Some act is committed which cannot be incorporated with the system of rights main-

tained by the law: the act is an objective fact, a system of perceptual relations, which arouses into full or partial activity the system of ideas and feelings constituting the consciousness of the right. Within certain limits the consciousness of legal right is strong in proportion to the frequency with which the law is invoked to resist the invasion of rights. Where the legal institutions have been developed largely as the result of national effort, the nation is likely to have a healthy sense of justice. Nothing confirms this proposition better than the history of legal development in England, in particular that of the common law which is a vast body of rights developed through judicial decisions determining particular concrete rights. A people which has much to do with the creation of its own laws, has its consciousness of the fundamental principles of moral right and justice largely bound up with the consciousness of law. A class which has never made use of the law as a means of its material or moral welfare, has no conception, much less a feeling of right. The rights which it does have, are due largely to the good will and conscience of the ruling classes. Rights which have been thus conferred, and not won by effort, do not rest upon habits of action firmly rooted in the personality of the class whose welfare they sustain. Aggression does not call forth a moral reaction in the form of disturbed feeling of right, but only the response of animal hostility. Too much litigation may, however, impair the consciousness of right. The enforcement of rights through the agency of the law is a rough and harsh process; and in the pain and irritation which is necessarily incident, the moral feeling for the law is frequently lost in the feeling of revenge or animosity. As soon as the law is severed in thought and feeling from the consciousness of moral right, it may degenerate simply into an instrument serving no purpose but to vex and harass an enemy.

6. The vividness and intensity of the experiences connected with the establishing of a right, are important factors. That which has been acquired at much cost and sacrifice, usually stands in intimate relation to self-feeling, and has thus strong affective elements back of it, helping to maintain its ascendancy. A right gained through profound social disturbances is firmly

implanted in the memory and affection of the people: they feel that they have put into it so much of their very life and being that it is an integral part of themselves. A whole array of powerful sentiments and emotions—family affection, patriotism, religious feeling—is brought to the support of a right acquired at the cost of the nation's blood and treasure.

7. The comprehensiveness of the interest involved affects the stability of a right. A right which ramifies through the whole social structure, will call forth proportional resistance to any disturbance. If the given right is enjoyed by all social classes, it is a motive to the whole will of society, and as such can command more energy to resist its invasion than only a particular right. The zeal with which a people rush to the defense of the national honor when assailed by a foreign power, shows the strength of motives which are supported by a tide of emotion surging through the entire current of social life.

8. A right derives strength from its interconnection with other rights in a system of rights. Custom makes for self-consciousness in a community in that it expresses a usage known to be valid for the whole community, but the custom precedes and does not follow as the result of a clearly conceived idea in the social mind. When, however, specific declarations of the social will exist in the form of statutory enactments, society has arrived at a very clearly defined idea of the various ends of its own existence, which form a relatively coherent system. There emerges the consciousness of a system of law, over and above the concrete laws which form its content. The idea of the legal system comes to be a motive and leads to efforts looking to the maintenance of the legal order itself. A law then acquires a force as being the member of a system, beyond that which comes from defending some particular right.

9. Where the enforcement of the right is a matter of option with the individual, there is a likelihood that he will look at the right and the consequences which flow from its invasion, largely from the standpoint of his own private interests, ignoring the wider social results in the way of the reflex influence of the enforcement of a particular right upon the consciousness of right in the mind of the community. So long as the enforcement

of the right is left to the individual himself, it is subject to all the caprices of individual temperament: personal feelings like revenge, anger, jealousy, fear, come in to operate independent of impersonal standards. There will consequently be irregularity, excess, deficiency of action in the enforcement of the right. The first step toward increasing the security of the right, consists in surrounding the penalty which the injured party may exact with certain limitations; here the violence and excess of the individual will is checked by the social will which declares itself through the particular rules. The volition in which the individual exacts vengeance, comes to include social motives that subject the emotive elements of the volition to some restraint. The right of punishment is first felt in the history of legal development as largely an individual matter. The socialization of the primitive power exercised by the individual in avenging his own wrongs reaches its final stage when the right of punishment is taken entirely out of the hands of the individual and vested in some regularly constituted authority which represents the community in the case. The changes which have occurred in the meantime in the individual and public consciousness of right, are numerous. In a general way moral conceptions have become more clearly defined in the social mind: the social mind has reached some consciousness of itself as a moral entity, in both idea and feeling, as is attested by the fact that its own will is now authoritative in the punishment of crime; while the individual now views crime from a wider social standpoint, seeing that conceptions of certain social standards, apprehended as valid for all minds, now enter into the composition of his idea of crime. Further, the state in making general provision in an authorized agency for the punishment of crime has been actuated by motives of general validity, and not by feelings and ideas arising out of a particular case. Thus the state has provided in the personality of its agents a psychic device for the judging of crime which makes accidental feelings and ideas, such as arise in the mind of the injured person, as little determinative as possible. Of course feeling is necessary in any volition, whether public or private; but it is a very important matter for legal development whether such feeling be objec-

tive, relating to the mechanism of social acts, or subjective, relating to the personality of the individual in relative detachment from the social whole.

10. Mental conflict has played a considerable part in the development of legal right. Those rights for which society maintains an elaborate protective agency, are evidently vital to institutions and relations which society regards of fundamental importance. The value which the social will attaches to them, is measured by the vigor of the resistance it offers to the invasion of them. In the domain of legal right the social disposition is receiving articulate expression in certain specific declarations of the social will. The mass of social experience which is here in function is so great that the resulting mental inertia resists change. Old legal rights are thus imbedded in psychic mechanisms of considerable stability. Many of the most fundamental rights have been secured only by revolutionary means, that is, by profound disturbances in the social personality in which not only have old habits been broken up but new conceptions of social and political values have emerged. But the mental conflict involved in the creation of legal rights, is seen not only in those more profound psychic movements which involve more or less the whole social will, but also in those more limited mental processes in which the will of the individual or group is concerned. Legal rights which are judicial affirmations of pre-existing customs and usages, have arisen in these minor conflicts of the individual or group will. The experiences of effort and struggle, which accompany the mental conflict, act as powerful motives to maintain the right during the first stages of its ascendancy. As the assertion of the right becomes more and more a matter of habit, these experiences lapse from memory, and leave the right a rule whose origin is no longer felt. In the absence of direct historic instruction to the contrary, the individual born into an old social order in which thorough harmony existed between the legal institutions and the national character, would be ignorant of the nature of the real process by which the idea of a legal right rises to the status of volition in the public mind.

11. The extent to which the individual feels the law as a

constraint depends upon the degree to which it restricts the spontaneous flow of thought in his own mind. If a strong desire arises in his mind that runs counter to the law, his processes of thought are blocked, and the idea of the law as an objective condition of conduct comes into full consciousness. While the psychological nature of these conflicts is much the same as that of any other conflict, the moral significance of the conflict varies considerably.

12. The atomistic way of conceiving social relations looks upon the state largely as a coercive agency standing over in somewhat mechanical opposition to the individual. The power of compulsory subordination which the state possesses is of course one of the fundamental incidents of its constitution, but to emphasize this aspect of state function to the exclusion of others, shows a serious lack of insight into the mental nature of society. Civil institutions are from such a standpoint merely artificial arrangements which men can enter into as freely and dissolve as freely as they do a business corporation or any other purposive association. Those processes in the mind of the individual in which he thinks, feels and wills the part of a citizen, are, however, possible only because his mind and the other minds of the particular political order, interact in a collective mental process that extends backward through many centuries. The same historic development which has given legal and political institutions and relations, has created the citizen as the subject of legal and political rights, with his love of liberty and his sentiment of law. It is only in a social order that a personality can exist, i. e., a psychic entity in which the consciousness of its own being is a motive; and of course there is implied in all this corresponding institutions that maintain objective conditions that render this motive effective.

13. In the desire for freedom the individual does not view himself as isolated from others, but as a member of a mental community in which a certain range of free determination of individual volition is secured through a system of reciprocal rights and duties. The practice of freedom must to some extent precede the desire for freedom. An animal is sometimes said to "desire its freedom" through a courtesy of speech that has

no strict regard for psychologic fact; but in the light of the discussion of desire given in a previous chapter, it is evident that the motive impelling the animal to escape its confines is a feeling of unpleasantness resulting from impeded action. A social desire like the desire for civic freedom can arise in the individual mind only as the result of its interaction with other minds in which the same desire is simultaneously developed, and thus presupposes certain social practices and experiences out of which the mind can elaborate it. The routine of social experience necessary to the development of definite and fixed legal and political concepts implies objectively modes of conduct generally obtaining in the social order, and corresponding rights with a regularly constituted agency to enforce them. The common consciousness of right means, therefore, a general harmony between the volitions of the individual will and those of the social will, though at times conflict at some points of contact occurs.

14. The particular institutions through which the feeling of right expresses itself, define and make real the feeling by connecting it with definite forms of activity. The individual, so far as he is aware of the authority which by common consent is vested in these institutions, has a conception of a common end which is being furthered by their enforcement, and must on the whole regard them as equally indispensable to his own well-being, though at particular times he may be blind to the truth on account of some conflict with certain of his inclinations. To the extent that such is the case obedience to the existing standards of right is a matter of free determination of the individual will and not of constraint by a motive externally determined and deriving but little support from the apperceptive activities of the mind. The beginning of the consciousness of legal right as a moral condition of man's existence, is obedience to the social standards of right expressed in custom. The social preparation which the discipline of custom makes for the higher morality of duty, consists in forming habits that inhibit some desires and in connection with these, a consciousness of a common welfare. A considerable portion of the motives which make for obedience to law, consists of habits formed with refer-

ence to concrete situations arising in the various private associations of which the individual is a member. It is here because of the more simple character of the social relations, that the fact of a common life is most clearly conceived and strongly felt. The opinion that the individual in passing from the life of the simple social groups to the life of the larger group of a political society, is completely bisected in his personality by the ascendancy of an entirely new set of motives, is at variance with all the laws of psychic causation. Undoubtedly a man in his function of a political subject is actuated by motives different in some respects from those dominant in his mind in other spheres of association; but still a considerable amount of psychic material in the way of habit and idea is carried over from private lines of social endeavor to public ones. Participation in the common life of private associations, tends to develop a consciousness of a collective well-being and a willingness to subordinate in some degree individual preferences and inclinations to the will of others, which will stand one in good stead in his civic relations.

15. The organization of the consciousness of a common well-being varies considerably in the history of culture. In the early periods of social development when the social will comes to expression in the individual consciousness chiefly in the form of impulsive motives, the collective is felt rather than clearly apprehended by the intellect. Historic effects have been organized in the social mind in habits and simple ideational systems in which only the most obvious relations of cause and effect are conceived. Any social movement is understood only through the changes it produces in the concrete relations of one individual to another. In the emotional excitement which at times sweeps through the social order, the individual passes through experiences of unusual intensity and is borne onward by a stream of life which he is powerless to resist. But up to this point the consciousness of a common life is a psychological, not an ethical fact. The individual is moved to action by motives externally excited, or by those which spring directly out of the character or disposition and form a system of practical tendencies of the will. It has in it no ideal of a common

good. A somewhat higher stage is reached, not only so far as the intellectual apprehension of social relations but in a more important way so far as moral development is concerned, when the particular relations through which the individual apprehends his connection with the social whole, are thought of as a common good in which he and others share in realizing some ideal of self. Whenever the law steps in to protect the individual from the invasion of these rights, he has direct demonstrable experience of the law as a practical condition of the moral order. Indeed the firm connection between the idea of the law and volitions operating within the practical relations of life is the necessary element in any effective consciousness of legal right, no matter what the individual's theoretical insight into the law as a social factor may be; and it is really the absence of such association that in part makes possible the criminal mind. A weak mental connection between the idea of the law and practical volition indicates that the law is not fulfilling its true mission in the social order: it indicates some serious lack of harmony between the national character and the legal institutions. This is the case when partial or group interests become ascendant and use the law with all its accumulated energy to constrain the social will into harmony with their demands; or when a nation imposes its own laws upon a subject people without any regard to native law or custom; or again when in the moral decay of a nation, insincerity and apathy have seized a people, and there is wanting the zeal and energy to enforce a system of laws adapted to a sturdy and resolute character. Provided there is widespread peace and security in the enjoyment of the ordinary private rights of business and the other common relations of life, considerable harmony between the legal institutions and the social disposition may exist, with but little of that higher idealism in the consciousness of law that makes the individual take an active and watchful interest in the law as the most efficient means of attaining the ends of collective welfare. He looks upon the state much as he does a business partnership, giving it his hearty support because he feels it as a practical necessity. Perhaps the higher civic idealism is not attainable in a social order where there is a sharp separation in thought

and practice between political and legal rights. However inefficient in some, perhaps many respects, democracy may be as a form of political organization, there is in it the possibility of the average person rising to a higher moral plane in his conception and feeling for the law than in a political order where the task of legislation and administration is the work of an hereditary class.

16. The consciousness of collective ends and existence becomes more precise in its ideational contents not only through the knowledge practically acquired through state affairs, but from many theoretical disciplines like history, political science and sociology. When to all this knowledge from whatever source obtained, is added feeling strong enough to raise the entire mental system to the status of a volition, a certain ideal of national existence has emerged that is to be a practical force in social life. And as the political organization is the medium through which social organization is most authoritatively expressed, this ideal is most intimately associated both in thought and deed with the activities of the state. This statement must not be taken to mean that the ideal of collective ends has been the sole motive or force acting to organize the social will in the state, for as a matter of history other motives of inferior moral worth have coöperated with it. The national ideal exists imbedded in fact in a complex of experiences relating to individual as well as social ends; but the fact of importance is that the ideal exists, whatever may be its connections with other mental states, and so makes to the extent of its ascendancy the political order a moral one as well. No state would rest secure even upon that higher form of selfishness, in which the individual looks at things from the social point of view on account of prudential motives. Egoism of itself tends to shrink the social horizon and obscure in the individual mind conditions and relations that are necessary to the realization of his own ambition. The calculation of purely personal advantage weakens the strength and spontaneity of action which social life in so many of its circumstances demands. A certain interest for the social life in and of itself is a necessary condition for that enlarged grasp of social relations without which a career successful in

merely external achievement is possible. A motive aroused solely by external constraint, i. e., one which excites an act not because of its general harmony with the constitution of the personality but because of its strength, could not be relied upon to render uniform support to the interests of the state; for as soon as the external circumstances which excited it pass away, the feeling and desires which represent the organized tendencies of the self are sure to assert themselves. It is precisely at times when the political order needs the most defense that a motive of this sort could be least relied upon. Mere restraint could never create a personality as an organized system of thought, feeling and volition; and to the extent that any institution rests upon coercion, is the mental life back of it narrow and impoverished. Coercion is justified at times simply because the repetition of a thing, even when done under compulsion, forms a corresponding habit that may later function as a part of a process which in its totality is free and rational. Despotism for this reason has a sanction in the ethics of political development.

17. Thus the state is not to be conceived as something over and above the humanity which manifests itself through social institutions. It is a collective phenomenon of a certain character existing in and through the reciprocal relations which the members of society sustain to each other. Society as organized in the state differs from society as organized in other forms in the ends of its existence and the mode of attaining such ends. The supreme compulsory power which is an incident of the constitution of the state, derives social significance from the fact that it is compulsion exercised according to law, maintaining a system of rights which are necessary to a collective life.

CHAPTER VI.

THE SOCIAL WILL AS EXPRESSED IN THE STATE: THE THEORY OF SOVEREIGNTY IN ITS PSYCHOLOGICAL BEARINGS.

1. The attribute which the state possesses, of being in certain cases the most authoritative expression of the social will, is generally spoken of by writers on political science as that of sovereignty. It is particularly in this field of social phenomena that political science and social psychology have much of their data in common. For this reason, no doubt, political writers have been prone to express their views on the authority of the state in psychological terms. In fact, in the writings of some authors we find a more or less clearly conceived psychological theory of the state. But on the whole the history of the theory of sovereignty reveals much confusion and vagueness, largely on account of an inadequate social psychology.

2. There are two facts of social psychology which are basic to an understanding of this subject: one is the reality of the individual will and the other is the reality of the social will. An attempt has been made in the preceding pages to show that the reciprocal interaction of individual minds in a community is a collective process having certain definite features; that only in virtue of such interaction is it possible for the individual mind to come to even a slight realization of its own powers; but that although so far as its total history is concerned, continuous social contact is implied, there is yet an apperceptive organization of experience which renders it relatively independent of immediate stimulation. Now there are some things in the interaction of individual minds which lead to nothing very definite along the line of conduct, such as vague sentiments and various forms of emotional excitement. So long as such is the case the public mind is in a state of confusion which continues until in the process of interaction some individual mind is reached in which are aroused apperceptive activities that issue in a clear

idea or concept, and in this way convert a vague emotional tendency into a volitional process that is social in its scope. In other words an individual personality is necessary to give articulate expression to social processes of thought and feeling before any definite resolution of conflicts in the social mind is possible. Once that a specific conception emerges at a point in the social medium, provided it satisfies existing mental conditions, it is forthwith incorporated by some system in the social mind. This "particularizing" function of the individual personality, to use a term of Professor Baldwin's, seems to be the truth in the contention of certain writers that sovereignty resides always in a determinate body of persons; and so far as this is the case, they have shown deeper insight into the constitution of society than others who hold to an extreme universalism that does away once for all with all individual determination.¹ But it is possible to conceive the independence of this particular body of persons in too absolute a manner. Atomism in psychology and ethics leads readily to atomism in political philosophy, in accordance with which sovereignty is likely to be thought of too much as physical coercion, commanding obedience through the motive of fear with but little regard to the real needs of human nature which a political order satisfies. The particular personality through which social desire comes to articulate utterance, is an outgrowth of the social life and can be considered as the source of authority only in the relative sense that it makes an individual contribution to a social tendency by converting the latter into a more or less clearly defined movement.

3. The form of social control which writers on political philosophy have in mind when speaking of sovereignty, is one which comes rather late in the history of society. It presupposes a consciousness in which society has arrived at some recognition of itself as a psychic entity, with defined concepts of values in the various spheres of life, and some regularly constituted and authoritative means of putting into execution its preferences. The volitional processes in which society declares its will through the mechanism of the law, are complexes which in their totality are consciously constituted with respect to par-

¹ The Austinians, e.g., in contrast to the French "Doctrinaires."

ticular conflicts, yet contain parts which are automatic and stable. Many of these automatic connections have had a long social history, and were developed by association in the period of the ascendancy of custom. These factors, which the usual legal discussions overlook, are of fundamental importance in a psychological analysis of the motives of political obedience.

4. It has frequently happened in the history of a people that a partial social will—the will of some social group—became ascendant in a universe of social relations and constrained the great mass of individual wills through fear into certain unwelcome modes of action. Such constraint, however, has usually not extended to all departments of social life, but only to certain external acts the performance of which left the deep undercurrent of social life undisturbed. The vast stream of social sentiment, desire, hope and ambition cannot be completely focalized in any particular body of determinate persons. For this reason despotisms even the most pronounced usually leave their subjects free in the general course of events in their domestic and religious life. Its constraint is felt chiefly in the levying of tribute and impressing in the military service. In other spheres the obedience of the people is due to sentiments and feelings of a pervasive nature that reside rather in the consciousness of society as a whole than in any one individual mind to the exclusion of others. So far, however, as these sentiments and feelings do come to clearer expression in some personalities than in others, such personalities are centers of influence not because of a power of compulsory subordination which they possess, but because of their representative function in some association. The influence which they wield may at times constrain individual wills, relatively few in number, to acts that they would not do of their own volition; but on the whole the influence is possible simply because the people feel it to confirm and strengthen their habitual sentiments and beliefs. Authority, so far as it is despotic, i. e., so far as it merely represents the will of a single person, or a class, and commands obedience through compulsion—acts as a block to the flow of mental life at the points of constraint, but the resulting mental conflict does not lead to a healthy apperceptive activity in which mental

systems are united into a more comprehensive and unified system, but simply to an arrest that means aversion. It is immaterial whether the despotism proceeds from a native or alien source, for the mental results are the same. Both attain their ends by inciting the public mind to acts that do not flow freely from its inner constitution. There is a suppression of sentiments and feelings which under conditions of freedom would operate to inhibit or perform certain acts. Extended to all departments of life as a real principle of control, compulsory subordination would ruin all creative mental activity on account of the encroachment upon apperception by the habits of servility in thought which are developed through mere compliance with external authority. The body may become the mere passive instrument of physical force, but the personality never, for the very condition of its manifestation has been removed.

5. The ascendancy of the political power may be less coercive and in more intimate contact with the social personality if it exercises its will through the preformed disposition of society which is mechanized in the ancient rights and customs. The supremacy of English power in India furnishes an admirable example of this more organic form of political and social contact. According to Bryce,¹ the English administrators in the time of Clive and Warren Hastings, found in the native law, "First, a large and elaborate system of Inheritance and Family Law. . . . Secondly, a large mass of customs relating to the occupation and use of land and of various rights concerned with tillage and pasturage, including water-rights, rights of soil-accretion on the banks of rivers, and forest rights. . . . Thirdly, a body of customs, according to our ideas comparatively scanty and undeveloped, but still important, relating to the transfer and pledging of property, and to contracts, especially commercial contracts. . . . Fourthly, certain penal rules drawn from Musulman law and more or less enforced by Musulman princes." "In this state of facts the British officials took the line which practical men, having their hands full of other work, would naturally take, viz: the line of least resistance. They

¹ *Studies in History and Jurisprudence*, p. 98.

accepted and carried on what they found. Where there was native law, they applied it, Musulman law to Musulmans, Hindu law to Hindus, and in the few places where they were to be found, Parsi law to Parsis, Jain law to Jains.”¹ Since that time many acts have been passed codifying and amending the native law, so that as a net result we have “the new stream of united law which has its source in the codifying Acts”² and “the various older streams of law, each representing a religion.” Thus an ascendancy like that of British power in India, while exercising but little direct coercion, really touches the life of the subject people in a more vital manner than a military despotism would. Instead of shrinking the mental life at the points of contact by an inhibitive and contractile motive like fear, it really amplifies it by giving greater stability and certainty to the flow of native sentiment and feeling. English administration and legislation has caused most changes in the native law in those spheres where the native law was either meager or where because of its peculiar nature it was comparatively flexible. The modification has been relatively superficial in those social relations rooted in deep habits and strong sentiments.

6. In passing to cases where the law and the power that enforces it are equally indigenous to the particular society; where the law has had a slow and continuous growth through trial and experiment in which the people themselves have played an important rôle, we find the points of contact between the legal institutions and the national character correspondingly multiplied. There is here also a determinate person or body of persons to whom in the first instance the making and enforcing of laws is due; but here the ascendancy is due more than in the other instances to the fact that the organized personality of society finds on the whole an adequate expression of its will through the instrumentality of such a body. Political power is now a force that is concerned in the maintenance and protection of rights that meet general approval; in other words, with the maintenance of general social conditions that are necessary

¹ Bryce, *op. cit.*, p. 99.

² *Ibid.*, p. 105.

to a certain freedom of volition. When a power of this sort exists, which "prescribes forms and obligations to all minor purposive associations, and shapes the social composition,"¹ it may be fitly described as the organ of the social will, since it is the medium through which habits and desires of the social mind come to expression in an authoritative way, i. e., in a way that commands the usual obedience of individual wills.

7. While the usual flow of social thought and feeling comes to regular and definite expression in the declarations of a determinate body of persons, in time of serious disturbance it frequently happens that no particular body of persons can be pointed out as the one in which ultimate authority actually resides. There is then no social will. Social volition has become merely social emotion; and it would be as rational to apply the term will to the ravings and fury of a madman as to the public mind in a state of excitement. If there is any collective tendency discernible in social thought and feeling, it arises from the fact that some of the various motives competing in the public mind through their massiveness and strength turn the tide of social emotion in a particular direction. Under such conditions there is no organized public sentiment and no particular will through which the social will declares itself. The usual motives of obedience which at other times unite to produce a consciousness of a commonweal, are out of function. The effect of such a state of the public mind is seen objectively in the suspension of private rights by which individual volition enjoys an amount of freedom and encouragement otherwise impossible. Some order emerges from this confusion when the points of conflict become clearly defined, and society divides into two contending parties, each having a degree of organization. There is now no sovereign power, for there is properly no social will in the sense of an organized volitional process growing out of the controlled interplay of individual minds. What we really have is a division of the social personality with the mental processes slightly organized about two competing centers. When society is thus divided into two hostile camps, there is usually a con-

¹ Giddings: *Principles of Sociology*, p. 174, ed. 1896.

siderable contraction in the volume of national life; for commerce and other pursuits are partially paralyzed. Within the ranks of each party we see something of control and obedience: each party has a leader, a definite body of persons to which the rank and file render obedience—an obedience which at times is far removed from servility, being inspired by a keen sense of duty and a rational idea of common welfare and of the interests at stake. But still it is true that private rights are insecure under such conditions, even within the ranks of each party, so that the individual is frequently compelled to do things against his own inclinations, no matter how great may be his general sympathy for the cause he defends. Fear comes into the foreground of consciousness as one of the motives of obedience at times of martial control. When the mental conflict in the social mind is brought to an issue, the emotional excitement abates, desire is again subordinated to rational control, and a supreme social will once more emerges, having a definite personal medium for the enforcement of its declarations. The private rights, temporarily abridged, are restored, giving to the individual will its former, and perhaps greater, range of freedom and activity. Now we have a personal agency exercising supreme coercive power when necessary within the limits of a system of public and private rights. But the authorized agency of the state, viz: the government, has a personality in some degree its own, and for this reason does not usually act as a mere neutral medium of expression. Frequently its conduct is dictated by egoistic and personal rather than legal and social motives. Frequently too it acts as the mold of public opinion, awakening the social consciousness to new matters of general interest and importance so that the organ of sovereignty must be looked upon as a social group having a will in a measure peculiar to itself. It thus comes about that the sovereign power may enforce laws which stand in conflict with the social will, at variance with the general system of rights which guarantee the individual freedom and protection in his ordinary pursuits.

8. The particular way in which the forces of social life select the membership of the governing body, seems to stand in no necessary relation to individual freedom—at best only remotely

so. The opinion that a law in order to be an expression of the social will must have the sanction of a majority ballot, rests upon a superficial view of things. The arts of the demagogue may so push private interests upon the attention of the public that a majority opinion upon the matter in hand, as recorded in a general ballot, does not express a rational volition, but simply the desire of a crowd acting under suggestion; while the decision of a select body, even if its membership is hereditary, may more truly reflect the real will of society, in that it accords faithfully with the spirit of the total system of public and private rights obtaining under rational conditions. The essential thing is that the governing body shall be responsive to the hopes, sentiments and desires of the people as these organize themselves into definite and urgent opinions. That the governing body may maintain its organic connection with the social order, true to the mission it has of maintaining rights and enforcing obligations, some surety beyond its condescension and good will is necessary. There must be institutional limitations that act as a powerful restraint upon the excesses of the will of the sovereign power, making difficult any concerted action toward the usurpation of authority and keeping the people in an attitude defensive of their rights. Provided the individual has in certain spheres of life that freedom of volition necessary to creative mental activity, a people may rise to a considerable plane of culture though the legislative and administrative functions of the state are practically in the hands of an hereditary class. Custom generally acts as a sufficient protection against the complete abrogation of a right, but is a weak security against the invasion of the right so far as isolated individuals are concerned. But something more than this is imperative, if the state is to be an efficient association in realizing the moral possibilities of man: rights must be so secure that the humblest citizen can command the aid of the state in his defense with as little effort as the most influential. The modern system of representative government by which the personnel of the law-making body is in part directly determined by the choice of the people, has much to commend it as a limitation to despotic authority. It is the most effective instrument yet devised for converting public

opinion into a practical force in political control, besides being a discipline of the highest order in training people into habits of independence and self-reliance.

9. Between political and other social institutions history reveals a delicate interdependence: universally societies having a high mental organization as externally manifest in the creations of art, science, and industry, are states of corresponding political development, guaranteeing rights and engaging in lines of endeavor that make for the moral and spiritual perfection of man. Then law is something more than a rule imposed by an external authority; it is a system of rights collaborated by the nation in the mental conflict that on the one hand gives a social will capable of realizing certain collective aims and on the other an individual will organizing social experiences in its own way. The struggle has been more of a conflict between the partial wills of antagonistic social groups than between individual minds of the same social level. The cleavage in the struggle has usually begun in the upper social ranks of superior initiative, working an alignment on this side or that side of the point at issue as it moved downward through the various strata of social life. A vast upheaval from the social depths can occur only in the relatively late stages of social history, when a vigorous interchange of thought and feeling is going on among and between social classes. Some great leader may then emerge from the commons to voice their demands in tones that compel an extension of their legal and political rights and make the state still more the servant of the humble citizen in protecting his now greater sphere of freedom from invasion.

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THE EFFECT OF ACHROMATIC CONDITIONS
ON THE COLOR PHENOMENA OF
PERIPHERAL VISION.

BY
GRACE MAXWELL FERNALD

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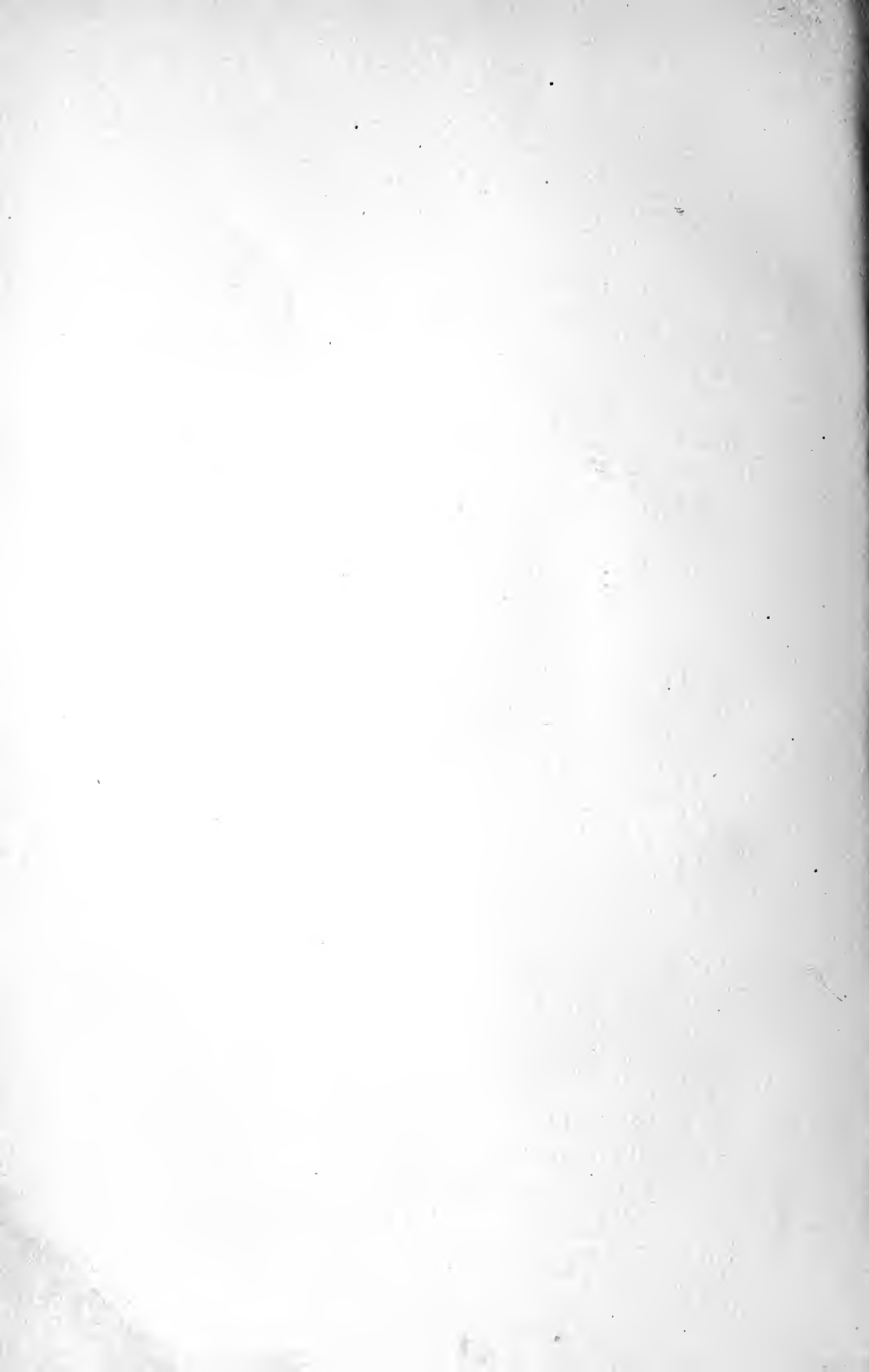


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THE EFFECT OF ACHROMATIC CONDITIONS ON THE COLOR PHENOMENA OF PERIPHERAL VISION.

BY GRACE MAXWELL FERNALD.

I. INTRODUCTION AND HISTORICAL SKETCH.

INTRODUCTORY STATEMENT.

The present investigation has as its purpose the determination of the effect of achromatic conditions on the color phenomena of peripheral vision.

Von Kries¹ has already shown that the Purkinje phenomenon, in so far as it consists of changes in the relative brightness of the different colors, occurs at the periphery. It has been shown by Miss Thompson and Miss Gordon,² as well as by the writer,³ that the brightness of a colorless background has a decided effect on the tone of peripheral color stimuli and on the appearance and relative frequency of the after-images which follow these stimuli.

In the present investigation a part of the previous work has been repeated and verified; observations have been made under more varied brightness conditions than those employed in our previous work in order to determine (1) what effect these achromatic conditions have on the color phenomena, and (2) just what factors are responsible for the changes observed;

¹ J. von Kries, 'Ueber die Farbenblindheit der Netzhautperipherie,' *Ztsch. f. Psychol. u. Physiol. d. Sinnesorgane*, XV., 1897, S. 247-279.

² H. B. Thompson and K. Gordon, 'A Study of After-images on the Peripheral Retina,' *PSYCHOL. REV.*, Vol. XIV., 1907, pp. 122-167.

³ G. M. Fernald, 'The Effect of the Brightness of Background on Color in Peripheral Vision,' *PSYCHOL. REV.*, Vol. XII., 1905, pp. 386-425; *ibid.*, Vol. XV., 1908, pp. 25-43.

finally, an attempt has been made to correlate the phenomena observed with the facts previously established concerning peripheral and central vision, under conditions of light and dark adaptation respectively.

I wish to express my gratitude to Professors James R. Angell and John B. Watson, of the University of Chicago, for suggestions and assistance throughout the entire investigation. I am also greatly indebted to the observers, instructors and students at the University of Chicago and at Bryn Mawr College, for the time they so generously gave to the work.

HISTORICAL STATEMENT.

A. *Methods of Investigation.*

Any historical account of the work on peripheral color vision presents a series of seemingly irreconcilable statements. A study of the methods employed in the various investigations serves, however, to explain why such a state of affairs should exist.

As Baird⁴ has given an exceedingly accurate and complete history of the experimental work up to the spring of 1905, it will only be necessary here to state the actual differences in methods which may affect the outcome of the investigations, and to give a somewhat detailed description of some of the more recent papers which have direct bearing upon the special problem to be discussed in this monograph. A brief section is also given on the effect of brightness adaptation.

I. APPARATUS.

The perimeter has been employed in all the more important investigations of the dark-adapted peripheral retina, as well as in some of the earlier investigations of light-adapted vision. With this apparatus the eye fixation remains constant, while a small sector of the stimulus color is moved in or out along the quadrant of the perimeter arc. The advantage of this method is that the eye takes a fixation straight ahead and does not have to turn to the side when the peripheral retina is stimulated.

⁴J. W. Baird, 'The Color Sensitivity of the Peripheral Retina,' Carnegie Institution of Washington, May, 1905, pp. 80.

In daylight illumination, however, there is a change in the brightness of the color at different points on the arc, unless all the illumination comes from a skylight.

The campimeter has been used in many of the investigations in daylight illumination. The advantage of this apparatus is that it makes possible a more extended background than could be employed with the perimeter and an easier control of the brightness of the stimulus in daylight.

Among the more recent investigators, Hess and Kirschmann used the campimeter in daylight illumination, while Hellpach and Baird used the perimeter in the dark room.

2. METHODS OF OBSERVATION AND STIMULATION.

(a) *Moving and Stationary Retinal Image.*

Either the eye or the stimulus was moved during stimulation in all the determinations of the color limits up to the time of Hellpach's work in 1900. In the investigations with the perimeter the eye remained stationary while the stimulus was moved in from the periphery to the center of the visual field, or vice versa; while in the work with the campimeter the eye followed a moving fixation point and the stimulus was stationary. An exception to this latter statement occurs in the case of Kirschmann's work on the campimeter. He maintained a stationary fixation and moved the stimulus, thus constantly changing the size of the retinal image. Hess also varied the method slightly by covering and uncovering the stimulus while the eye followed a moving pointer.

In any case the constant movement of the retinal image would be open to the following objections: (1) The observation would be difficult owing to the constant shift of the image; (2) the portion of the retina stimulated at a given moment would not be entirely unfatigued; (3) the constant movement of the image would make it very difficult to record exactly the points at which the various phenomena occur.

In the work with the campimeter, eye movement during stimulation introduces still further difficulties, as it is now well established that eye movement is necessarily irregular and also that vision is influenced by eye movement.

A further reason for employing a stationary fixation and a stationary stimulus is that this is the only method which is adapted for the observation of after-images, either in peripheral or central vision.

In the investigations of the peripheral retina, Hellpach and Baird used the method which was also adopted throughout our work, namely, that of stationary fixation and stimulus, with an interval of two or more minutes between stimulations.

(b) *Interval Between Stimuli.*

If the method of stationary fixation and of stationary stimulus is employed, the question at once arises as to the length of the interval which should intervene between stimulations. Hellpach allowed a three-minute interval. Baird claims that three minutes is not long enough to overcome fatigue effects and consequently he allowed a six-minute interval.⁵ Miss Thompson and Miss Gordon allowed a two-minute interval in the first half of their work and a five-minute interval in the remainder. They found no difference whatever in the results in the two cases. In all our work only a two-minute interval has been allowed. The reasons why a longer interval seemed unnecessary have been given in a previous paper.⁶ They are briefly as follows:

1. Though gray, white and black stimuli were frequently used, they always appeared colorless.

2. The stimulus was never seen as the color complementary to the previous stimulus, but always gave a perfectly characteristic response.

3. The after-images when experienced were the characteristic after-images of the immediate stimulus and not of previous stimuli.

4. As shown by the work of Miss Thompson and Miss Gordon, the results were in no wise altered by increasing the length of the interval.

⁵ Baird found that, when stimuli are given in too close succession, a stimulus sometimes appears as the color complementary to the previous stimulus (*cit.*, pp. 57-58).

⁶ *Journal of Philos., Psychol. and Sci. Method*, Vol. III., pp. 352-353.

(c) *Retinal Area of Stimulus.*

Several attempts have been made to determine the effect produced by a change in the size of the retinal image. There is a decided disagreement in the conclusions reached as a result of these investigations. The experiments of Woinow⁷ and Krükow⁸ show no change whatever in the limits of the retinal fields due to change in the size of the stimulus. Kirschmann⁹ found the limits for all the colors greatly widened on the upper and nasal half-meridians, but little changed on the lower and temporal, as the size of the retinal image was increased. All the other investigators who have worked on this problem agree that increase in the size of the retinal image results in a widening of the limits for all the colors.

It is quite possible that the lack of agreement may be due to the different amounts by which the stimuli were decreased as well as to the difference in the size of the original stimulus. It seems probable that, starting with a minimum visible stimulus and gradually increasing the size of the stimulus as much as is practicable, different phenomena would accompany various degrees of change. It is also true that in most cases, there is no way of determining the relative brightnesses and saturations of the stimuli used by different investigators, so that either or both of these factors may be at least partly responsible for the lack of uniformity in the results.

(d) *Urfarben as Stimuli.*

Bull¹⁰ (1881) found that there are only four colors which do not change in tone as they are moved from the center to the periphery of the visual field. These colors are a blue-green, a purple, a yellow and a blue. The colors were equated in saturation by using such proportions of the complementary Urfarben that 180° of one cancelled 180° of the other. This

⁷ M. Woinow, 'Zur Farbenempfindung,' *Von Graefe's Archiv*, XVI., 1, 1870, S. 212-224.

⁸ Krükow, 'Objective Farbenempfindungen auf den peripherischen Theilen der Netzhaut,' *Von Graefe's Archiv*, XX., 1, 1874, S. 255-296.

⁹ A. Kirschmann, 'Die Farbenempfindung in indirectem Sehen,' *Philos. Stud.*, VIII., 1893, S. 592-614.

¹⁰ Bull, 'Studien über Licht- und Farbensinn,' *Von Graefe's Archiv*, XXVII., 1, 1881, S. 54-154. Sur la périmétrie au moyen de pigment colorés. *Annales d'Oculistique*, CX., 1893, pp. 169-181.

method serves simply to equate the red with its complementary green and the yellow with its complementary blue. Unfortunately there seems to be no way of equating the saturation of the blue and the yellow with that of the red and the green. Bull found the limits for green and red practically coincident. The fields for blue and yellow were practically equal in extent, the field for blue being somewhat wider than that for yellow, and were in every case considerably wider than the fields for green and red.

Hess¹¹ (1889) determined the color limits for the Urfarben, after the complementary Urfarben had been equated with each other in saturation and all the colors had been matched in brightness to a neutral gray screen. He found the limits for the Urroth equal to those for the Urgrün and the limits for the Urblau equal to those for the Urgelb. The limits for the latter pair of colors were wider than those for the former pair.

Hegg¹² repeated and confirmed the work of Bull and Hess. The greater part of all three investigations was made with pigment colors in daylight illumination.

The only determination of the limits of the stable colors with the dark adapted retina is that made by Baird and reported in the monograph already referred to.¹³ For stimuli he used light transmitted through gelatin filters. The complementary Urfarben were equated in saturation and all four Urfarben were equated with each other in brightness. The coincidence of the fields of the two pairs of stable colors, already established in the case of light-adapted vision, was found to exist also in dark-adapted vision. Unfortunately there seems to be no way of determining whether the tone of the color which was the Urfarbe in light-adapted vision was the same as that of the Urfarbe in dark-adapted vision, nor whether, as has already been suggested, the blue and the yellow equalled the red and the green in saturation.

¹¹ Hess, C., 'Ueber den Farbensinn bei indirectem Sehen,' *Von Graefe's Archiv*, XXXV., 4, 1889, S. 1-62. (Also *Annales d'Ocul.*, CX., p. 177.)

¹² Hegg, Emil, 'Zur Farbenperimetrie,' *Von Graefe's Archiv*, XXXVIII., 3, 1892, S. 145-168; 'La périmétrie des couleurs,' *Annales d'Oculistique*, CIX., 3, 1893, pp. 321-347; 'Sur la périmétrie au moyen des pigments colorés,' *Annales d'Ocul.*, CXI., 1894, pp. 122-127.

¹³ Baird, Carnegie Monograph, 1905.

It seems to be established, however, beyond question that, under fixed conditions of illumination, there are four and only four colors which do not change as they are moved from the center of the field of vision to the periphery, and that when the Urroth and Urgrün and the Urblau and Urgelb, respectively, are equal in saturation and brightness, the fields for the Urroth and Urgrün and for the Urblau and Urgelb, respectively, are equal in extent.

(e) Brightness of Background.

In the work carried out in daylight illumination there has been as great a diversity in the backgrounds upon which the stimulus has been exposed as in the various other conditions. Very little work has been done for the express purpose of determining what influence the background exerts on the extent of the color fields and on the character of the color perceived.

Aubert used both black and white backgrounds and found that all the colors were perceived further out on the black background than on the white background.

Woinow and Krüchow insist that change in the brightness of the background, like change in other conditions, has no effect on the distribution and size of the color fields.

Hess carried out some experiments with backgrounds of different degrees of brightness. He states that the widest fields were obtained when the color and background were of equal brightness but does not give in detail the data upon which his conclusion is based.

During the years 1903-1905 the writer¹⁴ attempted to determine the effect of the brightness of a colorless background on the appearance of color stimuli and on the extent of the color fields in peripheral vision. During the years 1905-1906 Miss Thompson and Miss Gordon made a special study of peripheral after-images.¹⁵ As the method and the apparatus were practically the same in the two cases, the results of the two investigations will be summarized together. They are in brief, as follows.

¹⁴ PSYCHOL. REV., Vol. XII., 1905; Vol. XV., 1908.

¹⁵ PSYCHOL. REV., XIV., 1907.

α. Effect of the Brightness of the Background on the Extent of the Color Fields.—The limits for yellow and for carmine are much wider with the dark than with the light backgrounds. The limits for orange, and perhaps for red, are wider with the light than with the dark backgrounds. The limits for blue, for violet and for blue-green are little affected by changes in the brightness of the background.

β. Effect of the Brightness of the Background on Color-tone.—Red, orange and yellow stimuli are seen as orange or red with the light background at the same degree of eccentricity at which they are seen as yellow with the dark background. No similar effect was observed in the case of blue, violet and carmine.

γ. Effect of the Brightness of the Background on the After-image.—After-images follow color stimuli in a larger percentage of cases with the light than with the dark backgrounds. With the dark background, the after-images for blue and green appear orange, and for violet greenish-yellow, at the same points at which they appear yellow with the dark background. The after-image for red, orange and yellow is blue with both the light and the dark backgrounds.

(f) Intensity of Stimulus and Brightness Adaptation.

There is a general agreement that increase in the intensity of a stimulus extends the limits of sensitivity of a color. Baird concludes on the basis of his own experimentation that sufficiently intense stimuli would probably be seen at the extreme periphery.

Another effect produced by changes in the intensity of the stimulus, is that described in the literature as the Purkinje phenomenon. The discussion of this phenomenon is taken up in the following section on brightness adaptation. It is evident that intensity of stimulus and brightness adaptation cannot be entirely separated, since the intensity and character of the stimulus determine the adaptation of the part of the retina stimulated.

B. *General Discussion of Brightness Adaptation in Central and Peripheral Vision.*

The effect of brightness adaptation on the various spectral colors has been more fully worked out for central vision than for peripheral vision. The general name applied to the changes which occur is the Purkinje phenomenon, inasmuch as the first mention of them was made by him (1825).¹⁶ In his *Neue Beiträge* the following description occurs: "Objectiv hat der Grad der Beleuchtung grossen Einfluss auf die Intensität der Farbenqualität. Um sich davon recht lebendig zu überzeugen, nehme man vor Anbruch des Tages, wo es eben schwach zu dämmern beginnt, die Farben vor sich. Anfangs sieht man nur schwarz und grau. Gerade die lebhaftesten Farben, das Roth und das Grün, erscheinen am schwärzesten. Das Gelb kann man von Rosenroth lange nicht Unterscheiden. Das Blau war mir zuerst bemerkbar. Die rothen Nüancen, die sonst beim Tageslichte am hellsten brennen, nämlich carmen, zinnobar und orange zeigen sich lange am dunkelsten, durchaus nicht in Verhältnisse ihrer mittleren Helligkeit. Das Grün erscheint mehr bläulich, und seine gelbe Tinte entwickelt sich erst mit zunehmenden Tage."

Since this observation was made by Purkinje, the changes connected with varying degrees of light and dark adaptation have been made the object of extended investigations. Two methods of changing the brightness adaptation have been employed, (1) local and (2) general; *i. e.*, (1) the intensity of the colors observed may be varied with a minimum change in general brightness conditions, as for instance when a spectrum observed in a dark room is varied in intensity; or (2) the intensity of the stimulus may be varied by a change in general illumination,—when the illumination of a room is increased or decreased. In the former case it is necessary to suppose that the greater part of the retina remains practically dark-adapted, and that any change in the relative brightness and saturation of the colors is due to local adaptation. In the latter case, in which changes in the intensity of the stimulus are due simply to

¹⁶ J. Purkinje, 'Beobachtungen und Versuche zur Physiologie der Sinne,' Band II., 'Neue Beiträge zur Kenntniss des Sehens in Subjectiven Hinsicht,' 1825, p. 109.

the greater or less amount of light reflected from the colored surface, adaptation changes, similar to those which are found in the region of direct stimulation, must also occur over the entire retinal field. The results obtained under both sets of conditions agree in so far as they are concerned with the relative brightness and saturation of the spectral colors.

A spectrum of minimum intensity appears as a series of brightness bands, with the brightest section in the region of green (provided the eye is thoroughly dark-adapted), and the darkest in the region of yellow and red. As the intensity of the spectrum is increased the maximum brightness shifts from green to blue and finally, when still greater increase is made in intensity, to yellow—the appearance of the spectrum in ordinary illumination and brightness. The same general shift of brightness values occurs with pigment colors observed in a very faint illumination which is gradually increased to full daylight. The observation quoted from Purkinje (see p. 9) evidently describes the intermediate stage in which blue is the brightest color.

The order in which the colors are distinguished as the general illumination or the intensity of the spectrum is increased, does not correspond with the brightness shift. If the brightness of a spectrum of medium intensity is slowly decreased, red, blue and green gradually spread over the entire spectrum until they are the only colors visible. The red end is, in this case, the darkest part of the spectrum, the blue the brightest. As intensity is still further decreased, the green and then the blue fade out to gray leaving red as the last color visible.¹⁷

¹⁷ Accounts seem to differ somewhat concerning the exact order in which the various colors disappear as the intensity of the spectrum is decreased. The statement given above is taken from Ebbinghaus—'Theorie des Farbensehens,' *Ztsch. f. Psychol.*, Bd. V., 1893, S. 155. Helmholtz agrees with Ebbinghaus in giving red as the last color visible in a spectrum of decreasing intensity—Helmholtz, 'Physiol. Optik,' Aufl. II., p. 471. Mrs. Ladd-Franklin gives green and red as the last colors visible—Baldwin's 'Dict. of Philos. and Psychol.,' Vol. II., p. 796. W. von Bezold and E. Brücke first observed that yellow and cyan blue lose their color before red, green and violet-blue (cf. Helmholtz, 'Optik,' S. 469). Wundt states that blue and violet are the first colors distinguished in a spectrum of gradually increasing intensity, and the last to be lost under the reverse conditions. (Wundt, 'Physiol. Psychol.,' Aufl. V., Bd. II., S. 173.) For Purkinje's statement see p. 9 this monograph. All the above agree that yellow is one of the first

No especial stress is laid on any changes in color-tone which accompany the changes in brightness. Several writers, however, mention the fact that certain changes occur. Thus Purkinje states that green appears more bluish and consequently less yellow in faint illumination, and that, under similar conditions, yellow cannot be distinguished from rose-red. Ebbinghaus¹⁸ is undoubtedly describing a similar phenomenon when he speaks of the red, blue and green as spreading over adjacent colors in a spectrum of faint intensity. It is probable that the changes which accompany the darkening of pigment stimuli are analogous phenomena. For example, a red stimulus becomes deeper and more saturated and an orange stimulus becomes redder when darkened.

Undoubtedly the work done in the dark room, with spectral lights as stimuli, has been performed under more accurately controlled conditions, than when daylight is depended on for illumination. The changes, however, in relative brightness and saturation of the different parts of the spectrum are so pronounced that they can be illustrated very easily in a roughly conducted experiment in increasing or decreasing daylight illumination. Moreover, the experiments in daylight, despite any necessary irregularities in experimental conditions, deal with problems which cannot be investigated in any other way—namely, those presented by the phenomena of light-adapted vision.

Most of the investigations in peripheral vision with stimuli of different intensities, have been carried out in the dark room with self-luminous stimuli. Few of these have used stimuli differing sufficiently in intensity to determine whether the Purkinje phenomenon occurs at the periphery or not. Von Kries, working with self-luminous stimuli in the dark room, has shown that the Purkinje phenomenon, in so far as it consists of changes in the relative brightness of colors, exists at the extreme periphery beyond the limits for color vision: *i. e.*, if a red and a green appeared as matched grays at the periphery, the red colors to disappear as the intensity of a spectrum is gradually decreased, and all but Wundt and Purkinje agree that red is the last or one of the last to lose its color component under the same conditions.

¹⁸ *Cit.*, p. 155.

became much darker than the green when the intensity of both colors had been equally decreased by a certain amount.

The explanation most generally accepted, for at least a part of the phenomena just described, is that the development of the visual purple or 'rod pigment' during dark adaptation in some way heightens the brightness effect of certain colors, so that these colors appear relatively brighter than colors not thus reinforced.

Mrs. Ladd-Franklin and Von Kries hold that the effect of the rod pigment is due to its power to absorb certain colors and so intensify their brightness. In this case the brightness alone would be affected, because the rod pigment exists only in the rods which are the organs for colorless vision. According to this theory, the visual purple, in its completely bleached-out state (*i. e.*, in full illumination), has no effect whatever on brightness values. In an intermediate state of partial dark-adaptation, when the rod pigment can best be described as the visual yellow, it absorbs the blue light, so intensifying its effect on the rods and causing it to appear relatively brighter than the other colors. In its final stage of dark adaptation, the rod pigment becomes purple (hence the name 'visual purple') and absorbs green light, so intensifying the brightness of green.

Mrs. Ladd-Franklin explains the fact that blue and green, though relatively brighter than red, lose their color component at a higher intensity of stimulus than does red, by the overlaying 'of the color by the white constituent furnished by the rods.' Her statement is as follows:¹⁹

"In proportion as the blues become relatively brighter, they become also less saturated, and still more the greens as they become bright, become finally wholly uncolored. The reinforcement occurs, that is to say, not for the color in itself, but only by way of mixing in more white or gray. (This is sufficient doubtless, to account for the fact that in a very faint spectrum blue is not seen at all: The spectrum looks simply red or green, and this in spite of the fact that the Purkinje phenomenon is usually supposed to consist exactly in the brightening of this color. The blue becomes, in fact, so much overlaid

¹⁹ C. Ladd-Franklin, Baldwin: 'Dict. of Philos. and Psychol.,' Vol. II., p. 796.

with the white constituent furnished by the rods that it is no longer visible as blue.)”

The main reasons advanced for adopting this explanation for the Purkinje phenomenon are as follows: (1) The absorption spectrum of the visual purple, when extracted from the eye, has the same brightness distribution as the spectrum of the normal dark-adapted eye; (2) the visual purple is found only in the rods, which are supposed to be the brightness sensing end-apparatus (the purple would intensify the brightness effect of the colors which it absorbs); and (3) the Purkinje phenomenon is absent at the fovea which contains cones but no rods and consequently no visual purple. (For other reasons as well as differences in theory see articles of Mrs. Ladd-Franklin and Von Kries, referred to in the bibliography, p. 88.)

The changes in color-tone which accompany variations in brightness and saturation have not, so far as we are aware, been discussed in connection with any of these explanations. Mrs. Ladd-Franklin's statement as just quoted does not seem to offer a satisfactory explanation for the fact that an orange loses its yellow constituent and appears pure red when its intensity is sufficiently decreased, since the red and the yellow would have to be equally overlaid by the brightness constituent furnished by the rods and so would have their saturation equally affected by the decreasing intensity of the stimulus.

In the present writer's opinion Mrs. Ladd-Franklin's theory does not seem competent to offer a satisfactory account of the changes in the relative saturation of colors, or of the changes in the color tone of certain colors (see pp. 72ff.). In this respect, however, it does not differ from other color theories.

C. *General Summary.*

The extent of the color fields is influenced by various factors, such as the size of the retinal image, the intensity of the stimulus, the brightness of the background, and, in all probability, by the state of light or dark adaptation of the retina. The quality of the color stimuli seems also to be affected by changes in these factors, with the possible exception of the first, *i. e.*, the size of the retinal image.

The peripheral portions of the retina are less sensitive to colors than the central and paracentral regions. In general and under such conditions as have thus far been employed, all colors appear as blue or yellow or as colorless at the periphery, though red and possibly green may be perceived at a greater degree of eccentricity on a light than on a dark background.

After-images generally follow peripheral color stimuli in light-adapted vision, but are practically absent in dark-adapted vision. The color-tone of the after-image, in the former case, is influenced by the brightness of the background, the after-images for green and blue tending to appear more reddish and for carmine and violet more greenish with the dark than with the light background.

In addition to the points more explicitly mentioned in the preceding pages, it may be noticed that adaptation to colors (*i. e.*, such that the colors fade into gray) takes place more rapidly at the periphery than at the center of vision.²⁰ With the dark backgrounds or under conditions of dark adaptation, the colors, with the exception of the *Urfarben* (see p. 5), go through a series of changes in tone before they fade out to gray, the change being in all cases toward blue or yellow. With the light background no such change in color tone occurs, but the color fades directly into gray.

Under fixed conditions of illumination there are four and only four colors which do not undergo any change in color-tone as they are moved from the center to the periphery of the field of view. These are: (1) Purplish red, and (2) its complementary color, blue-green, (3) blue and (4) its complementary yellow. When the two pairs of complementary *Urfarben* have been equated in saturation and all four colors have been equated in brightness, the fields for blue and yellow coincide as do also the fields for red and green. The former pair of colors seem to have the wider limits, though up to the present time no method has been devised which gives any assurance that all four colors have been equated in saturation.

The Purkinje phenomenon, in so far as it consists in changes in the relative brightness of certain colors, occurs at the extreme periphery.

²⁰ Cf. Baird, *loc. cit.*, pp. 56 and 72; G. M. Fernald, *PSYCHOL. REV.*, Vol. XII., pp. 395, 397.

II. EXPERIMENTAL INVESTIGATION.

FORMULATION OF PROBLEM AND DESCRIPTION OF OBSERVERS.

The purpose of the present investigation was to determine the effect of general and local brightness conditions on the color phenomena of peripheral vision. The work may be divided into two main sections: (1) that in which fully saturated Hering colored papers were used as stimuli and (2) that in which the stimuli consisted of 'Urfarben' built up by appropriate mixtures of Hering discs. Both series of stimuli were tested under varying brightness conditions.

Nine observers, all possessing normal vision, served as reagents in the present investigation. Of these Dr. Harvey Carr (C), Dr. Joseph Peterson (P), Dr. Clarence Yoakum (Y), and the writer (F), had completed two or more years of graduate work in psychology and were familiar with the generally accepted statements with reference to color theory, P, Y, and F had at least two months' practice in the experiment before the series reported in this paper were begun. C had previously observed in an investigation involving considerable practice in holding a fixation and in the observation of color phenomena. Mr. Pritcher (Pr), instructor in mathematics at the University of Kansas, Miss Raichelen (R) and Miss Andersen (A), both undergraduates at the University of Chicago, had never done any work in experimental psychology and were not familiar with the general facts of color vision. Mr. Suiter (S) had had the first year's work in experimental psychology and knew enough about color vision to be greatly disturbed by his own results. Miss Hewett (H), a graduate student at Bryn Mawr, served as observer in the results reported from the Bryn Mawr Laboratory. She had completed one year's work in experimental psychology but had never done any special work in vision.

It seemed wise that half of our observers should be entirely unsophisticated with reference to color work, although our

method of procedure seemed to rule out any possibility of error due to suggestion. It was considered absolutely essential to carry out the entire series with well-trained observers because of the greater reliability of their observations. A study of the results shows throughout a striking agreement between those obtained with the two classes of observers.

APPARATUS.

The apparatus consisted of a vertical campimeter exactly like that used in the second part of our previous work.¹ The frame of the campimeter was supported by iron clamps, so constructed that they fitted exactly over iron bars which were screwed to a heavy, solid table.

"The campimeter frame *IJKL* (see Fig. 1) was fastened to heavy iron bars *AB* and *CD*, which were placed, parallel to

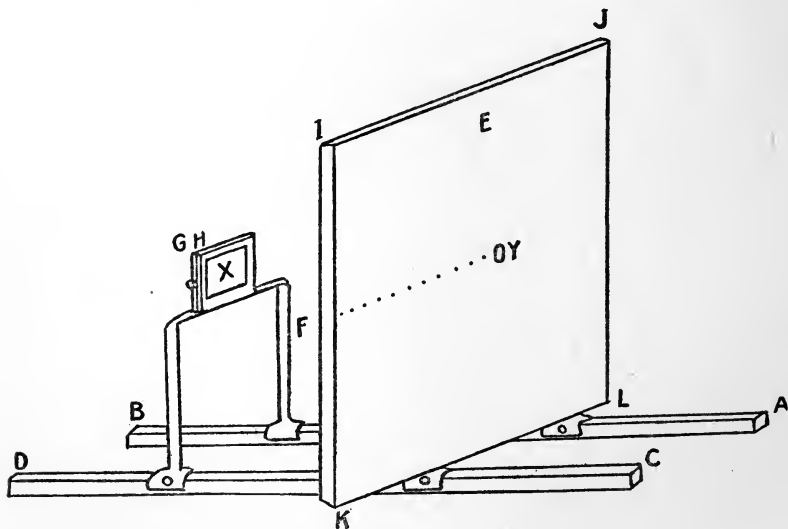


FIG. 1.

each other, on a long table. Behind the frame *IJKL* was an iron support *F*, which was also secured to the iron bars *AB* and *CD*. This support carried a frame, furnished with two

¹ PSYCHOL. REV., Vol. XV., 1908, p. 27.

grooves, *G* and *H*, into which small frames could be slid. A gray slide (*x* in the diagram), like the background in brightness, was fitted into the first of the two frames, *i. e.*, the one toward the campimeter, and the stimulus color into the second. . . . As the limits for the nasal meridian could not be obtained on the flat surface of the campimeter, a second frame was attached to the main campimeter frame, perpendicular to the surface of the campimeter along the edge *IK*."

An electric color mixer was suspended just behind the slide so that various color mixtures could be used as stimuli in place of the color slides. The entire apparatus was constructed with especial reference to solidity, as it was essential that there should be no change in the relations of the parts to each other, and to the source of illumination after the adjustments had once been made.

The backgrounds as well as the screens used to cover the stimulus when it was not exposed, were made of platinum paper or of the dead white and black of the Hering color series (*i. e.*, not numbers 1 and 50 of the Hering gray series). The platinum backgrounds were finally obtained after six months of strenuous effort. It was exceedingly difficult to get large enough sheets of paper printed evenly and toned to exactly the desired shade of gray, but the grays finally secured were far superior to any wash papers on the market. It might be stated here that the Hering grays used in our earlier work were very unsatisfactory because of their evident blue tone. Platinum was chosen in preference to any other photographic paper, because it has the dullest finish of any of those which print a pure gray. The paper was stretched on a canvas-covered frame, which was screwed to the upright campimeter frame.

A circular opening (*Y* in Fig. 1, p. 16) was made in the center of the background by means of a sharp metal punch, which cut a perfectly even line, and yet bent the edges of the paper back so that there was no white ring about the opening. Two sizes of opening were used, the larger being 12 mm. in diameter, the smaller 5 mm. Fixation points measuring degrees, calculated on a basis of an arc of 25 cm. radius, were marked out on the background, starting with the center of the opening *Y* as zero.

A circle was made on the gray slide, which concealed the stimulus color (*X* in Fig. 1); its size being such that when the eye was 25 cm. from the campimeter opening, the circle on the slide seemed to fit just inside the circle of the campimeter opening.² The center of the former circle was determined by placing a ruler perpendicular to the campimeter at the center of the opening. This method of determining the relative positions of the two circles was not so exact as the cathetometer method used in our earlier work, but the dimensions of the room made this latter method impracticable in the present case. No serious error could have been introduced by this change of method since the relative positions of the two circles were kept constant throughout the experiment.

The head was held in position during stimulation by means of a black sealing-wax mouthpiece in which a deep indentation of the observer's teeth had been made. It is evident that, when the apparatus had once been adjusted, the head position was absolutely determined, provided the mouthpiece was secure. To insure the stability of the mouthpiece, it was fastened to a triangular support which was screwed to the table.³

The work was all carried on in a north room which had been especially provided with a 4½ by 5½ ft. plate glass window, so that there was an abundance of north light, and an exceedingly constant illumination. The walls and woodwork of the room were calcimined a medium light gray and the floor was painted the same shade. The work was all done on bright days between the hours of nine A.M. and three P.M. As far as possible the results for a given observer were obtained at the same hour on successive days.

² Data (after Howell) for determining the diameter of the retinal image: Eye to stimulus, 25 cm.; retina to nodal point, 15.5 mm.; surface of cornea to nodal point, 7.3 mm.; diameter of stimuli 12 mm., 5 mm.

$$\begin{aligned} 12:x::250+7.3:15.5. \\ x=.7 \text{ mm.} + \\ 5:x::250+7.3:15.5 \\ x=.3 \text{ mm.} + \end{aligned}$$

³ PSYCHOL. REV., Vol. XV., p. 27-28.

METHOD OF PROCEDURE.

The observer was seated in front of the campimeter so that his right eye was on a level with the opening. The mouth-piece was so adjusted that, when he fixed his teeth in it, his eye was brought into such a position that the circle on the screen appeared to fit exactly into the circle of the campimeter opening. There is only one eye position in which the two circles seem concentric, *i. e.*, the one in which the visual axis coincides with the line through the centers of the campimeter opening and of the circle (x) on the screen.

After the eye position had been determined according to the method described, the observer took a given fixation point, while the experimenter, after a 'ready' signal, removed the screen, thus exposing the color. The observer started a stop-watch as soon as he saw the stimulus (*i. e.*, recognized it either as color or brightness) and stopped it as soon as all color disappeared. The second click of the watch served as a sign to the experimenter to push the screen back over the color. At the less eccentric fixation points the time of stimulation was arbitrarily limited to four or five seconds. The observer held the fixation until all trace of the after-image had disappeared. A two-minute interval was allowed between stimuli. The interval began when the after-image had completely disappeared. All the results were obtained on the nasal half-meridian.

In the first part of the work the stimuli consisted of colored slides, made of Hering colors (new series). The colors used were red, orange, orange-yellow, yellow-green, green, green-blue, violet and carmine. The stimuli were given on the different backgrounds (*i. e.*, platinum white, middle gray and black, Hering black and white and later Hering green, see p. 45) at degrees of eccentricity between 20 and 90 degrees. The stimuli were given in no regular order, so that the observer had no clue whatever as to the color used. To still further guard against the effects of suggestion, black, white and grays matching the various colors in brightness were frequently employed as stimuli. The observer was never given any information concerning the actual color or brightness stimuli used, but was shown samples of all the colors of the Hering series and asked

to identify the color he had seen with a color in the series—or, if an exact identification of the colors was not possible, to locate the color observed between any two colors of the series.⁴ After the effect of the background on these colors had been determined, the same stimuli were observed under different sets of general and local brightness conditions.

In the second part of the work the electric color-mixer was used. The four stable colors, made up of mixtures of the Hering colors used in Section A, were first determined on the middle gray background, and then tested with the black and the white background. They were then equated in brightness and saturation according to the method employed by Hess (see p. 62), and tested for their limits on the different backgrounds.

RESULTS.

SECTION A. OBSERVATIONS MADE WITH HERING COLORED PAPERS AS STIMULI.

1. *Effect on the Stimulus of Changes in Brightness Produced* (a) BY CHANGES IN BRIGHTNESS OF BACKGROUND AFFECTING

(α) *Variations in Color Tone of Stimulus.*—We may mention first the appearance of the stimuli with the white background. Yellow, orange and red, or the colors at the red end of the spectrum, are the only ones which undergo distinct qualitative change, due to the change in the brightness of the background. The effect of the background upon all these colors is, however, so great that observers suppose an entirely different series of stimuli have been used with the dark and with the light backgrounds. The results of the present investigation confirm the statement made in our previous papers, namely that the light background tends to emphasize the red component of red and orange stimuli. In the present investigation red was seen as red much farther out on the periphery than in any of our previous work and yellow was invariably seen as orange or red at the periphery. This is undoubtedly due to the fact that a white instead of a light gray background was used in the experi-

⁴For example a yellowish orange which was yellower than the orange and redder than the orange-yellow of the series, would be described as between these two latter colors.

ment here reported, and that the illumination was greater than any we had previously been able to obtain. The results for the different colors on the white background, are as follows (see Tables XX. and XXI., pp. 50ff.).

Orange and Orange-yellow.—Not only do orange and orange-yellow tend to appear as red in the 'red-green zone' but they are seen as clear, saturated red, practically to the outermost color limits. In fact they were perceived as red by all observers as far out as 85 or 90 degrees with the larger stimulus, and, with the smaller stimulus, to practically the outermost point at which the smaller blue stimulus is recognized under similar brightness conditions. Whenever the color is tabulated as red, the scarlet-red or the brickish red of the Hering series was designated as matching the color seen. No doubt whatever was expressed by the observers concerning the color seen. In most cases they were surprised at being questioned as to the possibility of the color being anything but red. C, P and S expressed considerable concern because red was seen at the extreme periphery, contrary to the statements of the color theories, and were sure their results must be due to some individual variation. The platinum gray, black and white, interposed in the series were never seen as color, with an exception in the case of observer C, who sometimes showed a tendency to see the middle gray and black as reddish, though not as a pure saturated red like that due to stimulation by orange and yellow.

Yellow.—All the observers tended to see the yellow as reddish with the light background, but in some cases this tendency was much more marked than in others. C and P perceived yellow as bright red as far out as it could be seen as color, while the other observers saw it as red, orange or in some cases only as orange-yellow or as golden yellow. It might be stated here that P and C showed the strongest tendency to see red throughout the entire investigation.

Red.—Red, with the white background, is seen only as red, appearing slightly more carmine at the outer limits than at the center.

Violet, carmine and blue all appear either in their true color-

tone or as blue at the periphery, while *green* appears colorless at all the more peripheral points.

With the dark background, of which mention will next be made, orange, orange-yellow and yellow all appear yellow or orange at all peripheral points (see pp. 53-55). Red is seen as yellow by all observers at 90 degrees with both the large and the small stimulus, but is seen as red, or as orange as far out as 85 degrees by observers P and C. These observers, however, described the orange and red, seen for any stimulus with the dark backgrounds, as a peculiar unfused mixture of red and yellow, a sort of red film over a bright luminous yellow, totally different from the carmine red resulting from these same stimuli with the white background. R saw red as red only to 65 degrees, and from 70 to 90 degrees as orange or yellow, with both the large and the small stimulus. Observer Y saw red as orange or orange-yellow from 60 to 90 degrees. Observer F perceived red as red out to 85 degrees when the dark background received full light from the window, but only as orange or yellow from 55 to 90 degrees when the background was shaded by the dark hood (see p. 54). The tables (pp. 53-55) show similar individual differences in the results for orange with this background. Orange-yellow was invariably seen as yellow or as orange-yellow, and yellow as yellow, with a single exception in the case of observer C whose yellows had a faint orange or golden tinge.

Violet, with the dark background, is seen as either blue or violet, and carmine as blue, violet or carmine. Blue appears either as blue or as colorless. The change in the brightness of the background seems to have very little effect on the quality of the colors perceived for these three stimuli, except in the case of carmine, which appears slightly redder with the light than with the dark background (see pp. 56-57).

(β) *Influence on the Saturation of the Color Stimuli and on their Zonal Limits of Changes in Brightness of Background.*—All the stimuli, except those which are seen as red on the light background, appear less saturated on the light than on the dark background, at all peripheral points. Of these latter stimuli, red appears rather less saturated to most observers on

the light than on the dark background, but orange, yellow-orange and yellow appear as very well saturated reds on the light backgrounds. All the observers reported them as being fully as saturated as, and in some cases more saturated than, the orange and yellow seen on the dark background.

In so far as our results justify any conclusions concerning the color limits,⁵ they seem to show that all the colors except the reds are perceived at a greater degree of eccentricity with the dark than with the light backgrounds. Red is seen as red to about the same degree of eccentricity with the dark and with the light backgrounds, but is seen as yellow or orange with the dark background at the same points at which it is seen as colorless with the light background. It should be stated that the red of the series is decidedly darker than either the yellow or orange. Consequently it appeared very dark—to some observers almost blackish—on the white background. For certain observers the limits for red were widest and the color appeared most saturated on the gray background. All the results obtained with violet seem to show that, while the limits for violet as violet are little affected by the change in the brightness of the background, the stimulus is seen as blue with the dark background at the same points at which it appears as colorless with the light background. Observers P and Y failed to see blue at 90 degrees with the light background but got a well-saturated blue at the same point with the dark background. At 92.5 degrees C perceived blue as colorless with the light background and as blue with the dark background (see p. 41).

It is difficult to state the effect of brightness on orange and yellow stimuli, because of the distinct qualitative change which these colors undergo. It is, at least, possible to say that these stimuli appear as red at a greater degree of eccentricity with the light than with the dark background, and that they are seen as red or orange with the light background at the same points at which they are seen as orange or yellow with the dark backgrounds.

The general conclusions from these results would seem to be that all the colors except the reds and perhaps the greens are emphasized when brightened by contrast with a dark back-

⁵ See footnote 25, pp. 65-66.

ground, but that the red appears more saturated where seen as red and is perceived as yellowish or orange, if not as red, at a greater degree of eccentricity with the dark or middle gray than with the light backgrounds.

(b) EFFECT ON THE STIMULI OF DECREASING THE GENERAL ILLUMINATION.

Two methods were employed for decreasing the general illumination: (1) A frame covered with a heavy black cloth (referred to throughout as the '*black hood*') was placed over the head of the observer, and (2) the entire room was darkened by hanging a black curtain over the window so that a greater or less amount of light could be admitted. The black background was used in both cases.

When the field was darkened by means of the black hood, the brightness contrast between the black background and the stimulus was heightened as the color still received full illumination from the window. Consequently the stimulus appeared very bright. Thus a double change was produced by this method, namely an increased brightness of stimulus by contrast with a dark background and a condition of partial dark adaptation. When the entire room was darkened by means of the dark curtain, the brightness of the stimulus as well as that of the general field was decreased.

(α) *Darkening the Field by Means of the Black Hood.*
—The results obtained by this method are given in Tables XXII., XXIV., A and B, p. 54, and D, p. 56. In general they are as follows:

Darkening the Field by Placing the Black Hood over the Head of the Observer, greatly heightens the effects obtained with the dark background. The reds, oranges and yellows are seen as yellow or orange much more frequently, and much nearer the centre of vision than with the dark ground without the hood (see Tables XXIV., A and B).

In the case of observers R and F the fields for orange and orange-yellow are decidedly narrower when the black hood is used, than when the colors are seen against the black background in full illumination (see Table XXIV.), as under

the former conditions the colors appear yellower than the stimulus at the same points at which they appear as orange or orange-yellow under the latter conditions.

The field for red is decreased in extent, for observers R and F (Tables XXIV., A and B) and it is seen as yellow at points well within the limits for the color with the dark background and full illumination. The effect of darkening the field by this method is much greater with red than with orange. In some of our more recent work, Carr and Ferree reported a very peculiar appearance with the dark background and hood—namely the simultaneous perception of red and yellow. A fire red film seemed to be spread over a clear yellow. Peterson and Carr described a similar phenomenon with the dark background alone (see p. 53).

(β) *Darkening the Entire Room by Means of the Black Curtain.*—The results obtained by this method are given in the order in which they were recorded with the exception of those for observer R, which are tabulated without regard to

TABLE I.

OBSERVER R.

Black Background + Black Curtain.

Stimulus.	Color Seen.	60°	65°	70°	75°	80°	85°	90°
Yellow	Yellow			I		2 ¹	I	I
Orange	Orange		I _b ^r	(I _b)		I ^r , (I _b), I	II	
"	Or.-Yellow		I _b	I _b			2 _b	I _b
"	Yellow			I _b	I _b	3 _b	2 _b ²	2, I _b
Red	Red					I	I	
"	Red-Or.			I _b			3 _b ¹	
"	Orange						I	I
"	Or.-Yellow						I	2
"	Yellow							I _b

Table I. is self-explanatory except for the following points: The number of times the stimulus was seen at a given fixation point is shown by an Arabic numeral in one of the vertical columns; the Roman numerals indicate the number of times the stimulus was seen as brightness simply. Numerals in parentheses represent judgments about which the observer was doubtful. Letters used as superscripts indicate the color tone of the color seen, *e. g.*, 'r' means a reddish tone of any color seen. Letters used as subscripts indicate the color of the after-image.

The stimulus was a circle of 12 mm. diameter in every case where not stated to the contrary. In certain tests a stimulus of 5 mm. diameter was employed.

order. No attempt was made to determine anything but the general quality of the colors perceived under these conditions. The amount of illumination was varied from that obtained with one thickness of curtain to that obtained with three thicknesses. The results are given in the following tables. All results were obtained at Chicago unless otherwise designated.

TABLE II.

OBSERVER S.

White Background.

Fix. Pt.	Stimulus.	Color Seen.	After-image.
90°	Or. yellow	Dk. gray	?
90°	Orange	Black	Blue
90°	Blue	Blue (?)	White (?)
85°	Blue	Black	Yellow
85°	Or. yellow	Not clear	Blue
85°	Orange	Dk. red	Blue
75°	Gray	Gray	White
90°	Blue	Black	Yellow (?)
80°	Or. yellow	Medium or.	Blue
85°	Or. yellow	Medium or.	Blue
80°	Orange	Red	Blue
75°	Or. yellow	Medium or.	Blue
85°	Orange	Orange	?
75°	Orange	Or. red	Blue
85°	Or. yellow	Dk. red	Blue
75°	Blue	Blue	Yellow
80°	Red	Black	Blue

Dark Background—Black Curtain.

Fix. Pt.	Stimulus.	Color Seen.	After-image.
90°	Or. yellow	White	Black
90°	Red	Yellow	Black
85°	Red	Yellow	Black
85°	Blue	Blue	Black
85°	Or. yellow	Yellow	Black
85°	Orange	Yellow	Black
80°	Gray	White	Black
90°	Blue	Blue (?)	Black
90°	Gray	White	Black
85°	Yellow	Yellow	Black
85°	Red	Yellow	Black
85°	White	White	Black
85°	Orange	Yellow	Black
85°	Red	Yellow	Black
85°	Or. yellow	Yellow	Black
80°	Yellow	Yellow	Black
80°	Red	Yellow	Black

The colors appeared well saturated, and were identified with the colors of the Hering series.

TABLE III.

OBSERVER H.

(Results obtained in Bryn Mawr Laboratory.)

Room Darkened by Single Curtain.

Fix. Pt.	Stimulus.	Color Seen.	After-image.
90°	Orange	Yellow	None
90°	Gray	Light	None
90°	Dk. gray	Nothing	None
85°	Orange	Yellow	None
80°	Orange	Yellow	None
80°	Yellow	Yellow	None
75°	Orange	Yel. orange	Blue
75°	Green	Light	None
70°	Orange	Yel. orange	Pale blue
70°	Gray	Light	Dark
75°	Red	Or. yellow	None
85°	Green	Light	Dark

Room Darkened by Double Curtain.

Fix. Pt.	Stimulus.	Color Seen.	After-image.
70°	Orange	Red orange	Blue
85°	Orange	Dark	None
80°	Orange	Red orange	None
80°	Gray	Nothing	None
65°	Green	Nothing	None
65°	Dk. gray	Nothing	None
65°	Orange	Red orange	Pale blue
70°	Orange	Red	Blue
75°	Yellow	Or. yellow	Pale blue
80°	Gray	Gray	None
80°	Orange	Or. red	None
80°	Yellow	Yellow	Blue
85°	Yellow	Or. yellow	Blue
75°	Orange	Red orange	Blue

TABLE IV.

OBSERVER F.

Dark Background—Single Black Curtain.

Fix. Pt.	Stimulus.	Color Seen.	After-image.
85°	Red	Faint yellow	Blue
85°	Light blue	White	None
85°	Dk. blue	Sug. of blue	None
85°	Orange	Bright yellow	Faint blue
85°	Yellow	White	?
80°	Orange	Yellow	Blue
80°	Blue	Blue	None
85°	Orange	Yellow	Blue

TABLE V.

OBSERVER F.

(Results obtained in the Bryn Mawr Laboratory.)

Black Background—Single Curtain.

Fix. Pt.	Stimulus.	Color Seen.	After-image.
85°	Orange	Or. yellow	None
85°	Red	Gray	None
85°	Red	Light	Dark
80°	Orange	Lemon yellow	None
85°	Yellow	White	None
85°	Orange	Yel. orange	None
85°	Blue	Blue	None
85°	Red	?	None
80°	Yellow	White	Blue
80°	Blue	Blue	None
80°			

Black Background—Double Curtain.

Fix. Pt.	Stimulus.	Color Seen.	After-image.
70°	Orange	Yel. orange	None
70°	Blue	Blue	None
70°	Gray	White	None
70°	Yellow	Yellow	None
80°	Orange	Unsat. yellow	None
80°	Blue	Pale blue	None
80°	Gray	No color	None
80°	Yellow	Light yellow	None
80°	Yellow	White	None
80°	Gray	Light	Dark
70°	Blue	Blue	None
70°	Yellow	Yellow	None
85°	Blue	Unsat. blue	None
80°	Orange	Yellow	None

The results for S, H, R and F show that when the general illumination is decreased by a single thickness of curtain the red, orange and yellow appear clearly yellow at the periphery. In the case of H the colors mentioned grew distinctly reddish as soon as the room was further darkened by a double curtain. CF saw the colors as distinctly reddish with both the single and the double curtain.⁶ P saw them as reddish with the single curtain.⁶ (No results were obtained for P with the double curtain.) F saw the colors as yellowish with both the single and the double curtain.

⁶Results not given. Observer C. F. is Dr. C. E. Ferree of Bryn Mawr College.

These results are less satisfactory than those obtained by other methods as the colors appeared less saturated than in any of the previous work. It is probable that the seeming irregularities are to be explained on the ground of individual variations. When the light is cut out by a single thickness of curtain both background and color are darkened, but the brightness contrast between the color and the background is still sufficiently strong to make the color appear relatively light and consequently yellowish to those observers who have the less strongly marked tendency to see red at the periphery. The observers who have a greater sensitivity to red see the stimulus as red when it has been slightly darkened (see section on individual variation, pp. 59-60).

In every case but that of F the colors were seen as reddish when the room was darkened by the double curtain, and F shows a less strongly marked tendency to see red at the periphery than any of the other observers except R.

The results seem to show that darkening the colors sufficiently will cause the colors to appear reddish even when the entire room is darkened. The reds and oranges are, however, less clear and saturated than when the stimulus is darkened by brightness contrast with the white ground.

(c) EFFECT OF CHANGING THE BRIGHTNESS OF THE STIMULUS
WITHOUT CHANGING THE GENERAL ILLUMINATION
OR THE BRIGHTNESS OF THE BACKGROUND.

Three methods were employed for varying the brightness of the stimulus without introducing any change in brightness of the background or in the general illumination. (1) The color was shaded by a dark screen placed above it, so that while the color was still easily distinguishable in central vision, it was decidedly darkened. (2) The brightness of the stimulus was increased or decreased by superimposing upon it the after-image of the black or the white screen used to cover the color before and after stimulations. (3) Black or white discs were combined with the stimulus on the electric color-mixer.

(a) *Darkening the Stimulus by Shading it with a Dark Screen.*—In the use of the first method the quality of the color

perceived in central vision was not affected, although the stimulus was noticeably darkened. The stimuli were given, as in our previous work, in irregular order, various colors as well as black, white and gray, being interposed between the red, orange and yellow stimuli. The following results were obtained with observers H and F.

TABLE VI.

OBSERVER H.⁷

(Results obtained at Bryn Mawr.)

Black Background. Stimulus Darkened by Black Cover. White Screen.

Fix. Pt.	Stimulus.	Color Seen.	After-image.
80°	Yellow	White	Light gray
75°	Yellow	Orange, like Or. disc.	Blue
70°	Yellow	No color	No after-image
70°	Yellow	Yellow orange	Blue
65°	Yellow	Yellow	Blue
75°	Orange	No color	Blue
75°	Orange	No color	Blue very faint
75°	Orange	Unsat. orange	Pale blue
70°	Orange	Orange red	Blue
65°	Orange	Sat. red like red disc	White
65°	Orange	Dark sat. maroon	Pale blue
65°	Orange	Sat. red like dark red of Hering series.	Blue
60°	Orange	Red like dk. red of series	Pale blue

TABLE VII.

OBSERVER F.⁸*Black Background. Stimulus Darkened by a Black Cover. White Screen.*

(Results obtained at Bryn Mawr.)

Fix. Pt.	Stimulus.	Color Seen.	After-image.
85°	Yellow	None—just white	None
80°	Yellow	Dull yellow; grew red rapidly	None
75°	Yellow	Dull unsat. orange	Blue
65°	Yellow	Orange yellow	Deep blue
85°	Orange	Possibly red	None
80°	Orange	Dark red	None
75°	Orange	Dark brick red	Blue
70°	Orange	Dark sat. red	Blue
65°	Orange	Orange	None

⁷ No results for red are given, as the red disc appeared simply as black at all the above fixation points, when it was darkened by this method.

⁸ Red was seen simply as black in to 65 degrees. It was reported as a doubtful red once at 65°.

The results for both H and F show that the effect of darkening the yellow and orange by this method is to decrease their saturation. Certain of these stimuli appear reddish at the periphery. Other later results agree with those for H and F.

(β) *Projecting the After-image of a White or a Black Screen on the Stimulus.*—The platinum black and gray, and the Hering black and white backgrounds were used in this part of the experiment. The method of procedure was exactly the same as that already described except that the screen (X in the figure p. 16), used to cover the color before and after stimulation was varied, *i. e.*, both the black and the white screens were

TABLE VIII, A.

OBSERVER H.⁹

Color seen is designated by letters, *e. g.*, R = red, O = orange, etc. The subscript letters indicate color of after-images.

Stimulus.	Backgr'd.	Screen.	55°	60°	65°	70°	75°	80°	85°	90°
Orange	White	White	RO _b O _b	R _b OR _b	2OR _b	2R _b	I _b	O _b		
"	"	Black	2O	RO O	RO OR	RO OY	I	I		
"	Black	White	RO	O	O	RO _b RO OR _b 2O	RO _b O	O	RO _b OY _b I	
"	"	Black	OY	OY	2OY	2O OY	O OY	O Y	OY Y III	Y
Yellow	White	White		RO _b	OR _b O _b OY _b	O _b		R _b R OR _b RO _b	III	
"	"	Black		Y-O _b	2Y Y _b	Y		2Y Y _b	2Y I _b	
"	Black	White		Y _b	Y _b	O _b	O _b OY			OY
"	"	Black		Y _b	Y _b	Y _b	2Y			Y

⁹ These results were obtained at Bryn Mawr College. The backgrounds were Hering black and white instead of platinum. The fixation was held about three seconds before the stimulus was exposed.

used in each series of tests with a given background. The fixation was held for a period varying from three to ten seconds. As soon as the screen was removed, the brightness after-image, namely, black for the white screen, white for the black screen, was mixed with the stimulus color, thus causing the color to appear darker or lighter. A possible objection to this method is that an element of fatigue may be introduced by the somewhat continued maintenance of the fixation. The observers, however, reported a complete lack of any subjective evidence of such fatigue.

The following tables give the results for observers, H, F and W. The tables for H and F are followed by a summary showing the number of times a stimulus was seen in a given color tone under given brightness conditions (p. 33).

B.

OBSERVER F.

Stimulus.	Backgr'd.	Slide.	50°	55°	60°	65°	70°	75°	80°	85°	90°
Orange	White	White	R _b	RO R _b	RO _b R	OR _b	?O _b	R _b	I	I	I
"	"	Black	O- RO _b	O- RO _b R _b	O _b	R RO O _b	OR I	II		I	I
"	Black	White				YO _b	YOY	O	O	Y-O	
"	"	Black				YO _b	OY	OY _b	Y _b	Y	
Yellow	White	White	Y _b	2 Y _b	O _b	2Y _b	O _b O I	II	III	II	I
"	"	Black	Y _b	2 Y _b	Y _b	Y _b	3Y _b		IO Y _b	II _b Y	
"	Black	White				Y _b	Y _b	Y	Y _b	I _b	
"	"	Black				Y _b	Y _b	Y _b	Y _b	Y	

The results for H show a constant tendency on the part of orange and yellow to appear redder when the dark after-image of the white screen is mixed with the stimulus color than when the white after-image of the black screen is mixed with it. The most striking results are those for yellow with the white background. In this case yellow appears as either orange or red

TABLE IX.

OBSERVER H.

Number of Times the Stimulus was Seen as R (Red), etc.

		<i>Stimulus, Yellow.</i>							Not Seen.	Total.
Background.	Slide.	R.	OR.	RO.	O.	YO.	OY.	Y.		
White	White	2	2	2	2	—	1	—	3	12
White	Black	—	—	—	—	—	—	10	1	11
Black	White	—	—	—	3	—	2	2	—	7
Black	Black	—	—	—	—	—	—	7	—	7
		<i>Stimulus, Orange.</i>							Not Seen.	Total.
Background.	Slide.	R.	OR.	RO.	O.	YO.	OY.	Y.		
White	White	3	3	1	2	—	—	—	1	10
White	Black	—	1	3	3	—	1	—	2	10
Black	White	—	1	5	6	—	1	—	1	14
Black	Black	—	—	—	4	—	7	3	3	17

TABLE X.

OBSERVER F.

Number of Times the Stimulus was Seen as R (Red), etc.

		<i>Stimulus, Yellow.</i>							Not Seen.	Total.
Background.	Slide.	R.	OR.	RO.	O.	YO.	OY.	Y.		
White	White	—	—	—	3	—	—	5	9	17
White	White	—	—	—	1	—	—	10	3	14
Black	White	—	—	—	—	—	—	4	1	5
Black	Black	—	—	—	—	—	—	4	1	5
		<i>Stimulus, Orange.</i>							Not Seen.	Total.
Background.	Slide.	R.	OR.	RO.	O.	YO.	OY.	Y.		
White	White	5	—	2	2	—	—	—	4	13
White	Black	2	1	3	2	—	—	—	5	13
Black	White	—	—	—	2	2	1	—	—	5
Black	Black	—	—	—	—	1	2	2	—	5

TABLE XI.

OBSERVER W.

Gray Background.

Fix. Pt.	Stimulus.	Black Screen. Color Seen.	White Screen. Color Seen.
80°	Unsat. orange	Unsat. or. yellow	White
80°	Unsat. orange	Unsat. or. yellow	White
75°	Unsat. orange	Bright orange	Orange to bright red
75°	Unsat. orange	Orange
80°	Unsat. orange	Yellow to orange	Dark saturated red.

Black Background.

Fix. Pt.	Stimulus.	Black Screen. Color Seen.	White Screen. Color Seen.
85°	Orange	Orange	Indistinct, some reddish
80°	Orange	Orange	Orange
85°	Red	Orange	Black
85°	Red	Orange	Orange
80°	Red	Orange	Red orange

The fixation was held for 10 seconds before the screen was removed in all cases except the last with orange. In this instance the screen was removed after a 5 seconds fixation.

without exception when the color is darkened by the black after-image of the white screen, and invariably as yellow when the stimulus is brightened by the white after-image of the black screen. At 60° with the black screen the color appeared once as a yellow when first exposed, but changed to orange (Y-O). This may be due to the fact that as the effect of the white after-image was lost the color grew darker.

The results for F are much less conclusive than those for H. Orange appears reddish with a white background and yellowish with a dark background, regardless of the brightness of the screen. The results show, however, a slightly greater tendency for the stimuli to appear reddish with the light than with the dark screen.

The results for W are somewhat incomplete. They were obtained in the course of one hour's experimentation, and are given just as recorded, except that all results for stimuli other than orange are omitted. Violet, blue and gray were included in the series as given. The results show a slight but decided effect of the brightness changes produced. Orange tends to appear redder with either background whenever the black after-image of the white screen is mixed with the red or orange stimulus.

The fact that the results for W and H show a more positive effect of the change produced on the color by the brightness after-image than is shown by the results for F simply affords an illustration of individual variation. Such individual variations are shown throughout the work (cf. pp. 59-60) and are sufficient to make a factor which is influential in one case, ineffective in another. The results for F and R seem to show that their reactions are always less easily modified by brightness changes than those of the other observers. The modifications are, however, very marked and exactly similar to those obtained by other observers when the brightness changes are sufficiently great.

(γ) *Mixing Black or White with the Stimulus on the Electric Color Mixer.*—The stimulus color was mixed with given amounts of white or black, by means of the electric color mixer. The difficulty in this case is that the saturation of the color is noticeably decreased.

The results are given of work with observers W, R and F. It is true that the results are few in number, but the facts that the entire series with any given background was obtained on the same day under identical light conditions, and that the dark and light mixtures were alternated, without any knowledge on the part of the observer concerning the stimuli employed, make these results significant.

TABLE XII.

OBSERVER W.

Black Background.

Fix. Pt.	STIMULUS.		
	260° Red + 100° White.	Fully Saturated Red.	260° Red + 100° Black.
90°	Yellow	Yel. orange	Yel. orange
85°	Yellow	Orange	Orange
80°	Orange	Orange	Orange

Gray Background.

Fix. Pt.	STIMULUS.	
	180° Red + 180° White.	180° Red + 180° Black.
80°	Orange	Black
75°	Scarlet red	Dark red
70°	Yellow orange	Brick red. (102° black instead of 180°.)

Black Background.

Fix. Pt.	STIMULUS.		
	260° Orange + 100° White.	360° Orange.	260° Orange + 100° Black.
90°	Yellow	Or. yellow	Or. yellow
85°	Orange yellow	Or. yellow	Or. yellow

Gray Background.

Fix. Pt.	STIMULUS.	
	280° Orange + 80° White.	280° Orange + 80° Black.
75°	Scarlet red	Brick red

Black Background.

Fix. Pt.	STIMULUS.	
	180° Yellow + 180° White.	180° Yellow + 180° Black.
75°	Colorless	Yellow
80°		Colorless

The description of the stimulus is given at the head of each column (*i. e.*, 260° red + 100° white, etc.). The color seen is given opposite the fixation point (90, etc.) and directly under the description of the stimulus.

TABLE XIII.

OBSERVER F.

Gray Background.

Red 246°	} Matched background in brightness.	At 80° only blue after-image, no color for stimulus.
Black 58°		
White 56°		
		At 70° good medium red.
Black 114°	} Darker than background.	At 70° good medium red.
Red 246°		
White 114°	} Lighter than background	At 70° orange.
Red 246°		

TABLE XIV.

80°	Orange yellow 252°	} Good clear or. yellow. B. violet after-image.
	Black 58°	
	White 50°	
80°	Orange yellow 252°	} Good clear or. yellow. B. violet after-image.
	Black 108°	
80°	Orange yellow 252°	} Or. yellow like above only lighter. Violet after-image.
	White 108°	

The results for observers W and F indicate a tendency on the part of the darker color to appear more reddish than the lighter color. R's results are not given, as the color fields were so diminished by the decrease in saturation involved in this method, that the color was not seen far enough out at the periphery to justify any conclusions.

In the case of W, whose results were obtained with both the black and the gray backgrounds, the change in the color-tone of the stimulus is decided only with the black background. With this background, light red (*i. e.*, red + 180° white) is seen as yellow at 90° and at 85° while at the same points the pure red and the red + 180° of black are seen as yellowish orange and orange. Orange + 100° of white is seen as yellow at 90°, while orange and orange + 100° of black are seen as orange-yellow at the same point.

F's results are given for red and orange yellow with the gray background. The amount of the stimulus color was kept the same in all cases, *i. e.*, 246°, but it was first mixed with 114° of black and white in such proportions that it matched

the background in brightness. The whole sector of 114° was next made black and then white. The results for red show that the two darker colors were seen as red, while the lighter color was seen as orange at the same point. The few results with orange-yellow are entirely negative.

The only conclusion which we can draw from these results is that the changes in the stimulus color produced by this method are primarily a decrease in the saturation and in certain cases a shift of the color toward red.¹⁰

2. *Effect of Brightness on the After-image.*

Explanatory Statement.—*Perceived* will be used to describe the stimulus when it appeared to the observer as colored, *unperceived* when it appeared colorless. *Background* will be used to designate the campimeter screen (*E* in figure, p. 16), and *projection-ground* to indicate the screen which covers the color before and after stimulation (*x* in figure, p. 16). No distinction was made between the two screens in our earlier papers as they were both of the same brightness and so formed one continuous background.

The conclusions concerning after-images are based (1) on the results given in the tables at the end of Section A (pp. 50–58), which give the characteristic after-images observed on screens (*i. e.*, projection grounds) matching the various backgrounds in brightness; (2) on the results reported in the section on the 'Effect of Decreasing the General Illumination' (pp. 24ff.); (3) on the results reported on pages 40–41 and 31–33, which show the effect of changing the brightness of the projection-ground without changing the brightness of the background.

Results.—Characteristic after-images followed perceived color stimuli almost without exception, when the observations were made under any of the following conditions: (1) With the white, gray or black backgrounds in full illumination.

¹⁰ Other results more recent than those given above, agree with those reported in the entire section on the effect of darkening the stimulus by methods other than brightness contrast (pp. 24ff.). In all cases, however, the color appeared poorly saturated and the change in color tone was less marked than that produced by brightness contrast.

(This statement does not apply to the results obtained with the black background in the Bryn Mawr Laboratory or to those obtained with the carmine stimulus on the black background in the Chicago Laboratory). (2) With the dark background darkened by the black hood, when the stimulus color was covered after the stimulation by simply pushing the slide (α in figure, p. 16) back over it. In this case the general field about the observer was very dark, but the projection-ground received full illumination and was further brightened by contrast with the dark background. It may be mentioned here that after-images were entirely absent when a black cardboard was slid directly over the opening in the background, so that the entire field was dark immediately after the stimulation.

After-images were relatively infrequent, even for clearly perceived color stimuli, when the illumination of the entire room was decreased, either by darkening the room by means of the black curtain or by general weather conditions. In the work with H and F at Bryn Mawr a smaller percentage of after-images was obtained with the dark than with the light background (cf. pp. 31-32).

In all of the above cases the after-image exhibited a most decided decrease in saturation as the illumination was decreased. That is, the most saturated after-images were obtained with the white background, in full illumination, between the hours of nine in the morning and three in the afternoon. The after-images, under these conditions were reported by most observers to be either equal to the stimulus in saturation or else as decidedly more saturated than the stimulus. The after-images with the dark background were reported as decidedly less saturated than the stimuli and as very much less saturated than those obtained with the white background. As the general illumination is decreased, the after-images not only decrease in number but become very poorly saturated when perceived. The main exception to the above statement occurs in the case of the after-image for blue and green-blue on the dark background and in a few instances for these stimuli in decreased illumination. The after-image, under these conditions, was a deep, saturated red for most of our observers and a fairly saturated orange

for the others. The observers who experienced the red after-image, described it as the most saturated after-image obtained throughout the series. Even in the case of this red after-image, however, there seemed to be a special brightness of background and illumination which gave the most saturated after-image, and any decrease in the illumination below this point was followed by a corresponding decrease in the saturation of the after-image.

In addition to the work reported in the tables in this paper a considerable amount of work was done on rather cloudy days or after three or four o'clock in the afternoon. The after-images were relatively infrequent or poorly saturated in all of these cases. Even working in a room with black walls seemed to have this same effect on the after-image.

With the dark and gray projection grounds the after-images for orange and red tended to appear greenish in the paracentral and less extreme peripheral regions, though they still appeared blue in the more peripheral regions. The after-image for violet appeared as green in the inner peripheral zone and as green or yellow or, in a few cases, as a very unsaturated orange in the outer peripheral regions. The most striking change occurred in the appearance of the after-images for blue and green-blue. With the dark background these were an orange or a very well-saturated red at the extreme periphery. (The red was invariably described as more saturated than the orange.)

In order to determine whether the color changes were due to the darkening of the after-image itself, or to the variations in the brightness of the background, or to changes in the general illumination, the after-images were projected, first on a light and then on a dark projection ground, throughout an entire series in which the background was not changed. Results were obtained according to this method with both the light and the dark backgrounds. Thus we have an entire series of results, showing the appearance of both light and dark after-images with the white background, and a similar series with the dark background. It should be stated here that this experiment is suggested by Misses Thompson and Gordon, though only carried out by them in central vision.

The results obtained for red, orange and yellow stimuli, according to this method are given on pages 31-33. As the after-image was invariably blue with both backgrounds the results are not repeated here. The results obtained with blue stimuli are given in the following tables.

The Arabic numerals give the number of times the stimulus was seen as blue, the Roman numerals the number of times the stimulus was seen simply as brightness. The after-images are designated by the suffixes y (yellow), r (red), etc.

TABLE XV, A.

OBSERVER W.

Stimulus.	Background.	Projection.	75°	80°	85°
Blue	Black	Black	I _o	2 _o	I _o
	"	White	I _y	I, I _y	I _y

B.

OBSERVER R.

Stimulus.	Background.	Projection.	60°	65°	70°	75°	80°	85°	90°
Blue	Black	[Black	I _{g-r}	I _y	I _{go}	2 _{yg}	I, I _o	4 I _o	2, I, I _{o-g}
"	Black + Hood	"			I _{gy}			2 _o , I _{yo}	I _o
"	Black	White			I _{oy}		3	I _{oy}	I, 2 _y
"	White	White	3 _y , I _o , 2	3 _y , I _g , I	I _y	I _y	I _y , 2	I _y , 2	I, I
"	Black + Curtain	Black					I _{ro} I	2, I ₇ , I _{g-o}	I

C.

OBSERVER H.¹¹

Stimulus.	Background.	Projection.	55°	60°	65°	70°	75°	80°	85°
Blue	White	White	3 _y	3 _y	4 _y	2 _y , II	II _y	IV	III
"	"	Black	2 _o , I _{ro}	2, I _r	2 _{or} , I _y , 3	2, I _r , II	3, I	IV	III
"	Black	White		I _r	2 _y	I _r		I _y	I
"	"	Black		I _{ro}	I _r , I _o	I _o		I	I

¹¹ Results obtained at Bryn Mawr.

D.

OBSERVER C.

Stimulus.	Back-ground.	Projection Ground.	60°	65°	70°	75°	80°	85°	90°	92.5°
Blue	Black	Black			I_r	I_r	2_r	I_r	$I_{or}, 2_r$	$I_r?$
"	Black + Curtain	"		I_r			I_r	2_r	I_r	
"	Black	Gray					I_{yo}	I_{ro-y}		
"	"	White					I_{ro}	I_{ro}	I_{r-y}	
"	White	"					2_y	$2_o I_y$	$2_{oy} II$	III
"	Black + Hood	Black					I	$I_r I_{y-r}$	$2_r I$	

Red after-image described as exactly like red paper, more intense than stimulus. O greatly disturbed by red as a.-i. for blue. Especially brilliant with black curtain.

E.

OBSERVER P.

Stimulus.	Back-ground.	Projection Ground.	60°	65°	70°	75°	80°	85°	90°	92.5°
Blue	Black	Black ¹²					2_r	2_r	I_r	
"	Black + Hood	"					3_r	I_r	I_r	
"	Black	Gray								
"	"	White ¹³					2_y	2_y	(I)	
"	White	"		I_y	I_y	I_y	I_y	II	$II I_y$	

F.

OBSERVER F.

Stimulus.	Back-ground.	Projection Ground.	60°	65°	70°	75°	80°	85°	90°	92.5
Blue	Black	Black ¹⁴				I_r	I	$I_r, 3$	I	
"	Black + Hood	"			2_y	I_o	I_{or}, I_{ro}, I_o	$3_o, I_o$	I_o	
"	Black	Gray				I_{yo}	I_{yo}, I_o	I_r	I	
"	"	White			I_{oy}	I_{oy}	I_y	2_{oy}	$I_{(y)}, I_{oy}$	$I_{(y)}$
"	White	"	$I_{yg} I_y I_{y-o}$ ¹⁵					$3_y, I_{yg}$	I, I_y, I_{oy}	

¹² Red after-image = 'Brilliant decided red.'¹³ Yellow after-image = 'Very poorly saturated.'¹⁴ Red after-image more saturated than yellow.¹⁵ Yellow, grew orange.

The results for W, R, C and P show that the after-image is almost invariably yellow when projected on the white screen and orange or pure red when projected on the dark screen. No results were obtained for these observers with the white background and black projection-ground. In the experiments with H and F the after-image was yellow when projected on the white screen and orange or red when projected on the dark screen, with the white as well as with the dark backgrounds.

Although the after-image shifts toward red, in all cases with the dark projection-ground, the effect of the local brightness change thus produced was greater for some observers than for others. Under the conditions just described (*i. e.*, with the dark projection-ground) the after-image for P and C was a pure red which was so well saturated that both observers asked if a red stimulus had not been given after the blue. The observers were the more surprised as they expected a yellow after-image. The red was identified with the red of the Hering series. H showed a decided decrease in the percentage of after-images with the dark background and the dark projection-ground, but when seen, they were red or orange with two exceptions. (The after-image was yellow twice with the dark projection-ground and white background.) F's after-images were always red or orange, the red being much more saturated than any of the yellows experienced on the white projection-ground. The after-images for W were orange in the few cases in which the after-image for blue was projected on the dark screen. R's results show less alteration with change of screen than do those of any other observer. There is, however, a shift toward orange with the dark projection-ground. R was not always sure of the color of the after-image, being at times doubtful whether it was tinged with green or orange.

The results just reported seem to show that the color-tone of the after-image is more dependent upon the brightness of the projection-ground (local brightness conditions) than upon the contrasting brightness of the surrounding background.

That the phenomena reported here are genuine after-images is shown by the following facts.

1. The colors are reported as perfectly clear and distinct by

all observers, with the few exceptions of the unsaturated images on the dark background. Observer R had nearly completed the series before she discovered that a second color was not actually used as a stimulus. She had been especially cautioned not to move her eye until every trace of color had disappeared, and consequently never saw the gray screen until the after-image had completely disappeared. One day, however, just at the close of the year, she suddenly exclaimed: "Why, isn't that funny—I looked back accidentally while the yellow was still there and there wasn't any color!" She was greatly surprised. She always spoke of the stimulus color as the 'color going in' and the after-image as the 'color going out.' When the colored after-image followed the unperceived color stimulus, she always reported 'gray,' 'black' or 'white' 'going in' and 'blue,' 'yellow,' etc., 'going out.' She was astonished to discover at the close of the experiment that a second stimulus had not been given throughout the work. In the same way observers C and Y asked if a second color had not been given when the red after-image followed the blue stimulus on the dark background, first, because the after-image was so well saturated, and second, because it was not the color complementary to the blue. As C expressed it: "I haven't any business to see that red way out there, especially as an after-image for that blue, but I never saw a better red in my life."

2. Although the stimuli were given in an irregular order and the observer was kept in ignorance concerning the nature of the stimulus, the after-image was always the characteristic one for the stimulus under the given brightness conditions, even when the stimulus color was not distinguished.

3. Black, white and gray were frequently given as stimuli, but were never followed by colored after-images, with a single exception in the case of observer C, who reported an unsaturated red after-image in a few instances when a dark screen followed a white stimulus. It may be well to state here that our method of procedure differed essentially from that of either Hellpach or Baird, in that both of these investigators gave the same stimulus in successive tests throughout a series, while we gave the stimuli in irregular order, seldom using the same stimulus twice in suc-

cession, and frequently introduced brightness stimuli, without the knowledge of the observer, into the series.

Our general conclusions based on all our work with peripheral after-images, are as follows:

(a) The relative frequency with which characteristic colored after-images follow color stimuli and the saturation of these after-images are directly dependent on brightness conditions. As the general illumination or the brightness of the projection-screen is decreased, the after-images become at first less saturated and finally less frequent. This decrease in the saturation of the after-image is much more marked than the corresponding decrease in the saturation of the stimulus color. That these results are due to the brightness conditions under which the after-image is observed is shown by the following facts: (1) That a decrease in the illumination, which has little effect on the appearance of the stimulus color, has a very decided effect on the saturation of the after-image; (2) that the after-image, with the exception of red, invariably appears less saturated on the dark than on the light background, and yet this is just the condition under which all the stimuli except red and orange appear most saturated; (3) that, in the observations made under the black hood, fairly well-saturated after-images followed the stimuli when the after-image was projected through the campimeter opening on a screen which received full illumination, but that after-images were entirely absent when the observation of the stimuli was made exactly as in the previous case save that the light was cut off from the observer by placing a black card directly over the circular opening in the campimeter, so that any possible after-images would necessarily be observed on an entirely darkened field; (4) that the after-images appear most saturated on a particular projection-ground, even when no other change is made in the brightness conditions.

(b) The color-tone of the after-image for certain colors is directly dependent on the brightness of the projection-ground. The after-image for blue and blue-green is yellow on the white projection-ground and red or orange on the dark projection-ground; for violet, greenish on the dark projection-ground and

yellow on the light projection-ground; for red, orange and yellow, blue on both the light and dark backgrounds except in the paracentral zone.

(c) Characteristic after-images followed unperceived color stimuli in some cases with the light projection-ground and with full illumination of a bright day. These after-images were never obtained on cloudy days or later than three or four o'clock on bright days. For illustrations of these after-images see Tables XX. and XXI.

3. *Comparison of the Effect of Brightness Contrast with that of Color Contrast.*

Although the background appeared entirely colorless there seemed to be the possibility that some subliminal color might be present and be effective in producing the results described. To determine whether the results could be due to color contrast, a series of experiments was made with the fully saturated Hering green as a background. Green was chosen for the background because, in central vision, green is the contrast color for red and so would tend to enhance the red in any stimulus, and produce results similar to those we obtained in peripheral vision when the color was darkened by contrast with the white background. It is obvious that, if our results in peripheral vision are due to the presence of subliminal green in the background, the use of a fully saturated green background might be expected to increase this effect.

The results obtained are given in Tables XVIII and XIX, pp. 48 and 49. The summaries given on pages 46 and 47 show the number of times the orange and yellow stimuli and the after-image for blue appeared as red, orange or yellow with the different backgrounds.

The results for both H and F give no evidence of color contrast with the green background, in spite of the fact that the color contrast was so strong in central vision that both the black and the white screen appeared reddish. (This reddish tinge is completely lost when the eye is turned out through an angle of 45 to 50 degrees.) It will be seen on examination of the results that the orange and the yellow stimuli show a de-

cidedly greater tendency to appear red with the white than with the green background. In fact, the results with the green background represent a mean between the results obtained with the white and those obtained with the black backgrounds. This is just what we should expect as the result of brightness contrast, since the green is matched in brightness by a light middle gray, but it is just the opposite to what we should expect as the result of color contrast.

The change produced in the brightness of the color by throwing the after-image of a white or a black screen upon it, is fully as marked with the green background as with any other background. Orange and yellow appear more reddish when darkened by the black after-image than when brightened by the white after-image. (See pp. 46-47 for summary of results.) With the green background the after-image for blue is invariably yellow when it is projected on the white screen, and orange or red when it is projected on the dark screen.

These results show (1) that the effect produced on the orange and yellow stimuli and on the after-image for blue were not due to any subliminal green in the background, and (2) that the brightness of a colored background is effective in determining the color seen, even though the color of this background is ineffective.

TABLE XVI

OBSERVER H.

Stimulus, Orange.

These results are simply a summary of the Tables XVIII and XIX.

Color Seen Designated by Letters—i. e., R = Red, etc.

Background.	Slide.	R.	OR.	RO.	O.	OY.	Y.	No Color Seen.	Total.
White	White	3	3	1	2	—	—	1	10
White	Black	—	1	3	3	1	—	2	10
Green	White	2	1	2	2	—	—	2	9
Green	Black	—	1	4	2	—	—	2	9
Black	White	—	1	5	6	1	—	1	14
Black	Black	—	—	—	4	7	3	3	17

TABLE XVI (Continued).

		<i>Stimulus, Yellow.</i>						
White	White	2	2	2	2	1	—	3
White	Black	—	—	—	—	—	10	1
Green	White	—	1	—	3	2	2	1
Green	Black	—	—	—	—	2	7	—
Black	White	—	—	—	3	2	2	—
Black	Black	—	—	—	—	—	7	—

Stimulus, Blue.

		After-image Designated by Name—Yellow, etc.			Total.
Background.	Slide.	Yellow.	Red or Orange.	No After-image.	
White	White	14	—	9	23
White	Black	1	5	20	26
Green	White	13	—	1	14
Green	Black	—	5	9	14
Black	White	5	—	1	6
Black	Black	—	4	2	6

TABLE XVII

OBSERVER F.

Stimulus, Orange.

		Color Seen Designated by Letter—R = Red, etc.								Total.
Background.	Slide.	R.	OR.	RO.	O.	YO.	OY.	Y.	Not seen.	
White	White	5	—	2	2	—	—	—	4	13
White	Black	2	1	3	2	—	—	—	5	13
Green	White	1	—	2	7	—	—	—	3	13
Green	Black	1	—	1	6	—	1	2	2	13
Black	White	—	—	—	2	3	—	—	—	5
Black	Black	—	—	—	—	—	3	2	—	5

Stimulus, Yellow.

		O.	YO.	OY.	Y.	Not Seen.	Total.
White	White	3	—	—	5	9	17
White	Black	1	—	—	10	3	14
Green	White	1	1	—	11	—	13
Green	Black	1	—	1	11	—	13
Black	White	—	—	—	4	1	5
Black	Black	—	—	—	5	—	5

Stimulus, Blue.

		After-image Represented by Letter—R = Red, etc.						Total.
Background.	Slide.	R.	OR.	O.	OY.	Y.	None Seen.	
White	White	—	—	—	—	15	6	21
White	Black	2	—	2	—	—	17	21
Green	White	—	—	—	—	12	1	13
Green	Black	1	2	1	1	—	8	13
Black	White	—	—	—	—	4	1	5
Black	Black	—	—	1	—	—	4	5

Explanation of Tables XX-XXIV inclusive.—The stimulus given is designated in the vertical column marked 'stimulus,' the color-tone as it appeared to the observer in the vertical column marked 'color seen.'

The degree of eccentricity at which the stimulus was given is designated by the figures at the top of the tables (*i. e.*, 60°, 65°, etc.).

The number of times the stimulus was observed at a given fixation point is shown by the figures in the vertical columns, the tone of the colors as seen being designated by the horizontal column in which the figure is placed. The Arabic numerals give the number of times the stimulus was seen as a color, the Roman numerals the number of times the stimulus failed to be seen as a color. Numerals in parentheses represent judgments about which the observer was doubtful.

The suffixes (*i. e.*, I_b , etc.) designate the character of the after-images perceived. R, red; Y, yellow; G, green, etc. BG, blue-green; OY, orange-yellow, etc. G-O means that the observer was unable to tell whether the after-image was green or orange, or that the two colors were not fused.

All observations were made with the nasal half-meridian.

TABLE XX, A.

OBSERVER P.

White Background.

Stimulus.	Color Seen.	Size of Stimulus.	60°	65°	70°	75°	80°	85°	90°
Yellow	Yellow	12 mm.							I
"	Red	"	I_b	I_b	I_b	I_b	I_b	2_b	$I_b(I_b)$
"	Yellow	5 mm.			(I) _b	(IV) _(b)	(II) _(b)	III(I)	I
"	Red	"	2_g	(I_b)	3_b				
Or.-Yel.	Or.-Yel.	12 mm.							
"	Red	"	I_b	I_b	I_b	I_b	I_b	3_b	2_b
Orange	Orange	"							
"	Red	"	I_b	I_b	I_b	I_b		$2_b, I$	3_b
"	Orange	5 mm.							
"	Red	"			I	I, I_b	II		
Red	"	12 mm.				I		2	
Blue	Blue	"		I_y	I_y	I_y	I_y	I_y, I	II, I_y
"	"	5 mm.	$2_y, I$	(I_y), I_y	II _y	I	II		

TABLE XX, B.

OBSERVER C.

White Background.

Stimulus.	Color Seen.	Size of Stimulus.	70°	75°	80°	85°	90°	92.5°
Red	Red	12 mm.			I _b	I		
Orange	Orange	"					I _b	
"	Red	"			I, I _b	(I) _b , I	2 _b , I (I _b)	(I) _b , I _b
Or.-Yel.	Or.-Yel.	"					I, I _b	
"	Red	"			2 _b	4 _b	I _{gb}	
Yellow	Orange	"				I		
"	Red	"			I	I		
Blue	Blue	"			2 _y	I _y , 2 _o	II, 2 _{yo}	III

TABLE XXI, A.

OBSERVER Y.

*White Background.**Size of Stimulus, 12 mm.*

Stimulus.	Color Seen.	75°	80°	85°	90°
Red	Red				
Orange	Orange		3 _{gb}	(I) _{gb} , I _b	
"	Red		2 _{gb}	2 _{gb}	
Or.-Yel.	Or.-Yel.				
"	Or.-Red		2 _{gb}		
"	Red		I _{gb}	(I), I _b , I	(I)
Yellow	Yellow				
"	Red		I _b	I _b	(I)
Blue	Blue		I _y	(I _y), I _y	I, I _y

(Note.—The G. B. after-image was almost pure blue.)

B.

OBSERVER F.

*White Background.**Size of Stimulus, 5 mm.*

Stimulus.	Color Seen.	50°	55°	60°	65°	70°	75°	80°	85°	90°
Red	Red				I _b	I _b , I	I			I
Orange	Orange			I _b	II _b , I		I _b			I
"	Red	I _b	I _b	I _b		I, I _b				
Yellow	Yellow		I _b		I _b	II	I _b , I			
"	Orange	I		I _b	I _b					

Blue not seen beyond 60°.

C.

OBSERVER R.

*White Background.**Size of Stimulus, 12 mm.*

Stimulus.	Color Seen.	45°	50°	55°	60°	65°	70°	75°	80°	85°	90°	95°
Red	Red				I _b	III, I	II _b , I, I					
Orange	Orange				2 _b ^r	I _b ^r	3 _b , I	I		I	I	
"	Red					I _b		I _b	2 _b	I _b	I	
Or.-Yel.	Or.-Yel.				2 _b		2 _b			I	I	
"	Orange				I _b ^r	I _b	I ^r	I _b ^r	2 _b	I _b	I _b	
"	Red								I _b			

D.

OBSERVER A.

*White Background.**Size of Stimulus, 12 mm.*

Stimulus.	Color Seen.	45°	50°	55°	60°	65°	70°	75°	80°	85°	90°	95°
Red	Red		I _b	I _b					I _b			
Orange	Orange								I _b	II _b		
"	Red			2 _b	I _b							
Or.-Yel.	Or.-Yel.								I _b , I			
"	Orange											
"	Red			2 _b	2 _b			I _b	I _b	I _b		
Blue				I _y	2 _y			I _y	2, I _y	?, I _y		

E.

OBSERVER PR.

*White Background.**Size of Stimulus, 12 mm.*

Stimulus.	Color Seen.	65°	70°	75°	80°	85°	90°
Red	Red				I	I	I
Orange	Orange				I	I	I
"	Red			3		I	
Or.-Yellow	Or.-Yellow						
"	Orange		I	I _b	I _b , I	4	
"	Red						
Yellow	Yellow						
"	Orange			I _b		2	
Blue	Blue					I _y	I _y , II
Grey	Grey				I	I	I

TABLE XXII.¹⁷

OBSERVER P.

Dark Background. Size of Stimulus, 5 mm.

Stimulus, or. yellow.

Seen as yellow 60°-85° (inclusive) After-image blue.

Not seen 90° No after-image

Stimulus, orange.

Background, dark.

Seen as yellow 60°-90° After-image blue.

(At 80° seen twice as orange.)

Dark background and black hood.

Seen as yellow 70°-90°

(Seen as orange once at 80°.)

Stimulus, red.

Dark background.

Peculiar unfused mixture of red and yellow 60°-85°

Not seen 90°

Dark background and black hood.

Orange or orange yellow 70°-85°

Not seen 90°

TABLE XXIII.

A.

OBSERVER Y.

*Dark Background.**Size of Stimulus, 12 mm.*

Stimulus, red.

Seen as orange 60°-65°

Seen as orange yellow 75°-90°

Stimulus, orange.

Seen as orange 60°-75°

Seen as orange yellow 85°-90°

Stimulus, orange yellow.

Seen as orange yellow 60°

Seen as yellow 75°-90°

B.

OBSERVER C.

*Dark Background.**Size of Stimulus, 12 mm.*

Stimulus, red.

Peculiar unfused mixture of

red and yellow—film of

red over bright yellow 70°-90°

Stimulus, orange.

Same unfused red and yellow 85°-90°

Stimulus, orange yellow.

Unfused mixture orange

and yellow 70°-80°

Stimulus, yellow.

Seen as golden yellow or

slightly orange yellow 70°-92.5°

¹⁷ The stimuli were not given nearer the center than the innermost point designated in the tables, *i. e.*, orange for observer P was not given at a less degree of eccentricity than 70° with the dark background and hood. In most cases each stimulus was given only twice at each fixation point.

TABLE XXIV, A.

OBSERVER R.

Size of Stimulus, 12 mm.

Stimulus.	Color Seen.	Back-ground.	45°	50°	55°	60°	65°	70°	75°	80°	85°	90°
Red	Red	Dark		3 _b	4 _b	3 _b	3 _b					
"	Orange ¹⁸	"			I _b	I _b	2 _b	3 _b , I	2 _b	3 _b	3 _b	3 _b
"	Yellow	"										
"	Red	"					I _b , I					
"	Orange	+ Hood	2 _{gb}	2 _{gb} , I _b					I _b		I _b	I _b
"	Or.-Yel. ¹⁹		I _{gb}		I _b , I _{gb}	I, 2 _b	I _b	I _b	I _b	2 _b		2 _b
"	Yellow		I _{gb}		I _g , 2 _b		I _b	I		I _b		
Orange	Orange	Dark	2 _b	I _b		2 _b , I	3 _b	I _b	I _b		I _b	I _b
"	Or.-Yel.	"	2 _b	2 _b	5 _b	I		I _b	I _b	I _b	2 _b	2 _b
"	Yellow	"				3 _b	2 _b	3 _b	I _b	4 _b	2 _b	2 _b
"	Orange	"	I _b	I _b	I _b		I _b	I _b				
"	Or.-Yel.	+ Hood	I _{gb}	I _b		2 _b	2 _b	2 _b	I _b			
"	Yellow		I _b	I _b	3 _b	4 _b	2 _b	I _b	I _b			

B.

OBSERVER F.

Stimulus.	Color Seen.	Size of Stim.	Back-ground.	50°	55°	60°	65°	70°	75°	80°	85°	90°
Red	Red	12 mm.	Black	I	I _b	2 _b	2 _b	2 _b	3 _b	I, I _b	I, I _b	
"	Orange	"	"					I _b		2 _b	I _b	2 _b
"	Yellow	"	"									
"	Red	"	"									
"	Orange	"	+ Hood	I								
"	Yellow	"			I _b	2 _b	I, I _b	2 _b , I _v	I, 2 _b	I	I	I _b
"	Red	5 mm.	Black	I		2 _b	I _b	2 _b	I, I _b , I	I, I _b	II	II
"	Orange	"	"		I _b		I _b		I	I _b , I(I)	I	
"	Yellow	"	"				I _b	I _b				
Orange	Orange	12 mm.	"			2 _b	2 _b	I	I _b	I _b		
"	Yel.-Or.	"	"			I _b	I _b	I _b	3 _b		I _b , I	I _v
"	Yellow	"	"							2		
"	Orange	"	"									
"	Yel.-Or.	"	+ Hood	I _b	I _b	I _b	2 _b	I _v	I _b , I	I _b	2	
"	Yellow	"	"				I _b		I	2	I	
"	Orange	5 mm.	Black					I _b	I _b			I _v , I
"	Yel.-Or.	"	"		I _b	2 _b	I _b	I _b		I _v	I _b	I _b
"	Yellow	"	"			2 _b	I _b	I _b		3	I _b	

¹⁸ The orange was described in every case as being as red as or redder than the orange disc.¹⁹ The orange yellow was almost pure yellow.

C.

OBSERVER P.

Dark Background. Size of Stimulus, 12 mm.

Stimulus.	Color Seen.	60°	65°	70°	75°	80°	85°	90°
Yellow	Yellow					I _b	I _b	3 _b , I.
Orange	Orange	I _b		I _b		I _b	2 _b	I _b
"	Or.-Yel.							2 _b
"	Yellow							2, 2 _b
Red	Red					2 _b	2 _b	
"	Orange			I _b			2 _b	I _b
"	Yellow							4

Orange yellow seen as yellow, 70°-85° inclusive.

4. *Conclusions to Section A.*(a) *Effect of the Brightness of the Stimulus on its Color-tone.*

In peripheral vision the tone of the colors at the more refrangible end of the spectrum is decidedly dependent on the brightness of the stimulus. When the stimulus is sufficiently darkened, red, orange and yellow all tend to appear red in the more peripheral regions. The most pronounced results were obtained when the stimulus was darkened by contrast with a white background. In this case orange, and even yellow, appeared as pure, saturated red from the outer paracentral zone to the extreme periphery. Orange and yellow were seen as red, in this case, beyond the outer limits at which the red stimulus could be distinguished as red.

When the colors were relatively light, as was the case when they were observed on the black or gray backgrounds, the colors just mentioned appeared as yellow or yellow-orange at the periphery, or at the same points at which they appeared red with the light background. The most striking results were obtained when the color was brightened by contrast with a very dark ground.

Changes in brightness seem to have little effect on the colors at the less refrangible end of the spectrum. Purple becomes if anything slightly redder when darkened.

D.

OBSERVER F.²⁰

Stimulus.	Color Seen.	Size of Stimulus.	Back-ground.	25°	30°	35°	40°	45°	50°	55°	60°	65°	70°	75°	80°	85°	90°
Carmine	Carmine	5 mm.	Black				I_g	I_g, I	3						I		
"	Violet	"	"							I							
"	Blue	"	"							2		2	2	2	2	(I)	I
"	Carmine	12 mm.	"							I	2	3	5	4	4		
"	Violet	"	"														
"	Blue	"	"														
"	Carmine	"	+	3													
"	Violet	"	Hood														
"	Blue	"	"														
"	Violet	"															
"	Blue	"															
Violet	Violet	5 mm.	Black	3_g	I_o, I_g $3_g, I_o$ $2_g, I$	I_y, I_o I_g, I_o I_g, I_o 2_{yg}	I_y, I_o I_g, I_o I_g, I_o 3_{gy}	I_g I_g, I_o $2_{gy}, I_g$ I_g	I_o, I	I_y	I_o	I_o	I_o, I	I	I	2	2
"	Blue	"	"														
"	Violet	12 mm.	"														
"	Blue	"	+														
"	Violet	"	Hood														
"	Blue	"	"														
"	Violet	"	"														
"	Blue	"	"														

²⁰ Suffix G-O means that F. was unable to tell whether the after-image was green or orange.

३३

OBSERVER R.

Size of Stimulus, 12 mm.

[illegible]

F.

OBSERVER R.

Size of Stimulus, 12 mm.

Stimulus.	Color Seen.	Background.	35°	40°	45°	50°	55°	60°	65°	70°	75°	80°	85°	90°
Carmine	Carmine	White		I _g	I	I	I, I	III, I _y	I, I _{oy}	IV	II	I	II	
"	Violet	"			I _g			I _y , I	I					
"	Carmine	Grey						I	I _{oy} , I _y , I	3, I _g	II, I _g , I	I, I		
"	Violet	"							I _y , I	I	4	2 _o	I	
"	Blue	"										I _y , I		
"	Carmine	Black							I _g		I, I	3 _g	I ^v , I ^v _g	2
"	Violet	"						I _b				I	I ^b	1

*(b) Effect of Brightness on the Limits for the Color Fields.**(General Conclusions Continued from Page 55.)*

The limits for blue and violet are little affected by brightness changes. Yellow and carmine have the widest limits when the stimulus is very bright, as when it is observed on the dark background either with or without the black hood. The red component of orange is emphasized when the color is darkened by contrast with the white background, orange appearing, in this case, as pure red at the same points at which it appears yellow with the dark ground. The limits for red are practically the same with the light and the dark backgrounds (see p. 23), but the limits for orange and red are both decidedly decreased when these colors are brightened by contrast with the dark background covered by the black hood.

(c) Effect of the Brightness of the Projection-ground on the Tone of the After-image.

The after-image for blue and green-blue is yellow when projected on a white ground and red or orange when projected on a dark ground. When projected on a dark ground or observed in a darkened field, the after-image for blue is pure, saturated red at the extreme periphery. The after-image for violet, which is yellow with the light screen, appears, in most cases, greenish when projected on the dark screen. The after-image for red, orange and yellow is blue, regardless of the color-tone of the stimulus or of the brightness of the projection-ground.

The table gives the color-tone of the stimulus and of the after-image under various brightness conditions. When there is no specification concerning the brightness, the color is one which seems not to be affected by changes in brightness.

	Stimulus.	After-image.
Green blue	Seen as <i>blue</i> .	<i>Yellow</i> on light projection ground.
Blue		<i>Red</i> on dark projection ground.
Red	Seen as <i>yellow</i> when relatively light.	<i>Blue</i> .
Orange		
Or. yellow	Seen as <i>red</i> or orange when relatively dark.	
Yellow		
Violet	Seen as <i>blue</i> .	<i>Yellow</i> with the light projection grounds. <i>Greenish</i> with the dark projection grounds.

Green is not followed by after-images at the more peripheral points. (The limits for the Hering green are much narrower than those for any other color.) A red after-image follows green at a greater degree of eccentricity with the dark than with the light projection ground. It should be kept in mind that the green of the Hering series is very unsatisfactory because unsaturated.

(d) Effect of Changes in Brightness on the Relative Frequency and Saturation of the Peripheral After-image.

Colored after-images follow color stimuli in a much larger percentage of cases in full than in decreased illumination. They also undergo a steady decrease in saturation as the brightness conditions under which they are observed are varied from maximum to minimum illumination. Only blue and yellow after-images were obtained, however, under the conditions which gave the largest total percentage of after-images and also, in general, the most saturated after-images (*i. e.*, when the after-image is observed on the white background in full illumination). Red after-images only occur when the projection-ground is dark or the general illumination is decreased (or under the conditions which give the smallest total percentage of after-images). These red after-images are, at least in the case of certain observers, fully as saturated as any peripheral after-images experienced under any brightness conditions. The very infrequent and poorly saturated green after-images experienced at the periphery require a darker projection-ground for their development.

At the upper brightness extreme (*i. e.*, when the stimulus and after-image are observed on a white background) characteristic after-images sometimes follow unperceived color stimuli.

(e) Individual variation.

Decided quantitative individual variations are shown throughout the results. These variations, however, seem to be due to the fact that the color-tone of stimuli and of after-images is more dependent on brightness conditions in the case of certain observers than in the case of others. The same general effect of brightness on color is apparent in all the results. For example, most of the observers saw the after-image for blue on the dark projection-ground, as pure red, but a few of them saw it simply as orange. The change due to the darken-

ing of the after-image is toward red in both cases, since the after-image for blue on the light projection-ground is always yellow. The results seem perfectly consistent throughout with respect to the individual variations. That is, if the results for a certain observer indicate a very marked effect of brightness in one experiment, they show a correspondingly decided effect in all the experiments with this same observer. For example, P obtained a red after-image for blue on the dark projection-ground and saw the orange-yellow stimulus as red when it was darkened by contrast with the white background. R got an orange after-image for blue on the dark projection-ground and saw orange-yellow simply as orange on the white background. P also saw red as red at a much greater degree of eccentricity than R with the dark background.

SECTION B. OBSERVATIONS MADE WITH THE URFARBEN.

The work of the preceding section demonstrates clearly the necessity of maintaining constant conditions of illumination in a given series of tests, or at least of recognizing the effect of changes in the illumination not only on the brightness of the colors perceived (Purkinje phenomenon) but also on their color-tone. Consequently the work with the Urfarben has all been done on bright days when there was no variation in the illumination from day to day. It was found possible, by working with each observer at the same hour on different days, and by omitting the work on cloudy or dark days, to avoid any appreciable change in the general brightness conditions.

1. *Determination of the Urfarben.*

All four Urfarben were determined for observers F, Y and P. Owing to the time limitation only the Urroth could be determined for observer C.

Method.—The four colors were first found which did not change in tone as they were moved from the center to the periphery of the visual field. These colors were then mixed in pairs (*i. e.*, Urroth with Urgrün and Urblau with Urgelb), in such proportions that they canceled each other, giving gray. The proportion of each color thus determined was then mixed

with black and white until the brightness of the color matched that of a middle gray background, when the color fell upon a region of the periphery at which it appeared as colorless. Since it is an established fact that the brightness value of a color varies from the center to the periphery of the visual field, the peripheral method of equating brightnesses (*i. e.*, the method just described) seemed preferable to any other for our work, as we are here dealing with peripheral and not with central values.

The first part of the work, *i. e.*, through the determination of the Urfarben, was very carefully carried out, all the determinations being verified by repeated series of tests. The work on the limits for the Urfarben was somewhat hurried, as it came at the end of the academic year when the observers who had been serving for several months were leaving the University. A long stretch of dark weather made it impossible to work for days at a time in the spring and so brought the completion of the work well along into the summer (see footnote 25, p. 65).

The proportions of the various colors required for the Urfarben of the different observers are given in the following paragraphs.

TABLE XXV.

*Gray Background. Determination of Urfarben**Observer F.*

Urgrün.	Urroth.	Urgelb.	Urblau.
Blue 49°	Blue 85°	Green 21.5°	Blue of series
Green 311°	Red 275°	Yellow 338.5°	

Observer Y.

Urgrün.	Urroth.	Urgelb.	Urblau.
Blue 49°	Blue 85.5°	Green 21.5°	
Green 311°	Red 274.5°	Yellow 338.5°	Blue of series
		or	
		Y. green 40°	
		Yellow 320°	

Observer P.

Urgrün.	Urroth.	Urgelb.	Urblau.
Green blue 90°	Blue 76°	Y. green 40°-48°	Blue of series
Green 270°	Red 284°	Yellow 320°-312°	

Observer G.

Urroth.
Blue 75°
Red 285°

2. Equating of the Urfarben in Brightness and Saturation.

The Urgrün and Urroth thus determined were found to be practically complementary in all three cases. That is, when mixed in the following proportions, they gave a medium gray which could be matched fairly closely by a mixture of black and white discs.²¹

TABLE XXVI.

Observer P.

158° Urroth composed of $\begin{cases} 33^\circ \text{ blue.} \\ 125^\circ \text{ red.} \end{cases}$

202° Urgrün composed of $\begin{cases} 50.5^\circ \text{ green blue.} \\ 151.5^\circ \text{ green.} \end{cases}$

Observers Y. and F.

210° Urgrün composed of $\begin{cases} 181^\circ \text{ green.} \\ 29^\circ \text{ blue.} \end{cases}$

150° Urroth composed of $\begin{cases} 35^\circ \text{ blue.} \\ 115^\circ \text{ red.} \end{cases}$

The proportions for the Urblau and Urgelb which gave a gray when mixed on the color-mixer, were as follows:

198.5° Urblau—composed of the blue of the series.

161.5° Urgelb—composed of 10° green + 151.5° yellow.

It was found possible to vary somewhat the proportions, given above for the Urblau and the Urgelb, without making the colors unstable, *i. e.*, without causing them to undergo a change in tone as they were moved from the periphery to the centre or *vice versa*. The possible variation was considerably greater for the Urblau and Urgelb than for the Urroth and Urgrün. The Urgelb determined for F and Y was stable also for P.

The four Urfarben, in the proportions just given, were equated in brightness with the same middle gray background. Thus we have the four Urfarben, in such proportions that they are equated with each other and with the middle gray background in brightness, and that each of the Urfarben is of equal saturation with its complementary Urfarbe.

²¹ It will be seen that the proportions of the colors composing the Urfarben as given here are the same as in the original mixture. The gray produced by mixture of the Urroth and Urgrün was slightly reddish. The change in brightness necessary to equate the colors in brightness would change somewhat their saturation.

3. *Extent of Fields for the Urfarben.*

The limits of the colors thus equated in saturation and brightness were as follows (for table of limits see p. 64) :

TABLE XXVII, A.

OBSERVER Y.

Stationary Fixation.

<i>Urroth.</i>		<i>Urgrün.</i>
1. Black background.		
Seen surely at 60°.	60°.	
Suggestion of color at 62.5°.		
2. Gray background.		
62.5°.	65°.	
3. White background.		
Sure of red like Urroth at 61°.	Sure of Urgrün at 60°	
Unsaturated red scarlet (<i>i. e.</i> , redder than Urroth) from 65° to 61°. If anything slightly redder than center at 61°.		
<i>Urblau.</i> ²²		<i>Urgelb.</i>
1. Black background.		
90°.	90°.	
2. Gray background.		
85°.	85°.	

B.

OBSERVER F.

Stationary Fixation.

<i>Urroth.</i>		<i>Urgrün.</i>
1. Black background.		
52°.	50°.	
2. Gray background.		
60°. (Color not well saturated until brought into 55°.)	55°. (Limits not absolutely sure until color brought into 47.5°.)	
3. White background.		
At 50° seen clearly, but redder than Urroth—almost carmine.	Saturated red after-image at 55°.	
	50°.	
<i>Urgelb.</i>		<i>Urblau.</i>
1. Black background.		
92.5°. Determinations were not made further out than 92.5°. The color was very well saturated out to this point.	87°. Very well saturated.	
2. Gray background.		
90°.	90°.	
3. White background.		

²² No determinations were made between 85 and 90 degrees with the gray background. The colors were both clearly seen at 85° and not seen at 90°

The limits were not determined with the white background as the yellow which was Urgelb with the other backgrounds was orange with the white background.

Moving Fixation Point.

Gray Background.

Urroth at 57°.

Bluish from 80°-70°.

Reddish from 70°-60°.

Urgrün at 53°.

Bluish gray from 75° to 65°.

C.

OBSERVER P.

Stationary Fixation Point.

<i>Urroth.</i>	<i>Urgrün.</i>
1. Black background.	
Urroth at 49°.	51°.
2. Gray background.	
45°.	44°.
3. White background.	
55°.	50°.
<i>Urgelb.</i>	<i>Urblau.</i>
1. Black background.	
90°. (See once out of 4 tests at 92.5.)	92°.
2. Gray background.	
85°.	84°.
3. White background.	

The limits were not determined with the white background. The Urgelb appeared as orange at about 85°, grew more nearly yellow at about 55° and appeared practically pure yellow at the fovea.

TABLE XXVIII.

Limits for the Urfarben given in Degrees from the Fovea.

	Background.	Urgelb.	Urblau.	Urroth.	Urgrün.
	Black	92.5	87	52	50
O = F	Gray	90	90	60	55 (?)
	White	—	—	50	50
O = P	Black	90	90	49	51
	Gray	85	84	45	44
	White			55	50
				(redder than Urroth)	
O = Y ²³	Black	90	90	60	60
	Gray	85	85	62.5	65
	White			61	60

²³ The limits for the Urroth and the Urblau were not determined for Y with the gray background at points between 85 and 90 degrees, so that it is possible that the color-limits may have been slightly wider than 85°.

4. *Conclusions to Section B.*²⁴(a) *Effect of the Background on the Color-tone of the Ur-farben.*

The Urfarben determined with the middle-gray background were stable with the dark background in every case. With the light background the Urblau and Urgrün thus determined remained perfectly stable but the Urgelb and, to a lesser extent, the Urroth appeared slightly redder at the periphery than at the center; the Urgelb appearing decidedly orange and the Urroth slightly more scarlet as the stimulus is moved out to the periphery. If a larger proportion of green was added to the yellow, it appeared more nearly pure yellow at the periphery on the white background but greenish at the centre.

(b) *Limits for the Urfarben.*

α. *Coincidence of the Fields for the Complementary Ur-farben.*²⁵—With a given background, the limits for the Urgrün

²⁴ In this part of the investigation, the contrast between the various colors and the white or dark background was less than in our previous work, since the colors here were all matched to the middle gray background in brightness. Thus the yellow was darker than in the previous work and so presented less of a contrast with the dark ground and more of a contrast with the light ground, the blue on the other hand was lighter in the present case and so presented less of a contrast with the light background and more with the dark ground.

²⁵ It has been shown throughout our work that the results for a given stimulus vary slightly from day to day, even when the experimental conditions are kept as far as possible constant. Consequently, in most of our determinations a large number of tests was made at all points near the outer limits. The point was chosen as the outer limit, at which the stimulus was distinguished in a fairly large percentage of cases. In our work with the Urfarben lack of time made it impossible for us to determine the outer limits according to our previous method and consequently we considered the last point at which the color was ever seen as its limit.

Since the completion of the work reported in this monograph, a criticism of certain of our earlier investigations by Professor Titchener has raised the question concerning the significance of the variability of results obtained near the outer color limits. Until further evidence can be obtained, we consider the problem as unsettled. We doubt whether absolutely invariable results can be obtained at the outer color limits under conditions of daylight illumination, and question the possibility of obtaining them under conditions of artificial illumination, especially in an investigation extending, as in the present case, over several months time. The results used by Professor Titchener and Mr. Pyle in the publication referred to, show irregularities similar to those to be found in our work. It is to be noted that, in this latter case, as well as in our work,

are practically coextensive with those for the Urroth, and the fields for the Urgelb with those for the Urblau. The greatest discrepancy occurs in F's results; with the dark background the limits for the Urgelb being slightly wider than those for the Urblau, and with the gray background the limits for the Urroth being wider than those for the Urgrün. In each of these cases the difference, which is only about 5° , is too small to be regarded as signifying any definite effect of the background on the stimulus color.

The limits for the Urblau and the Urgelb are in every case wider than those for the Urgrün and the Urroth. As already stated in the historical section (p. 6) we have no way of determining whether all four colors were equal in saturation.

β. Effect of the Brightness of the Background on the Color Limits and on Color Saturation.—The change of background seems to have no great effect on the limits for the Urfarben, though the Urgelb was perceived at the greatest degree of eccentricity with the dark ground and the Urgrün and Urroth at the greatest degree of eccentricity with the gray or light backgrounds. The colors were reported by all observers as much less saturated on the light than on the dark background. The Urroth and Urgrün appeared, if anything, more saturated on the middle gray than on the dark background, and the Urblau and Urgelb most saturated on the dark background.

The results obtained with the Urfarben before they had been equated in brightness and saturation indicate that the widest limits for the Urroth and Urgrün would be obtained on the middle gray background, and for the Urblau and Urgelb on the dark background. The limits were carefully determined only on the middle gray background.

In all our previous work, blue seemed to be more stable the results with yellow and blue were much more regular than those with spectral red and green. In our work, the results with the Urroth and Urgrün showed as little variability as those with blue and yellow. For a more complete statement of the case see, *Jour. of Philos., Psychol. and Sci. Methods*, Vol. IV., 1909, pp. 398-403.

It is to be noted, moreover, that in so far as our results do not deal specifically with the determination of limits, but rather with the relative saturation of the stimuli and after-images and with the quality of the same, they are, in general, so pronounced as to be valid regardless of minor changes in illumination.

than red, yellow or green, and green more stable than the two remaining colors. The results with the Urfarben seem to show the same relative stability of these several colors, in that (1) the red and the green required the addition of blue before they became even approximately stable, and that the yellow which was the Urgelb with the dark or gray backgrounds was decidedly orange with the white background; and (2) the Urblau and the Urgrün were more stable than the Urroth and the Urgelb at the brightness extremes.

(c) *Individual Variation.*

It will be seen that the proportions of the different colors required for the Urfarben were very nearly the same for observers Y and F, and that the proportions for C, in so far as they were determined, were practically the same as those for observer P. Yet there was a decided difference between the results for P and C on the one hand, and those for Y and F on the other. That this difference is not due to any chance variation is shown by the fact that repeated determination of the Urfarben invariably gave the same proportions for the same observer. Moreover, repeated experiments with Y and P, showed that any slight change in the proportions of the Urroth and the Urgrün, as determined for each observer, caused the colors to appear as blue or yellow at the periphery. Y's Urroth, which required 10.5° more blue than P's (see p. 61) was distinctly bluish for P from 55° to 90° , while P's Urroth changed to yellow at the periphery (*i. e.*, at a point between 55° and 60°) for Y. These tests were repeated on different days and the same results obtained on each occasion.

It is interesting to note that the individual variations here are exactly in agreement with the general tendencies shown throughout the work. P and C reported the reds seen at the periphery as especially well saturated, and showed a somewhat greater tendency than other observers to see all red, orange and yellow stimuli as red. It seems quite natural that the observers for whom pure red and orange undergo the least change toward yellow at the periphery should require less blue for their Urroth than do those observers for whom these colors undergo a more decided change toward yellow.

III. GENERAL CONCLUSIONS AND THEORETICAL DISCUSSION.

A. GENERAL FORMULATION OF CONCLUSIONS.

I. Red is experienced in the outer color zone (*i. e.*, the blue-yellow zone) under the following conditions: (1) When an orange or yellow stimulus is observed on a white background and so appears relatively dark by contrast with the background, and (2) when the after-image for blue is observed on a black projection ground or in a darkened room.

II. Any sufficient darkening of the red, orange or yellow peripheral stimuli causes them to appear either as colorless or reddish at the same points at which they appear yellow when relatively light, though the method just mentioned, namely, that of darkening the color by brightness contrast with the white background, gives more striking results than we have obtained by any other method of darkening the color.

III. The brightness of the projection-ground has a very decided effect on the color-tone of the peripheral after-image. The after-image for blue and green-blue is yellow when projected on a light background and red or orange when projected on the dark background. The after-image for violet is yellow when projected on the light ground, but shows a tendency to appear greenish on the dark ground. The after-image for orange or yellow is blue, regardless of the brightness of the projection-ground or of the color-tone of the primary sensation (see pp. 58-59).

IV. The relative frequency with which chromatic after-images follow peripheral color stimuli, as well as the saturation of the peripheral after-image, seems to be directly correlated with certain brightness conditions. After-images follow perceived color-stimuli almost without exception when the observations are made under conditions of brilliant illumination, or when the after-image is projected on a light screen in medium illumination. As the brightness of the projection-ground or of

the general illumination is decreased, after-images become relatively infrequent, and they are entirely lacking even for very intensive stimuli when the observations are made in an entirely darkened field. (Cf. experiments with black hood, p. 44.) As the illumination or the brightness of the background is decreased, the after-images grow steadily less saturated. On the white background, in brilliant illumination, the saturation of the peripheral after-image is equal to, or greater than, that of the stimulus, a relation which is gradually reversed as the illumination is decreased or the background darkened. The main exception to the above statement occurs in the case of the red after-image for blue and for green-blue. This after-image which is observed only under conditions of decreased brightness, is reported as the most saturated after-image obtained in peripheral vision and as very much more saturated than the stimulus which conditions it. The observers who obtained this red after-image were the ones who showed throughout the entire work the strongest tendency to see red at the periphery. The exception just mentioned does not hold for those observers who got an orange instead of a red after-image for blue on the dark background. In these cases the orange is described as clear and fairly well saturated but as distinctly less saturated than the stimulus. Our work suggests that brightness conditions could be obtained which would emphasize more decidedly the green after-image. The most favorable projection ground would probably be a grey somewhat lighter than that for the red after-image.

It is important to state here that there seems to be a degree of illumination at which the red after-image is described as at maximum saturation and that any decrease or increase of the illumination below or above this point results in a decided decrease in the saturation of the after-image in comparison with that of the stimulus color. That is, at a certain degree of illumination—a degree of illumination distinctly less intense than that of maximum saturation for other colored after-images—the red after-image appears as much more saturated than its stimulus color, but as the illumination is decreased the after-image gradually becomes less saturated than the stimulus-color. Consequently the relation between the brightness conditions and the relative satura-

tion of the after-image seems to hold for the red after-image as it does for other colors. But the illumination for maximum saturation is less in the case of these red after-images than in the case of any other peripheral after-image, a fact which is in perfect agreement with all our findings concerning the peculiarities of the peripheral sensitivity to red. The general correlation between achromatic adaptation and the peripheral after-image can be nicely illustrated by taking a series of tests with given stimuli in a room whose walls are black and then repeating the experiment with the same stimuli—and even with the same background—in a room whose walls are white, or by making the same series of tests on a white and then on a black background in a room of medium illumination.

IV. When the observations are made on a white background in brilliant illumination, after-images sometimes follow unperceived color-stimuli. The color-tone of these after-images is the same color-tone as that obtained for perceived color-stimuli under the given brightness conditions.

V. The *Urfarben* determined with the middle gray background were stable with the dark background. With the white background the *Urblau* and the *Urgrün* were perfectly stable, but the *Urroth* became slightly redder at the periphery than at the center, and the *Urgelb* grew decidedly more orange. The proportions required for the *Urfarben* differ slightly for different observers, but show no variation at different times for the same observer. The limits for each of the *Urfarben* were practically the same as those for its complementary *Urfarbe*. The limits do not seem to be appreciably affected by moderate changes in the brightness of the *Urfarben* (see footnote, p. 65).

VI. The individual variations shown throughout the work seem to be explained by the fact that certain observers have a greater peripheral sensitivity than others to red. The same observers who exhibited the greater sensitivity to red in the first part of the work, required less blue for their *Urroth* than the other observers.

B. THEORETICAL DISCUSSION.

The theoretical discussion will be taken up under the following heads: (1) The sensing of red at the periphery; (2)

the effect of brightness on color-tone of the stimulus and the after-image; (3) the comparative sensitivity of the peripheral retina to red and to green; (4) the qualitative relation between the stimulus and the after-image (*i. e.*, are the stimulus and the after-image complementary to each other, in terms of either central or of peripheral vision?); (5) (*a*) the effect of brightness on the relative frequency and saturation of after-images; (*b*) the colored after-images of unperceived color-stimuli.

1. *The Sensing of Red at the Periphery.*

It has been suggested that the red perceived in the more peripheral regions is due to the 'tendency to interpret certain degrees of brightness in terms of certain color-tones.'¹ If this explanation is adopted it will be necessary to suppose that the blue and yellow perceived at the periphery are also due to brightness interpretation, since red is described by all observers as, if anything, more saturated than any of the other colors seen at the outer color limits. The following facts seem to prove that the color perceived is due to the color itself as well as to the brightness component of the stimulus: (1) gray, black and white failed to appear as colors or to be followed by colored after-images; (2) colors that are matched in brightness, as in the case of the Urfarben, are distinguished from each other; (3) the limits for a color are narrower when the color is decreased in saturation than when it is fully saturated.

A more satisfactory explanation seems to be that the red-sensing process, in a somewhat modified form, exists at the periphery as well as in the more central retinal zone. The main difference between the processes in the two cases seems to be that its activity at the periphery is more directly dependent on brightness conditions than at the center. This seems to follow from the fact that certain changes in color-tone which occur in central vision when the brightness is increased or decreased occur much more strikingly in peripheral vision with much less of a change in brightness. That is, the darkening of the colors at the more refrangible end of the spectrum causes

¹ H. Thompson and K. Gordon, 'A Study of After-images on the Peripheral Retina,' *PSYCHOL. REV.*, Vol. XIV., 1907, p. 134.

them to shift toward red in central vision, but a variation in the brightness conditions which causes no appreciable change in central vision is sufficient to make orange and even yellow appear pure red in peripheral vision. This was shown by the tests in which the orange, which appeared red under certain brightness conditions in peripheral vision, was observed under the same conditions in central vision. In a few cases in central vision the color was reported as slightly redder than the orange of the Hering series, but was more often identified with it. In the same way a yellow which was decidedly red or orange at the periphery showed but the slightest golden tinge in central vision.

A question which naturally arises here is that concerning the complementary relations of peripheral stimuli. The issue is complicated by the fact that a decrease in saturation such that the color is still seen as color in central vision, causes the same stimulus to appear colorless in peripheral vision. The problem is one which requires further investigation in both central and peripheral vision.

2. Effect of Brightness on the Color-tone of the Stimulus and of the After-image.

If we admit that the red-sensing process exists at the periphery, but that its activity is more dependent on brightness conditions than in the more central zone, it will be necessary to explain how brightness can have such an effect on color. None of the theories which make brightness and color processes entirely independent of each other offers a satisfactory explanation of changes in color-tone when the only variation in the stimulus is an increase or decrease in its brightness.

The explanation of the Purkinje phenomenon which makes the changes in the relative brightnesses of colors due to the absorptive properties of the visual purple or rod-pigment (see historical section, pp. 12-13), does not explain equally well the changes in color-tone which accompany variations in brightness, both in peripheral and in central vision. This theory, as stated by Mrs. Ladd-Franklin, supposes that the rods are the organs for brightness vision alone. As the visual purple is

situated only in the rods, any increased effect of light of a particular wave-length, due to its absorption by the visual purple, will cause the stimulus to appear relatively darker or lighter, and possibly more or less saturated, but can in no way produce a change in color-tone.

It would seem quite possible for the Hering theory to make use of the explanation just given for the Purkinje phenomenon, by simply considering that the black-white substance alone exists in the rods while the two color substances, as well as the black-white substance, are to be found in the cones. If the color and brightness substances were arranged in this way, any absorption of colored light by the rod-pigment would intensify the effect of the color stimulus on the black-white substance and so increase the relative brightness of the color. But, even with this modification, the Hering theory seems to offer no explanation of *color* changes based on anything but a change in the wave-length of the stimulus.

At present we have only one hypothesis to suggest as a possible explanation for our results. This is that different intensities or brightnesses of stimulus are required for the excitation of the different color substances—*i. e.*, that a stimulus must possess a certain range of brightness, as well as a certain range of wave-lengths in order to be effective in arousing an activity in a certain color substances, and that the brightness required for the various color substances differs for the different colors at the long wave end of the spectrum. This brightness factor is effective to a very limited extent in central vision, to a much greater extent in peripheral vision. This would simply mean, that, as the color substance becomes more highly differentiated in central vision, it becomes less dependent on the brightness and more dependent on the wave-length of the stimulus. Consequently a stimulus of a given wave-length will condition an activity in a given color substance through a wider range of brightness and a narrower range of wave-lengths in central than in peripheral vision.

We seem able to explain our results most satisfactorily if we suppose that the yellow requires the greatest brightness of stimulus, and the red the least brightness of stimulus for its

excitation. The blues and greens occupy an intermediate position, requiring less brightness of stimulus than the yellow and more than the red. That such a dependence of color on the brightness of the stimulus exists seems to be shown by the fact that, in central vision,² yellow is the first color to disappear with decreasing intensity of stimulus and red the last, while blue and then green lose their color component at intensities between that for yellow and that for red.³

Another fact concerning central vision which seems to agree with our hypothesis, is that red is the first color to lose its color component as the intensity of the stimulus is increased above a certain point, so that red will appear as colorless at an intensity of stimulus at which the other colors are still distinguishable.

On the hypothesis just suggested the explanation of these changes in color-tone which accompany the Purkinje phenomenon in central vision, would be that the yellow, blue and green lose their color component at an intensity of stimulus at which red is still visible, because the color processes for the former three colors actually require a greater intensity of stimulus for their excitation than does the latter process. Orange loses its yellow component and appears pure red when its intensity is sufficiently decreased, because the intensity of the stimulus is not sufficient to condition activity in the yellow substance, but only in the red substance.

Since the color effects which accompany brightness changes in central vision were found to occur more strikingly in peripheral vision with a similar though much less extended change in brightness, we must suppose that the factor which is responsible for the change in central vision is more strongly operative in peripheral vision. If, as we have suggested, this factor is the brightness of the stimulus, the explanation for our results would be that a given color process at the periphery can be active only through a limited range of brightness. Yellow requires a greater brightness of stimulus for its excitation, while the red process is active only through the lower range of brightness

² This does not apply strictly to foveal vision.

³ As already stated, Mrs. Ladd-Franklin offers a very different explanation for the fact that a relatively dark red is still seen as red, while a relatively light blue appears colorless. (Historical section, p. 12.)

values. The process for blue seems to be active under as wide a range of intensities in peripheral as in central vision, while the process for green seems to be incapable of activity under our brightness conditions or else to be entirely lacking in the other peripheral zone.

The term brightness is used in most instances in this discussion, instead of intensity in order that there may be no question concerning its application to all cases of change in the brightness of the color perceived, whether the change is produced by varying directly the intensity of the stimulus, or by superimposing the brightness on the color by contrast with a colorless background, or by projecting colored after-images on screens of different brightness values. In the discussion of central vision the term intensity is used because, in the investigations referred to, the brightness of the color has been modified by direct changes in the intensity of the stimuli. The hypothesis suggested is only partial and makes no attempt to give any final statement concerning the way in which the brightness component affects the color processes (see p. 79).

3. *The Comparative Sensitivity of the Peripheral Retina to Red and to Green.*

The question naturally arises as to the relative sensitivity of the peripheral retina to red and to green. Our results seem to indicate that, under certain conditions, red may be sensed at a greater degree of eccentricity than green. It is true that, with the exception of the Urroth and the Urgrün, the red and the green used throughout the investigation were not equated in saturation, and that the greens are unquestionably less saturated than the reds. It is also true that the results with the Urfarben give practically the same limits for the Urroth and the Urgrün, but these colors were a bluish red and a bluish green. There seems to be no way of equating a spectral red and a spectral green, or their pigment representatives, in saturation, if the only validity of such a comparison equation rests on the cancellation of one color by another, since the two colors are not complementary and so cannot be made to cancel. The stimulus which was seen as red at the greatest degree of eccentricity was a

yellowish red (*i. e.*, orange) and its complementary color is a very bluish green, which is seen as pure blue at the periphery. Perhaps the only statement that we are justified in making is that none of the stimuli used in our investigation, including yellow-green, green, and blue-green, were seen as green at as great a degree of eccentricity as that at which certain stimuli were seen as red, and that the sensitivity to red seems to be more strongly influenced by brightness conditions than is the sensitivity to green.

This greater dependence of the red sensitivity on brightness is also suggested by the fact that the yellow-green, green and blue-green do not show as great an effect of change in brightness as do those exhibited by the yellow, orange-yellow, orange and red stimuli. That is, if we compare the results obtained with the yellow-green, green or blue-green on the white background, with those for the same stimuli on the dark background, we find very little difference in the two sets of results except that green is seen as yellow less frequently with the light than with the dark background, while the results for red, orange, yellow, and to a lesser extent for carmine, stimuli with the dark background are very different from those obtained with the light background, both in the tone of the colors perceived and in the limits for the various colors. Moreover, the after-image for both green-blue and blue comes out as red, when it is observed on the dark background and as yellow when it is observed on the light background; while the after-image for red, carmine and violet, although it tends to appear as green at a somewhat greater degree of eccentricity with the dark than with the light background is relatively infrequent with the dark background, and when seen is very poorly saturated. As already suggested, the green after-image would undoubtedly be more emphasized by a projection-ground of just the right brightness value, but further experiments indicate that the green after-image would not be obtained at as great a degree of eccentricity as the red, nor show as marked changes in color-tone with variations in the brightness conditions.

Although our results seem to suggest, under certain conditions, a more widely extended sensitivity to red than to green,

the only conclusion actually justified by our results is that the green sensitivity is not as dependent as the red on brightness conditions.

It seems difficult to explain, on the basis of any theory which makes the red and the green processes reversible, why the peripheral sensitivity to red and to green should not present exactly analogous cases. The difference, as already stated, seems to be that the red sensitivity is more dependent on brightness conditions than is the green sensitivity. The results seem to agree with our hypothesis, that in peripheral vision a stimulus must possess a certain brightness to arouse an activity in the yellow substance and that a stimulus of less brightness will arouse an activity in some other color substance or merely in the brightness substance, since at the same degree of eccentricity, a light green is seen as yellow while a dark green tends to appear merely colorless.

4. *The Qualitative Relation Between the Stimulus and the After-image.*

The question, raised by certain investigators, concerning the relation between the peripheral stimulus and its after-image (*i. e.*, whether the after-image is complementary to the primary sensation at the point stimulated) seems to depend for its answer on the brightness conditions under which the observation is made. Under certain conditions the after-image is complementary to the primary sensation at the point stimulated: An orange, for example, observed on a dark or gray ground, appears yellow but is followed by a blue after-image regardless of the brightness of the projection-ground. Under other conditions the after-image may be complementary to the stimulus as seen in central vision, though illustrations of this are somewhat infrequent: For example, a green-blue which is seen as blue may be followed by an orange after-image when the after-image is projected on a dark ground. Finally, the after-image may not be complementary to either the peripheral or the central sensation: For example, an orange stimulus, observed on a white background, appears red and is followed by a blue after-image regardless of the brightness of the projection-

ground; or a blue is followed by a red after-image when the after-image is projected on a dark ground. The following list shows the relation between the after-image and stimulus.

I. CASES IN WHICH THE AFTER-IMAGE IS COMPLEMENTARY TO THE PRIMARY SENSATION AT THE POINT STIMULATED.

Stimulus.		After-image.	
<i>Blue</i> , seen as <i>blue</i> under all brightness conditions.		<i>Yellow</i> , when projected on light ground	
<i>Red</i>	} Seen as <i>yellow</i> when relatively bright.	<i>Blue</i> , regardless of brightness of projection ground.	
<i>Orange</i>			
<i>Yellow</i>			

II. CASES IN WHICH THE AFTER-IMAGE IS COMPLEMENTARY TO THE STIMULUS AS SEEN IN CENTRAL VISION.⁴

Stimulus.		After-image.	
<i>Green-blue</i> , seen as blue.		<i>Orange</i> , when projected on dark ground.	
<i>Green</i> , seen as yellow.		<i>Red</i> , when projected on dark ground.	
<i>Violet</i> , seen as blue.		<i>Yellowish green</i> when projected on the dark ground.	

III. CASES IN WHICH THE AFTER-IMAGE IS NOT COMPLEMENTARY TO THE STIMULUS AS SEEN EITHER IN CENTRAL OR PERIPHERAL VISION.

Stimulus.		After-image.	
<i>Blue</i> , seen as blue.		<i>Red</i> , when projected on dark ground.	
<i>Red</i>	} Seen as red when relatively dark.	<i>Blue</i> , regardless of brightness of projection ground.	
<i>Orange</i>			
<i>Yellow</i>			

Two views have been held concerning the relation of the after-image to the peripheral stimulus. These, as reported by Baird,⁵ are as follows: "Both these investigators (Aubert⁶ and Franz⁷) report that the peripheral after-image is of the same color as the central—that, *e. g.*, the after-image of a purple stimulus is green, no matter at what part of the retina the after-image be aroused. That this statement is erroneous, we have succeeded in demonstrating by means of an experiment in which an intensive purple stimulus and a long exposure were employed.

⁴ Stimuli which appear the same in peripheral and in central vision are omitted here (cf. blue and yellow).

⁵ Baird, *op. cit.*, pp. 64–65. The following references (*i. e.*, footnotes 6–9, inclusive) are from Baird's text.

⁶ Aubert, H., 'Ueber das Verhalten der Nachbilder auf den peripherischen Theilen der Netzhaut,' *Moleschott's Untersuchungen*, IV., 1858, S. 220ff.

⁷ S. I. Franz, 'After-images,' *PSYCHOL. REVIEW*, Mon. Supp., III., 1899, p. 29.

We found that the application of this stimulus to different regions of the retina gave the following after-images: Green at 0° , blue-green at 25° , bluish at 40° , and dark gray (?) at 50° . The results of this experiment are fully in accord with the findings of Adamük and Woinow,⁸ who also report that the color of the after-images aroused at any retinal region is complementary to the color of the primary sensation produced at that region. Our results are further confirmed by the data contained in a more recent paper by Walther."⁹

In work under our experimental conditions it is evident that the peripheral after-image may be 'of the same color as the central,' or it may be of the color complementary to the primary sensation produced at that region, or it may fail to bear either of these relations to the stimulus, depending on the achromatic conditions under which the observations are made.

Any explanation of the effect of brightness on color phenomena would necessarily show exactly how the brightness of the stimulus effects the color processes. If the color and brightness are conditioned by activities in separate retinal substances, there seem to be two possible ways of explaining the facts: Either the brightness of the stimulus has a direct inhibitory or stimulating effect on the color processes, or the brightness primarily effects the brightness substance and the activity in the brightness substance has some differential effect on the color substance. With our present knowledge of retinal processes, it seems impossible to decide which of the above hypotheses is more probable. The fact that our most striking effects were obtained when the brightness is superimposed on the color, *i. e.*, when the brightness is largely determined by contrast with a brightness background, or when the after-image is projected on a light or dark ground, seems at least to justify the statement that the superimposed brightness acts in such a way as to inhibit, increase or modify the color activity.

⁸ Adamük and Woinow, 'Beiträge zur Theorie der negativen Nachbilder,' *Graefe's Archiv.*, XVII, 1, 1871, S. 141f.

⁹ Anthon Walther, 'Beobachtungen über den Verlauf centraler und extramacularer Nachbilder,' *Pflüger's Archiv.*, LXXVII, 1 and 2, 1899, S. 53-69.

5. (a) *Effect of Brightness Conditions on the Relative Frequency and Saturation of the Peripheral After-image.*

In addition to the very marked effect which the brightness conditions have on the color-tone of the after-image, is their equally important effect on the relative frequency and saturation of the after-image. As has been abundantly indicated,¹⁰ all our work has gone to show that a very definite correlation exists between local and general brightness conditions and the relative frequency with which after-images are observed, as well as the relative saturation of such after-images as are observed.

Baird has already reported the complete absence of peripheral after-images under conditions of complete dark adaptation. Our results in a completely darkened field agree with Baird's in showing a practical absence of after-images under these conditions.

Misses Thompson and Gordon¹¹ have suggested that the reason for this difference between light and dark adapted vision, is that white light is actually necessary for the production of the after-image on the peripheral parts of the retina. They suggest an explanation in terms of the Ladd-Franklin theory, which is in brief, that when the color molecule has been partly decomposed by a given color-stimulus, the residual portion of the molecule requires the added excitation of white light for its decomposition. This would mean that white light has some effect on the color substance, if only in the sense that it adds to the excitability of the color substance.

According to the Müller and Hering theories there seems to be no way of explaining the absolute dependence of the after-image on the general illumination, since the antagonistic or reverse process to that excited by the stimulus, would be set up in the brightness as well as in the color substance, and so condition a light or dark after-image, according to the brightness of the original stimulus. It would seem, then, that the Müller and Hering theories serve to explain the appearance of the central after-image, but not the absence of the peripheral after-image in dark-adaptation.

¹⁰ Cf. pp. 59ff., 68ff.

¹¹ *Cit.*, 133-134.

(b) The Colored After-images of Unperceived Color Stimuli.

As in our previous work after-images were sometimes perceived when the stimulus color was not distinguished. Under our experimental conditions, these after-images occurred almost exclusively with the white background in brilliant illumination. By brilliant illumination is meant the sort of light obtained from a skylight, or as in the present case, from our large north window, on a very bright, clear day in a room with white or light gray walls. The results seem so directly dependent on the illumination that it is necessary to work during the middle part of even a bright day. These after-images seem to be merely another illustration of the correlation between the illumination and the saturation and frequency of the after-image (see preceding section). They seem to represent typical results of one extreme of brightness adaptation, of which the other extreme is complete dark adaptation with its practical absence of after-images.

These after-images occur when the local brightness conditions are such as to favor the color of the after-image more than that of the stimulus. For example, with the white background and projection-ground, an unperceived yellow is sometimes followed by a clear, decided blue after-image, while under the same conditions an unperceived blue may give rise to a clear yellow after-image. In both of the cases just mentioned the stimulus is relatively dark because of brightness contrast with the white background, while the after-image is relatively light because projected on the white ground. It will be remembered that the limits for yellow are narrowest when the color is darkened by contrast with a white background and widest when the color appears light because of contrast with a dark background. Blue, for most observers, shows a similar though very much less marked effect of decrease in intensity (*i. e.*, its limits are narrowest when the color is darkened by contrast with a light background). When the blue stimulus is given on the white background, it is very dark and so not seen at as great a degree of eccentricity as under other conditions, while the yellow after-image which is light because projected on the white ground, comes out under just the conditions which most

favor the perception of yellow. When, on the other hand, the yellow stimulus is given on the white background it fails to be seen at points at which it would be clearly seen with the darker backgrounds, but the after-image, projected on the white screen, is blue, or a color affected much less than yellow by changes in brightness. The same general explanation would hold for red and orange, since these stimuli appear colorless with the white background at the same points at which they appear yellow with the dark backgrounds.

In several instances, in our later work, a red after-image has followed an unperceived green when the stimulus was given on the white background and the dark screen pushed over the color, and a green after-image was obtained for red and orange when the projection-screen was middle grey or black.

The two conditions mentioned as necessary for the after-image, can perhaps be correlated as follows: The white background drowns out the stimulus color by darkening it beyond the point at which it can be seen as color. The brighter the illumination the greater the contrast, consequently the color limits are narrowest when the illumination on the white background is greatest. Now if the after-image is projected on a screen which favors the after-image, the result is the perception of the after-image even though the stimulus color is not seen. Undoubtedly a larger percentage of after-images would be obtained for unperceived stimuli, if the projection-screens were simply determined so as to most emphasize the after-image color, and the background to least favor the stimulus color.

The failure of after-images to appear in dark adaptation seems to be analogous to the effect produced on any color by mixing it with black. That is, when a peripheral stimulus or after-image is darkened beyond a certain point it loses its color component and appears merely as brightness. This occurs when the brightness of the color is such that the color is still clearly perceived in central vision. As certain colors are changed from maximum to minimum brightness a series of color changes occurs. Particular brightness values emphasize particular colors in the case of either the stimulus or the after-image. At the upper brightness extreme, *i. e.*, when the stimulus is suffi-

ciently brightened by contrast with the dark background, or the after-image is projected on the white background, only blue and yellow are perceived at the outer color zone. As the brightness is decreased all stimuli tend to appear greenish or red instead of yellow (blue and violet are, of course, exceptions). Finally, however, a point is reached at which neither stimulus nor after-image can be recognized, and this, for the after-image, is complete dark-adaptation. With the stimulus we approached this condition when the color was darkened by contrast with a brilliantly illuminated white background.

As already stated, the facts seem to agree with those already established concerning central vision, but to show a greater dependence of the peripheral than of the central color-sensitivity on brightness conditions. The one exception to the above statement seems to be the dependence of the peripheral after-image on brightness conditions. This latter fact seems quite in agreement with the general character of peripheral results but not with the facts of central vision.

It may be that further work in central vision will show that the two cases are perfectly analogous. Certainly it is quite possible that, if central after-images are investigated under sufficiently varied conditions of achromatic adaptation, they will be found to be affected in the same general way as peripheral after-images by these conditions.

The theoretical conclusions are necessarily incomplete and purely hypothetical. As already stated, our knowledge of retinal processes is so incomplete as to render speculation of little value. Moreover, several questions raised in this investigation or suggested by it, must be settled by further experimental evidence before we can complete even the few theoretical suggestions made here. The work as it stands will at least answer several of the questions concerning the quantitative and qualitative relations of peripheral stimuli and after-images and show the dependence of both on achromatic conditions. The following statement, recently made by Baird,¹² is shown to hold only under certain achromatic conditions: "Even in light adaptation they (*i. e.*, peripheral after-images) are less per-

¹² E. B. Titchener and W. H. Pyle. 'On the After-images of Subliminally Colored Stimuli.' *Proc. of Amer. Philos. Soc.*, Vol. XLVII., No. 189, 1908, p. 377.

ceptible than are the primary images aroused by the given stimulus," whereas an hypothesis like that suggested by Franz¹³ seems utterly untenable. His statement is as follows: "This gradual, but finally absolute, lack of ability to distinguish an after-image is probably due to several factors. Aubert mentions that the periphery is more easily fatigued than the fovea; but he does not seem to consider that this may be partly mental and not entirely physiological. From observations made during the progress of the experiments it seems likely that the inability to attend to these things not in its immediate vicinity is the primary reason for the lack of images toward the periphery, and for the long durations at the fovea."

We are confident that all the phenomena described in the present monograph, including the "colored after-images of unperceived color stimuli" have some direct physiological explanation, though further work must determine the exact nature of the explanation.

¹³ Franz, S. I. *Psychol. Rev., Monograph Supplement*, Vol. III., 1899-1901, p. 30. Franz worked for the most part under conditions of dark-adaptation.

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¹This bibliography is simply intended to supplement that given by Baird (*op. cit.*, 74-80). Our list includes the more recent articles which have come to our notice, as well as those prior to the year 1905 which have special reference to achromatic adaptation or are not included in Baird's bibliography.

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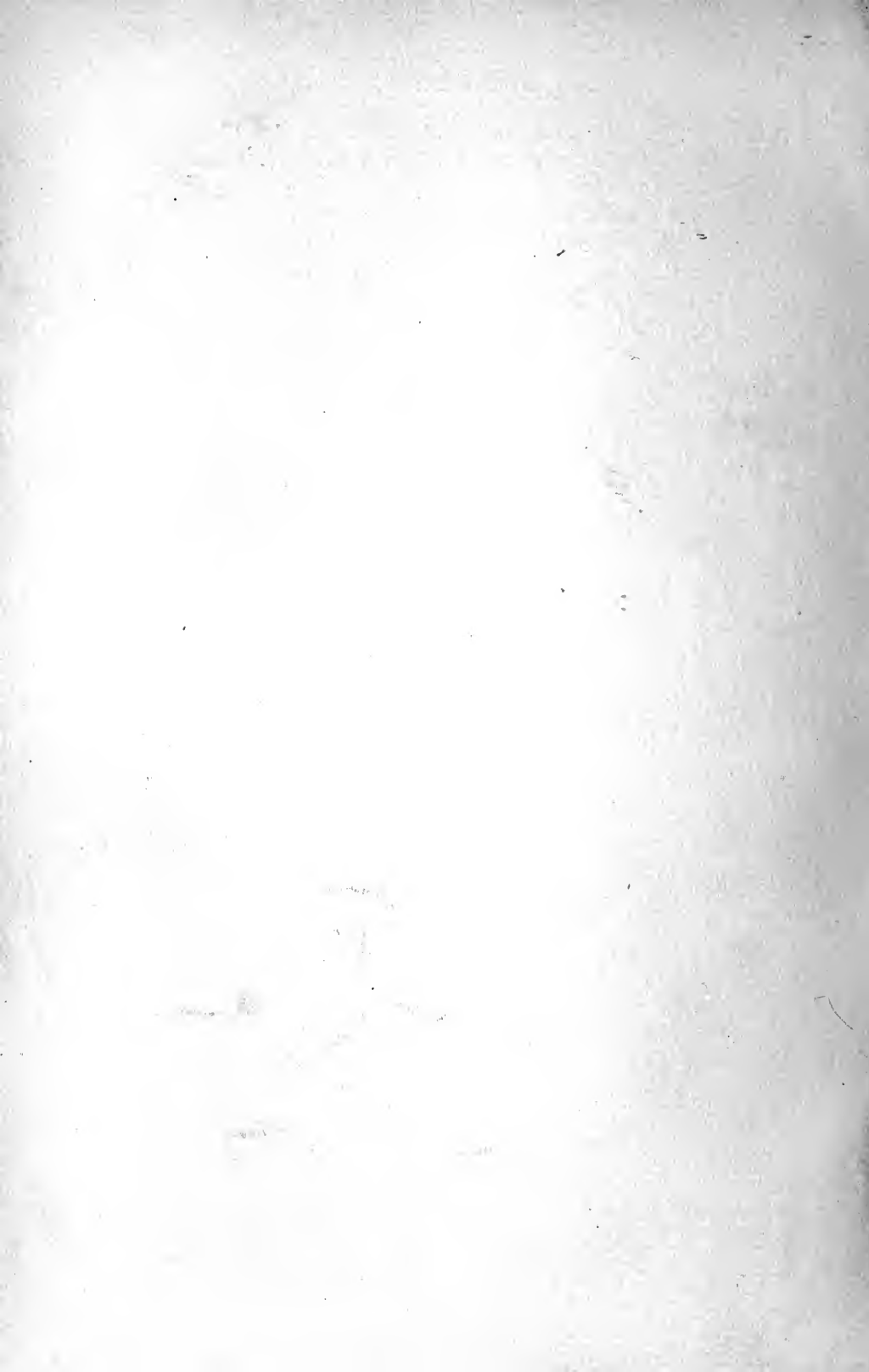
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Wellesley College Studies in Psychology No. 1

A Study in Memorising Various Materials by the Reconstruction Method

BY

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PREFACE.

The purpose of this monograph is to report and to discuss the results of a series of experiments made, with one unimportant exception, in the Psychology Laboratory of Wellesley College during the seven academic years ending with the Christmas holidays of 1908. A brief appendix outlines the history of other investigations carried on in this laboratory and published before this first volume of the Wellesley College Psychological Studies. The object of this preface is to define the writer's responsibility for the chapters of the present monograph, and at the same time distinctly to acknowledge the assistance of numerous co-workers.

As divided by their personnel, the experiments fall into four groups, namely: (1) Long series of experiments made upon the writer by advanced students; (2) shorter but parallel series of experiments made by advanced students upon one another; (3) still shorter series of experiments made by first-year students upon one another; and (4) two series of experiments made by the writer,—one upon Wellesley students and the other upon two young German women in Göttingen. By advanced students are meant students who were doing a second, third, or fourth full year of college work in psychology. Of these experimenters, only a few can be enumerated here. The names of the others are given in connection with a tabular view of the experiments at the end of the first chapter. The first-year students are by far too numerous to be listed at all, but are none the less gratefully remembered as contributors.

The students who took the largest share in the work are Miss Mary C. Smith and Miss Mabel B. Woodbury. This report embodies the results of experiments which formed the basis of theses successfully presented to the faculty of Wellesley College in partial fulfillment of the requirement for the master's degree. These theses were not prepared by their writers for publication.

The results were left in the hands of the present writer and were to be published in one paper under the three names. The writer delayed this report for the sake of working over the record in different ways and of making set after set of control-experiments. Thus, the mass of results has grown to dimensions not at first imagined. Moreover, the later work has overturned conclusions based upon the earlier experiments. Unfortunately, as the case now stands, unity and brevity require that the work of Miss Smith and Miss Woodbury should be merged with that of other contributors. The writer appropriates with especial compunction the work of Miss Smith, who not only furnished a larger mass of numerical results than any other one student has presented, but who also took part in working out the method of procedure upon which the whole investigation has hinged. Other students to whom cordial acknowledgment is due are Misses Eleanor M. Bennett, Ethel Bowman, Helen D. Cook, Maud Dewar, Sarah J. Woodward, and Helen M. Young.

The writer is responsible for the conduct of the whole investigation, served as the principal subject through almost its whole course, took the lion's share in computing and in tabulating the results, and has written this report *in toto*. She stands to the the experiments by the reconstruction method much as Professor Ebbinghaus stood to the pioneer memory-experiments published in *Ueber das Gedächtnis*. There are, however, two important differences. In the first place, the writer did not herself experiment upon herself. In the second place, the most important conclusions which may be drawn from the experiments made upon her have been confirmed by tests made upon other persons.

In serving so largely as subject, the writer had at first a purely practical and pedagogical motive. It is her duty to direct students in elementary research-work, and she is convinced that she can best direct such work by serving freely as subject. Later, as one set of experiments grew out of another, it seemed important that the subject most often employed should remain the same person. Moreover, as a matter of hard fact, the writer continued to be the only subject available for an extended and exacting investigation.

This merging of the offices of laboratory director and of subject has three obvious disadvantages. In the first place, the subject as director knows more than a subject ought to know about the purpose of the experiments and may thus be influenced by "expectation," in the technical sense. In the second place, the director as subject is certain to notice flaws in the method, as well as mistakes on the part of new experimenters, and such reflections may seriously distract attention from the task immediately in hand. In this investigation these two sources of error were not very serious, as the results will show. In the third place, as subject the director is debarred by the experimental proprieties from examining the numerical results as they come in week by week. This third disadvantage has not only been one factor in belating the report of the earlier experiments, but has also entailed, at some points, a lamentable lack of foresight in the conduct of the investigation. Over against these draw-backs is to be counted, in partial compensation, the advantage that the director as subject has an intimate knowledge of the procedure such as no mere overseer, however observant and critical, could possibly have. The greatest advantage, however, is strictly personal and practical and has no scientific value. This lies in the fact that the relation of teacher and pupil is, for the most part, merged in the mutually stimulating relationship of fellow-workers. In the pages which follow, the 'laboratory *we*' has not merely the editorial significance, but denotes a very genuine plurality.

In ending this preface, the writer wishes to express her gratitude to her own instructors, Professor Edward Bradford Titchener and Herr Professor G. E. Müller, for ideals of rigid experimentation which are none the less revered because this work falls so far short of them, and desires, above all, to thank her 'chief,' Professor Mary Whiton Calkins, for constant encouragement in research and for unstinted help in the final preparation of this study for publication.



ANALYSIS.

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CHAPTER I.

PROBLEMS.

This investigation is largely concerned with the memorising of the serial order of concrete impressions; or, more explicitly, of smells and of colors. The word *concrete* is here used as the opposite to the term *verbal* and as an equivalent to the German word *anschaulich*. The question upon which most of the experiments were designed to bear is this: Do the laws of memorising concrete impressions differ from the laws which have been demonstrated for the memorising of series of nonsense-syllables?

As a matter of historical fact, the genesis of this question was accidental. The investigation grew out of an interest in gauging the development of smell-imagery. In order of time, our interest centered, first, in the occurrence, differentiation and life-likeness of true smell-images; second, in the comparison of smells and colors with nonsense-syllables as material to be memorised; and third, in comparing our method with other memory-methods. On the other hand, in order of importance, the third interest stands first and the first third. Thus, the largest concern of the investigation is neither its first concern in time nor its most important concern in reality. The inter-connection of the chapters which follow will be clearer if a preliminary account is given of the fashion in which one question grew out of another.

A. Our interest in *smell-imagery* was stimulated by a remark of Professor Titchener upon its small importance in the mental life of civilized human beings. He calls attention to the fact that we often have an *illusion* of smell, and to the possibility that "with continued practice the power of imaging scents could be regained." He concludes, however, that as it is, the power is, for the most part, unemployed if not lost.¹ These

¹ *Primer of Psychology*, edition of 1898, p. 128.

statements attracted our special notice in the academic year 1898-1899, at a time when we were encountering experimental difficulties which were apparently due to the suggestibility of smells. We were then attempting with Zwaardemaker's fluid-mantle olfactometer to obtain from about forty students in a first year course in psychology certain statistical data in regard to the keenness of smell. This investigation was a failure and demonstrated chiefly the impropriety of using unpracticed observers in smell-work. One great difficulty lay in the inability of such observers to distinguish between a 'real' and an 'imaginary' smell. Of course, one encounters an exactly parallel difficulty in all work with stimuli of barely liminal intensity. In olfactometric work, however, one finds the error in an aggravated form. Whatever the explanation of this fact may be, the mere existence of such a difficulty in determining the smell-stimulus threshold proves the ready suggestibility of weak smell-images. Furthermore, *pari passu* with the statistical olfactometric tests of 1898-1899, we obtained questionnaire evidence, more direct if less reliable, for the occurrence of at least some smell-images in the cases of many of our observers. The interest aroused by this evidence, direct and indirect, led to continuance of the questionnaire-investigation, and finally, to the experiments in memorising series of smells. In these experiments, the question of smell-imagery soon dropped to a place of secondary importance. In this report, it will be treated as a subsidiary issue, and mere questionnaire evidence upon the subject will not be included.

B. Our experiments in *memorising the serial order of concrete impressions* began with the use of smells. The first of the smell-experiments were made in the year 1901-1902, and were undertaken as a possible, though indirect, means of gauging the suggestibility of smells in terms of smell-images proper. Our first plan was vague; our purpose was simply, first, to study the process by which series of smells could be memorised, and, second, to compare the difficulty of memorising such series with the difficulty of memorising series of nonsense-syllables.

To explain the transfer of interest from the memorising of smells as such to the memorising of non-verbal impressions as

such, it is necessary (as will soon appear) to give a brief account of the method employed in memorising and reproducing the smell-series. The detailed description of method will be reserved for Chapter III.

To test the memory of a series of smells, one must either require the subject to name (or to describe) the smells in order or must resort to some form of the reconstruction method.¹ A really direct reproduction is non-communicable, even if it should occur unmixed with verbal images. That is to say, the subject cannot by any *fiat*, without the use of words, produce in the mind of the experimenter the smells of rose, tar, almond, or whatnot, in succession. But to require the translation of the smell-experiences into words would have vitiated the experiments in so far as they were designed to throw light upon the existence and differentiation of smell-imagery. We were, therefore, confined to the second alternative, the use of the reconstruction method.

The method of procedure, when perfected, was briefly this: The odorous materials of the series were put into small bottles of uniform size and shape. The subject was blindfolded. The bottles were handed to her in strict succession at intervals of about five seconds. When the series was finished, the bottles were so mixed together as they stood on the table that the subject could not (except by a negligible chance) take them up in the original order. The first bottle was then given her again by way of initial suggestion. She was now required, to take up, blindfold and at random, the bottles one by one, and, by the aid of smell alone, to arrange them in a row or rows on the table in exactly the order in which they were first given her. She was not allowed, during the same attempt at reproduction, to smell again any bottle after she had once smelled another. A distinction was drawn between a merely 'correct,' and a 'perfect,' that is, an 'unhesitating' reproduction. A reconstruction was not accounted perfect unless (1) no bottles had been transposed, (2) no bottles which came next to each other in the series had ever been separated by a gap, and (3) no bottles which did not come next to each other had ever been placed in contact. These

¹ On the origin and application of the term *reconstruction method*, see p. 51.

rules we commonly called 'the placing-rules,' and they will, henceforth, be referred to as such. They will be discussed in some detail in Chapter III. It should, however, be noted here that since the subjects were blindfolded and could smell but one scent at a time, all memories of sequence (such memories, for example, as that 'tar came next to rose') necessarily involved the representation, direct or symbolic, of at least one 'absent member' of the series, that is, of one member not then present to sense.

If a series was not reproduced perfectly, all the bottles were again presented to the subject in the proper order, and a second reproduction was required under the same conditions as the first. This process was repeated until a perfect reproduction was obtained.

As will readily appear, the objective or numerical results of these experiments can throw no direct light upon the problem of smell-imagery. For, in the first place, by a limitation characteristic of the reconstruction method, the subject can correctly reproduce the order of the series without really representing to himself, either directly or symbolically, the individuality of any one of its members. Memories of sequence involve the representation, the imaging, direct or indirect, of absent members, but one may conceivably reproduce the series-order without any memories of sequence. That is to say, it would be quite possible, if not easy or natural, to reproduce the series-order simply by remembering in number-words, or other number-symbols, the absolute position of the links. Let us suppose, for instance, that ginger and cloves are the fourth and seventh smells in a series, and that the cloves bottle comes first to the subject's hand and the ginger bottle second. Then, he will set the ginger bottle on the proper side of the cloves and will leave space for other scents between ginger and cloves, if he simply remembers that the 'last smell' was the seventh in the series and that 'this one' is the fourth. As a matter of fact, these mental number-tags were an exceedingly important guide to most of the subjects in arranging the smell-series.¹

¹ We shall discuss later another exceedingly important form which memories of absolute position may take, namely, the association of the links of the series with different spatial positions.

It should be noted, however, in passing, that although the numerical results are largely equivocal as regards the question of method in memorising, yet the tabulated introspection of the subjects (see Chapter V, pages 115-119) fully bears out the natural assumption that memories of sequence coöperated with those of absolute position in the learning of the series-order.

But, in the second place, even if one memorises the order of the odors entirely by noting the sequences, and abstains entirely from counting and other methods of 'absolute placing,' this does not imply that one recalls the series, even partially, in terms of true smell-images. Let us suppose that the first seven smells of a series are, respectively, rose, tar, almond, ginger, chloroform, cinnamon, and cloves. Then one may memorise the order simply by repeating to oneself word-sequences such as 'tar, almond,' or 'cake-smell, sickly-sweet smell.' Or one may indulge in a series of mental pictures of a rose-bush, a tar-barrel, an almond-nut, a ginger-cake, an operating-room, a pink lozenge, and a phial of oil of cloves. As a matter of fact, the writer, who was the subject most often employed, has no true smell-imagery whatsoever, in spite of eleven years of practice in smell-experiments, an excellent discrimination for olfactory qualities, and a good capacity for memorising the order of the scents. In the writer's case, the smell-images are replaced primarily by color-images. The smell of rose, for example, is represented by a spot of rose-color, the smell of tar by a black spot, and the smell of almond by a pale violet tint, which is the subject's synæsthetic color for the word *almond*. These substitutes for smell-images will be more fully discussed in Chapter V. Enough has been said in this place to emphasise the very indirect bearing of the numerical results upon the problem of smell-imagery. Upon this question the self-observation of the subjects alone is crucial. The figures which represent a subject's achievements in memorising as compared with those of other subjects serve merely as a somewhat distant control upon her introspective statements in regard to her method of memorising.

Nevertheless, these numerical results of our early experiments with smell-series proved to be distinctly interesting quite

apart from the purpose with which they were originally obtained. The interest arose through a comparison of our results with those obtained by Ebbinghaus and by Müller and Schumann with nonsense-syllables as material and by the method of complete memorising. In making this comparison we were impressed with the following facts: First, in our form of memorising, a relatively small number of presentations of the series was required even at the outset and apart from the effect of practice. Second, in our form, practice had a continuous and marked effect which could not be neglected at any stage of the experiments.¹ Third, the increase of difficulty with increase of series-length was very small as compared with the increase found by Ebbinghaus.² These differences might conceivably be due to differences of material or to differences of method or to both. The testing of these suppositions constituted our second problem, the comparison of concrete impressions with nonsense-syllables as material for memorising, and our third problem, the comparison of the reconstruction method as used by us with other memory methods.

Experiments with smells were made according to the method described above for four successive academic years, ending with the year 1904-1905. In 1902-1903, the second year of the smell-experiments, tests were begun by the same method with colors as material, in order to discover whether or not a second kind of concrete material would show the peculiarities shown by the smells. Small squares of colored paper made up the series and were presented to the subject singly as the scent-bottles had been presented. These experiments also were continued through the year 1904-1905, but by the spring of 1904, it was fully proved that memorising the serial order of colors showed exactly the differences which were shown in memorising the order of smells when compared with the memorising of series of nonsense-syllables by the method of complete memorising. Therefore, in the year 1904-1905, experiments were made by our reconstruction method with nonsense-syllables as material in order to decide whether the differences in question were due

¹ See p. 78 below.

² See p. 87 below.

to the difference between concrete and verbal material or to the differences between the reconstruction method as used by us and the method of complete memorising. The syllables were printed by hand upon small cards and were presented to the subject exactly as the colors were presented. It was now proved, easily and absolutely, that nonsense-syllables, when memorised by the reconstruction method, show no crucial differences from smells and colors, and that the divergence of our results from those of Ebbinghaus and of Müller and Schumann is to be explained through the difference in method. Thus, late and by a very round-about path, we arrived at our third and most important problem, the characteristics of the reconstruction method as compared with other methods of memory-experimentation.

Chapters II, III, IV, and V all deal with the experiments bearing upon our second problem, the comparison of smells and colors with nonsense-syllables as material to be memorised. These experiments are placed first, not merely because they bulk largest in these studies as a whole, but because the third problem shaped itself out of their results and because the first problem cannot be satisfactorily considered apart from them.

C. Our *comparison of the reconstruction method with other methods of memory-experimentation* has been mainly directed toward finding out why the order of a series can be memorised by our form of the reconstruction method with a number of presentations which is relatively very small as compared with the number required to memorise the series itself by the method of complete memorising. To this question the following answers readily suggest themselves:

I. The mere reconstruction of the series-order is intrinsically far easier than the reproduction of the links themselves. This is the explanation which most readily suggests itself but it is also the least adequate. As a matter of fact, the subjects tested in this regard were found to learn the series itself fairly well in the process of learning the series-order. Detailed experimental evidence is not in place in this chapter. It is important, however, to affirm at this point the insufficiency of the first explanation, for if it were adequate, there would be no justifica-

tion whatever for comparing the reconstruction method with the method of complete memorising. It may, at least, be noted here that it is somewhat gratuitous to assert that it is very much harder to reproduce a series link by link in order, than it is, when the links are presented one by one, to reconstruct the order of the series with a perfect observance of the 'placing-rules' given on page 3. In the one case, each link, as it is reproduced by the subject, must suggest some other link; in the other, most of the links, as presented to the subject, must severally suggest either two other links or an idea of absolute position or number in the series. Let us suppose that the syllables *kex*, *lum*, *dat* and *wog* form part of a series of nonsense-syllables. Why should it be very much harder to recall these syllables bodily in succession than to remember when first *wog*, then *kex*, and then *dat* is presented, that the *dat*-card must be placed between the *kex*-card and the *wog*-card and must touch the *wog*-card but not the *kex*-card? As a matter of fact, the process of reproduction with both methods is more complicated than this simple statement and illustration indicate. For instance, in the reconstruction of series, memory is often eked out by inference. It seems best, however, to reserve further discussion until our experimental method has been described in detail, and until the actual numerical results and the introspective testimony of the subjects have been presented. The writer wishes at this point merely to insist that the difference between the results of the reconstruction method and the method of complete memorising cannot be disposed of, in easy and summary fashion, by saying that, in the one case, merely the series-order and, in the other, the series itself must be reproduced.

Of course, it is perfectly true that the reconstruction of a series implies directly only a mastery of the series-order, and does not necessarily involve an ability to reproduce the series-links. Experiments by this method are experiments only in memorising series-order and not in memorising series. This distinction will never be forgotten in the pages which follow, although in the chapters which precede the comparison of the reconstruction method with others, series will be said, for the sake of brevity, to be memorised when they are reconstructed perfectly in the technical sense.

II. Another answer to the question under discussion is as follows: Memorising by the reconstruction method is relatively easy because in the act of reconstruction the subject translates a merely temporal into a spatial order to which he particularly attends. After a certain amount of practice in the reconstruction of series, visual images of spots on the table, or tactile images of positions relative to the body, may spring up even when the series is presented for the first time and before the subject has made any attempt to rearrange it. Now we know, even from the most ancient mnemonics, that spatial associations are extremely valuable in memorising.¹

III. A third answer is this: With the reconstruction method a series need be presented but a small number of times because after each presentation the subject tests his knowledge of the whole series and the degree and nature of his ignorance. In the method of complete memorising, on the contrary, he may read the series any number of times he chooses without attempting to reproduce it from memory, and in any attempt at reproduction, he always reads to the end of the series as soon as the first hesitation occurs.² Indeed, the reader may perhaps be tempted to consider that one of our presentations and reconstructions was equivalent to two presentations by the method of complete memorising. A little reflection, however, shows that this view is hasty. In the process of reconstruction, the units were indeed again presented to the subject, and through this presentation, might become more familiar, but the order, that which the subject was required to reproduce, was not repeated. In the act of reconstruction, the subjects did indeed impress the order upon themselves, in so far as they already knew it, but they also impressed upon themselves their own mistakes, so that the next presentation had not merely to foster correct impressions but also to weed up errors.

IV. Still another theory is as follows: It may be that a relatively small number of repetitions is required by our method

¹ See Cicero, *De Oratore*, ii, 86 squ.

² Ebbinghaus, *Ueber das Gedächtnis*, p. 34; Müller and Schumann, *Experimentelle Beiträge zur Untersuchung des Gedächtnisses*, Zeitschrift f. Psych. und Phys. der Sinnesorgane, vi, p. 84 and pp. 187-190.

because the subject is not timed and thus 'flurried' in the process of reproduction. With some subjects the emotional disturbances involved in reciting a series of syllables learned by the method of complete memorising is considerable, and is in its results akin to stage-fright.

V. Finally, a fifth answer to the question under consideration is the following: A small number of repetitions is required by our method because the series are presented with longer intervals (1) between the units and (2) between the end of one presentation of the series and the beginning of the next. As far as verbal material (including numbers) is concerned, there is adequate experimental evidence for the belief, that, at least within certain limits, the lengthening of the intervals, either between the units or between the repetitions of the same sequence, increases the amount which can be memorised in the single presentations of the series.¹ Now the intervals between our syllables and colors, which were presented approximately at the rate of three and one-half seconds to one unit, were four or five times as long as the intervals used by Müller and Schumann and almost nine times as long as the intervals employed by Ebbinghaus in the experiments described in *Ueber das Gedächtnis*.² Moreover, in our case both the lengthening of the single presentations and the interpolation of the attempts at reconstruction between presentations lengthened the intervals between the repetitions of the same sequences.

To discuss in full the nature of the advantage given to the learner by long intervals would lead us into a thicket of evidence and argument which it is better to avoid in an introductory chapter. However, one consequence of making the intervals between the units very long is perfectly obvious: the subject has ample time to interpolate connecting links. For instance, in our own experiments, before a third link of the series was presented, *gur jal* would have full time to suggest currant jelly, scarlet and black to suggest the 'devil's mourn-

¹ See p. 43, note 1, and p. 193, note 1. The statement in the text must not be understood to mean that a slow presentation accomplishes more in proportion to the time expended than a more rapid one.

² Müller and Schumann, *op. cit.*, pp. 91 and 97; Ebbinghaus, *op. cit.*, p. 38.

ing,' and the smells of wintergreen and pine-needles to suggest a search for arbutus. In the first year of the experiments, the subjects were charged not to use such artificial links but to 'memorise mechanically.' The meaning of the regulation was made perfectly clear to them, yet it proved utterly impossible to enforce it. The smells could not be presented faster than at the rate of four seconds to each unit. The colors and syllables were also presented without apparatus¹ and slowly that the conditions might be comparable. Yet when the links were presented thus slowly, extraneous associations were certain to obtrude themselves. The conscientious subject could only struggle against these associations much as the mediæval recluse struggled in vain against the verbal images which he regarded as Satanic suggestions. This combat distracted attention from the series, and, because only in part successful, prevented uniformity in the experimental conditions not only as regards different subjects but as regards the same subject at different times. Therefore, one year after the smell-experiments and one month after the color-experiments were begun, all restrictions upon the subjects' *mental* operations were abolished, and they were allowed to memorise in any fashion which the external experimental conditions permitted.

Other consequences of slow presentation will be discussed when the experimental results are presented. One is the possibility that during the intervals the subjects were able to make a mental review of foregoing sequences. Another is the relatively accurate apprehension of the material. A third is emotional composure on the part of the subject during the presentations, composure coupled, perhaps, with a rather low grade of attention. It is not profitable to discuss these circumstances apart from the details of method and from the experimental and introspective data.

For the sake of brevity, the foregoing suppositions will be called the explanations (I) *by presence of the links*, (II) *by spatial localization*, (III) *by multiplied tests of mastery*, (IV) *by leisurely recall* and (V) *by long intervals*. The fifth explanation is twofold, including both the explanation by slow presen-

¹ See p. 130.

tation and the explanation by long intervals between presentations, that is, between the end of one presentation of the series and the beginning of the next. In the spring of 1904-1905, we began to test these suppositions experimentally, and have made tests of different sorts at intervals until the Christmas holidays of 1908. As the results reported in Chapter VI will show, all five suppositions contain a certain amount of truth,—the fourth and fifth, most, and the first, least.

Before the reader enters upon the description of the materials and method used in the reconstruction experiments, it should be stated clearly—it cannot be stated too clearly or emphatically—that we have to describe only a large mass of rough experiments—some of them shockingly rough. The cause of this roughness was twofold. In the first place, the smell-experiments were the nucleus of the whole investigation, and most unfortunately all work with smells is at present crude. The experiments with colors and nonsense-syllables were made primarily for the sake of comparison with the smell-experiments, and in them, greater precision of method, though possible, seemed out of place. In the second place, the *personnel* of the experiments was large, varied, and in part unpracticed. The conditions of the experiments differ *toto coelo* from the conditions of the precise experiments made by Professor G. E. Müller and his co-workers. Nevertheless, the writer believes that the results have a certain value upon account of their large mass and even just upon account of the varying conditions under which they were obtained. It is true that a chain, however long, is 'no stronger than the weakest of its links,' but it is also true that very stout rope may be composed of fragile fibers of hemp *which run parallel to one another*.

To this chapter is appended for the reader's convenience a tabular view of the experiments:¹

¹The advanced-student experimenters not mentioned in the preface were Misses M. G. Alexander, M. Clark, F. V. Cook, E. B. Doak, E. Foster, C. B. Green, J. A. Hewitt, M. C. Kasson, A. J. Klingenhagen, L. Loos, A. G. Lyon, A. E. Moore, E. D. Newton, J. Risdon, E. C. Taylor, J. P. Wilson, and L. Wilson.

GROUP I. 1901-1902 and 1902-1903.

Initial object: To study the development of smell-imagery.

Method: Reconstruction.

Outcome: Data as to the effect of practice in memorising the serial order of smells.

GROUP II. 1903-1904.

Object: To study the effect of series-length in memorising the serial order of smells.

Method: Reconstruction.

GROUP III. 1902-1903 and 1903-1904.

Object: To study the effect of practice and of series-length in memorising the serial order of colors.

Method: Reconstruction.

GROUP IV. 1904-1905 and 1905-1906.

Object: To study the relative difficulty of memorising the serial order of smells, colors, and nonsense-syllables.

Method: Reconstruction.

GROUP V. 1903-1904, 1904-1905, 1905-1906, and 1908-1909.

Object: To check the memorising-methods of the subjects—chiefly, of G.—in the reconstruction-experiments.

Methods and Materials: Miscellaneous.

GROUP VI. 1902-1903.

Object: To study the effect of distraction in memorising the serial order of smells.

Method: Reconstruction.

Value: Bearing upon the explanation by long intervals.

GROUP VII. 1907-1908.

Object: To test the explanation by presence of the links.

Methods: Reconstruction and a modified form of the method of retained members.

Material: Nonsense-syllables, normal series.

GROUP VIII. 1907-1908.

Object: To test the explanation by spatial localisation.

Methods: Reconstruction and a modified form of the method of right associates.

Materials: Smells and nonsense-syllables, normal series.

GROUP IX. Spring of 1907.

Object: To test the explanations by spatial localisation, by multiplied tests, and by presence of the links.

Methods: Reconstruction and a modified form of the prompting-method.

Material: Nonsense-syllables, German normal series.

GROUP X. 1907-1908.

Object: To test the explanation by multiplied tests.

Method: Modified form of the method of retained members.

Material: Nonsense-syllables, English normal series.

GROUP XI. 1908-1909.

Object: To test the explanations by multiplied tests of mastery and by long intervals between presentations.

Materials: Colors.

GROUP XII. 1905-1906 and 1907-1908.

Object: To test the explanations by multiplied tests, by slow presentation, and by leisurely recall.

Method: Complete memorising with and without prompting.

Material in Subgroups 1 and 2: Nonsense-syllables, roughly constructed series.

Material in Subgroups 3 and 4: Nonsense-syllables, normal series.

CHAPTER II.

MATERIALS.

A. Description of Materials.

I. Scents.¹

The list of scents actually in use in these experiments at any one time numbered at its longest 129. In the first year of the reconstruction-method experiments, we had 102 scents in use; in the next three years, 129; in the last two years of the experiments we had 135 available scents, but have confined ourselves to a range of about 83, a range which includes no very weak smells, no approximate duplicates, and no solids. Of the 129 smells on the longest working-list, 45 were essential oils, such as cassia, myrtle, rose, parsley, and garlic; 25 were artificial perfumes, such as 'lilac' and 'honeysuckle,' of formulæ unknown to the writer; 7 were flavoring extracts, such as pineapple and celery; 36 were common articles of the drug-trade and grocery-trade, such as tincture of rhubarb and chloroform, molasses and peanut butter; 10 were rarer chemical-laboratory products of known formulæ, such as lactic acid and pyridin; and the remaining 6 were civet, ambergris, and natural musk (in tincture), sour milk, water from decaying flowers, and alcohol which had been used for preserving potato-beetles.² According to Zwaardemaker's classification, 8 of the 129 smells were ethereal, 57 aromatic, 32 fragrant, 5 ambrosiac,

¹ The word scent is used throughout as a synonym for odorous material or smell-stimulus.

² A complete list of smells will be furnished to anyone who may wish to repeat the experiments. The artificial perfumes were nearly all either Palmer's, or 'De Laire specialties' (trade name of a large variety of concentrated artificial perfumes) obtained from the firm of Dodge and Olcott, 86-88 Williams Street New York. Most of the flavoring extracts were obtained from Joseph Burnett and Company of Boston. All the essential oils and all the rarer chemicals not included among the perfumes were obtained from the Theodore Metcalf Company of Boston.

11 alliaceous, 8 empyreumatic, 5 hircine, 2 virulent, and 1 nauseating.¹

The scents were manipulated as follows: All were introduced into bottles which bear the trade-name of 'half-ounce squat Caswell's.' These bottles are square and therefore not easily upset, have rounded corners, and weigh about 50 grams each. The bases measure about one inch square. The stoppers are of glass and have a disk-shaped top which is easy to grasp. All liquid scents and all solids likely to rattle were dropped upon absorbent cotton which had *already been put* into the bottles.² Upon the bottles were pasted small labels bearing the numbers of the smells according to an alphabetical list or key. For experiments in which the subjects were not to be blindfolded, the cotton was colored, with Diamond dye, as dark a blue as possible in order that differences in the color of the liquids might not appear. In these experiments only fluid scents were used, and the labels were pasted on the bottom of the bottles. For the sake of brevity all records were kept in terms of the bottle-numbers.

We used as few solutions as possible because we wished to have for our series as many relatively discrete smells as possible. To use many alcoholic or paraffin solutions in such series is comparable to throwing a faint reddish or greenish light upon many of the paper-squares in a color-series. We considered the

¹ The fullest statement of Zwaardemaker's classification is found in his *Physiologie des Geruches*, Leipzig, 1895, pp. 233-235. It will be remembered that the framework of this classification is, in the main, that of Linnæus.

Anyone who attempts to 'bottle' as many scents as possible must be impressed with the very great qualitative differentiation of the agreeable smells and the very limited variety of disagreeable ones. Excellent additions to the list of smells used in these experiments are cadaverin (pentamethylene diamine) and scatol (β -methyl indole). The first is 'hircine'; the second, 'nauseating.' The word *nauseating* is, of course, used in a technical sense. Pyridin, which, with one exception, is the most disagreeable smell in the whole collection, belongs technically in the virulent class. (Zwaardemaker has retracted its assignment to the empyreumatic class.)

² When a multitude of scents is much handled it is necessary to avoid cork stoppers, which are both brittle and absorbent, and to avoid the presence of free liquid in the bottles. At best, such experimental material requires constant examination and care which it would be tedious to detail here. It is, of course, entirely unsuited to quantitative work.

highest possible degree of discreteness among the smells as more desirable than such rough uniformity of intensity as we might have secured by solution and dilution. The intensity of a smell in any particular series depends not merely upon the concentration of the scent, but also, among other circumstances, both upon the quality and the fatigue-value of the scent which immediately precedes it. Therefore, within certain limits inequalities of intensity are unavoidable.¹

Without the use of a solvent, the intensity and the pungency of very volatile scents can be reduced by the simple expedient of leaving the cork out of the experiment-bottle for a short time. We used this method with pyridin and bleaching-powder. Solutions, however, cannot be altogether avoided. Very pungent oils, such as garlic, can be used satisfactorily in solution only. Certain substances, such as coumarin, must be diluted because they have the paradoxical property of giving a weak odor at a high concentration. Others, such as the scents of some of the flavoring extracts, can be conveniently obtained only in the form of alcoholic tinctures. But fortunately the odor of pure alcohol in a tincture is almost negligible in our method of presenting the scents, since the alcohol evaporates from the cotton very quickly and leaves behind a large quantity of the scent which has been dissolved in it.

II. Colors.

The color-stimuli used in the experiments consisted of two-inch squares cut from the Milton Bradley kindergarten-papers.²

¹Moreover, the intensity of the smells could not be made uniform by 'levelling up,' because the intensity of the weak smells could not be increased, nor by 'levelling down,' because if uniformity had been secured by weakening the stronger smells, many of the odors would have been too hard to distinguish from one another.

²These papers are manufactured by the Milton Bradley Company in Springfield, Mass., U. S. A. All the papers represented in the sample book were used by us with the exception of the engine-colored papers. In spite of the defect to be noted in the text, the Milton Bradley papers are extremely useful in elementary color-work on account of their variety of color, dead-finish, toughness, and cheapness. For a full description of the papers, see Milton Bradley's *Elementary Color*, published by the Milton Bradley Company.

There were 136 different colors and 'neutral colors' in use. In detail, these colors were as follows:

- a. Ninety colors of the 'pure-spectrum scales,' comprising:
 1. A saturated spectral red, orange, yellow, green, blue, and violet, designed to imitate the 'true' or 'pure' sun-spectrum colors as nearly as this imitation can be secured by pigments spread upon paper.
 2. Two saturated colors intermediate between each two links in this series and between violet and red.
 3. Two tints and two shades of each of these eighteen spectral colors.
- b. Thirty-six colors of the 'broken-spectrum scales' (grayish).
- c. Six colored grays.
- d. Four neutral colors, namely, white, black, and a lighter and a darker neutral gray.

The tints and shades are designed to be proportional in brightness to the spectral colors to which they correspond. Thus, for example, the paler tint of the pure yellow is a very pale gold and the darker shade of pure yellow almost as bright as the spectral red, whereas the paler tint of the pure red is so deep a pink as scarcely to merit the name. (The set includes no good pale rose-pink.)

The Milton Bradley color scales have the defect that the series of hues is more finely graduated over exactly that portion of the scale of hues where our color discrimination is poorer. For instance, in the pure-spectral scales we have, in passing from blue to yellow through red, eleven intermediate steps, whereas in passing from yellow back to blue through green, we have only five. As a result of this disproportion, we have a mass of browns, pinks, plum-colors, and lilacs which are, of course, easy enough to discriminate when seen side by side, but which are sometimes very hard to identify correctly when seen one by one.

In the experimental records, the colors were denoted by a convenient set of symbols suggested by the manufacturers.

III. Nonsense-Syllables.

The series of nonsense-syllables used in the experiments were of three sorts, which the writer will call for brevity (*a*) the Ger-

man normal series, (b) the English normal series, and (c) the hit-or-miss series. The German normal series were made for German subjects and pronounced by German rules; the English normal series were made for American subjects and pronounced after the analogy of English words.

a. The German normal series it is not necessary to describe. They were made in Prof. G. E. Müller's laboratory in Göttingen in accordance with the rules given in such great detail by Professors Müller and Schumann.¹ The adjective *normal* is borrowed from them.

b. The English normal series were made in as exact imitation of the German normal series as the peculiarities of the English language make advisable. The series, like the normal series of Müller and Schumann, were of the lengths twelve and eighteen. In English there are twenty-one consonants and digraphs available as initials, viz: *b, d, f, g, h, j, l, m, n, p, r, s, t, v, w, y, z, ch, sh, and th*. In German there are but sixteen (or seventeen, including *sch* which was finally excluded by Müller and Schumann). In English, there are nineteen available as final consonants, viz: *b, d, f, g, j, k, l, m, n, p, r, s, t, v, x, z, ch, sh, and th*. In German, there are but twelve. Thus, in English a normal series of eighteen syllables can be made without any duplication of either initial or final consonant sound, whereas in a German series of eighteen, two of the initial and six of the final consonants which occur in the first twelve syllables must be repeated in the last six.

But although in the matter of consonants the advantage is with the English, in the matter of vowels it is with the German. Each German vowel and diphthong has a single proper sound (if one disregards certain very slight quantitative differences); in English most vowels and diphthongs have several. If, however, syllables cannot be translated readily from visual into auditory and from auditory into visual terms, the most undesirable complications are likely to arise in experiments upon subjects of different image-types. We aimed, therefore,

¹ Müller and Schumann, *op. cit.*, pp. 99-106. The same series were used by the writer with Professor Müller as subject in a group of experiments which does not belong among these studies.

at excluding both from the normal and from the hit-or-miss series, all syllables over whose pronunciation, or over whose spelling if presented to the ear, an educated person would hesitate. We were also unwilling to use diacritical marks. At once to avoid the use of diacritical marks, and to secure a sufficient range of vowel sounds, we adopted, in both classes of English series, the expedient of indicating the long sounds of the vowels by affixing to the final consonant the silent *e*. No educated person to whom English is the mother-tongue can hesitate long as to the difference in pronunciation of *bipe* and *bip*, *gobe* and *gob*. To correspond with the twelve vowels and diphthongs of the German normal series (which are *a*, *aa*, *ä*, *e*, *i*, *o*, *ö*, *u*, *ü*, *au*, *ei*, and *eu*), we selected for our own normal series the long and the short English sounds of *a*, *e*, *i*, *o*, *u*, and the diphthongs *ou* (to be pronounced as in *foul*) and *oi* (to be pronounced as in *boil*). A specimen-series of eighteen members runs as follows: *jil*, *nume*, *bike*, *thene*, *gour*, *pev*, *foig*, *chuch*, *rap*, *shade*, *yoje*, *sosh*, *vif*, *tabe*, *hoiz*, *wat*, *keth*, *dux*.

In these series every combination of letters is strictly to be given its normal English sound. For example, *ch* is not to be pronounced either in French or in German fashion; the syllable *chuch* has the initial and final consonant sounds of *church* and rhymes with *Dutch*. A *t* may well be inserted before the final *ch* if the series are to be presented to the eye of untrained subjects. The long *a* is pronounced as in *babe*; the short *a* as in *cat*. The Italian *a* was excluded, because it can be indicated only by a diacritical mark and because it approximates with confusing closeness to the short sound of *o*.

We imitated the method of Müller and Schumann in providing three sets of small cards,¹ bearing severally the three sets of syllable-elements (initial consonants, vowels and diphthongs, and final consonants), in making the syllables by drawing the elements separately after shuffling, and in entering every syllable thus formed in a chart, so that we might not inadvertently use the same syllable again within a given length of time.² In

¹ It saves trouble to have the sets of cards of three different colors since individual cards are likely to slip into the wrong boxes or packs.

² Müller and Schumann, *op. cit.*, pp. 101-104.

making the series we observed two different sets of rules, namely, (1) the general rules of Müller and Schumann, and (2) certain rules necessitated by the peculiarities of the English language.

1. The Müller and Schumann rules are these:¹

(a.) *In a series of twelve no initial consonant, no vowel or diphthong, and no final consonant may be repeated.* The carrying out of this rule is insured mechanically by not returning any card to its pack till the series is complete. The same rule applies to the last six syllables of a series of eighteen taken by themselves, but in these syllables six of the vowels and diphthongs of the first twelve syllables must be repeated. The vowels of the twelfth and thirteenth syllables may not be the same.

(b.) *No syllable may begin with the consonant with which the last syllable ended.*

(c.) *No syllable may end with the consonant with which the last syllable began, if the two syllables in question belong to the same foot.* We extended this rule by omitting the conditional clause. The Müller and Schumann series were read either in trochaic or iambic rhythm. Our series were not always read rhythmically.

(d.) *No two successive syllables may together make a word or phrase.* For example, in the German the sequences *weib—lich* and *gib—mir*, and in the English the sequences *par—don* and *yes—mam* would be ruled out. It is a mistake to suppose that all German words were ruled out from the Ebbinghaus series and from the Müller normal series; *they were not*. Because they were not, we refrained from excluding English words from our normal series, except in the cases covered by the rule just given. To include words in English series may be a mistake. The language is incomparably richer than the German in monosyllables. In a set of 30 German normal series of 18 members each, with no syllable duplicated, the writer can find only 15 German words (2.8 per cent of 540), including dialect words and all verb-forms. In a similar set of English series, there are 86 combinations of letters which spell English words or common slang-words properly, and 59 more which spell them phoneti-

¹*Op. cit.*, p. 106 (Summary).

cally, 145 in all (26.8 per cent). The including of so many words is, of course, a circumstance which tends to make the English series easier than the German. On the other hand, no devices seem to make the reading and spelling of English non-sense-syllables a simple matter for all American college students. Some of them will call *tut toot* to the end of the chapter.¹

2. The rules necessitated by the peculiarities of the English language are these :

(a.) *e* and *i* may not be used after the initial *g*, since they normally give it the sound of *j* (as in *gem* and *gin*).

(b.) The silent *e* may not be added to the final *g* or *s*,² since it normally gives them the sounds of *j* and *z* respectively (as in *oblige* and *surprise*).

(c.) *R* may not be used after short *e*, *i*, or *o*, since *er* is in sound indistinguishable from *ur*, *ir* from *ere*, and *or* from *ore*.

(d.) The silent *e* may not be affixed to any digraph or to *x*, since the resulting syllables look so uncouth as especially to annoy the subjects.

(e.) No syllable may stand which has the spelling of an English word not pronounced phonetically (for instance, *dove*).

c. The hit-or-miss series differed from our English normal series in the following respects:

1. The syllables were not constructed by drawing the syllable-elements separately, but by the systematic combination of the consonants and digraphs listed above, with the addition of *qu*, and the ten long and short vowels. The vowel, of course, stood between the consonants. No diphthongs were used.³

2. The series were formed by drawing small cards, on which the syllables were written, out of a large box which contained the whole stock. The order in which they came out determined

¹ The modern method of teaching children in the American schools to read, the so-called 'word-method,' is probably responsible for the scanty knowledge of English phonetics in the rising generation.

² After the long vowels and before the silent *e*, we now replace *s* by *c* but we did not do so in the series used in these experiments.

³ The hit-or-miss series were used in the earlier and rougher experiments. When we made the normal series used in these experiments, we abandoned the use of *qu* but are now using it again. *Wb* sounds too much like *w* to be a safe initial.

the order in which they stood in the series. Rhymes, alliterations, and combinations which constituted words were not ruled out.

3. The same syllable might appear on successive days. Since, however, all the series for each experimental sitting were ordinarily drawn out at one time, the same syllable almost never appeared twice on the same day.

4. English words, and common slang words, such as *yep* or *bike*, and nick-names, such as *Tom*, were thrown out in making the syllables. So were all combinations of letters which spelled words phonetically. Foreign words, however familiar, were not thrown out. The following series of syllables, in which those thrown out are italicised, illustrates our principles of formation and rejection: bab, *babe*, beb, bebe, *bib*, bibe, *bob*, bobe, *bub*, bube, *bad*, *bade*, *bed*, *bede*, *bid*, *bide*, bod, bode, *bud*, buide.

For our hit-or-miss series, we had a stock of about 1400 syllables. In the German normal series 2304 different syllables are possible. Müller and Schumann, however, excluded from the list 94 syllables which were especially hard to pronounce. In our English normal series, 4033 different syllables can be made out of the elements enumerated above under the first four rules peculiar to these series. Some of these syllables, however, are thrown out under the fifth rule (*e*).

For the experiments by the reconstruction method, the syllables were written or printed by hand, as legibly and uniformly as possible, with black ink lengthwise upon white cards measuring 1 x 2½ inches.

B. Method of Series-Formation in the Case of Smells and of Colors.

This section contains (I) a statement of the degree to which chance ruled in the formation of the smell- and color-series, and (II) an explanation of the fact that we did not attempt, by any more elaborate set of rules, to make up series of smells and colors which, apart from length, should be uniform in difficulty. The method by which the series of nonsense-

syllables were formed has already been fully described. It will be well, however, to add here a few words of excuse for using the roughly constructed hit-or-miss series at all. These series were used only in the experiments which had to do with the comparison of smells, colors, and nonsense-syllables, and in certain other very rough experiments. For reasons soon to be explained, the smell- and color-series were themselves of necessity constructed roughly. In extremely rough experiments, it seems hardly worth while to use series in whose making a very considerable amount of labor is involved.¹

I. In explaining the degree to which chance determined the membership of the several smell- and color-series, it will be well to state separately (a) the mechanical methods by which the series-members were drawn and (b) the rules which were made slightly to limit the operation of chance.

a. 1. The color-squares were drawn, after shuffling, like cards out of a pack. The order in which they were drawn determined their order in the series. Each experimenter had several duplicate sets of color-squares expressly in order that several series, all drawn by chance out of the same total number of colors, might be prepared beforehand for the same experimental sitting. Thus, any color in any given series was as likely as any other color to appear in the very next series. (This statement does not apply to all the small groups of control-experiments.)

2. In most of the smell-experiments, the bottles were taken up in fairly random fashion, as they stood in the order of their numbers upon two low shelves. Thus, they were, so to speak, drawn without being shuffled. In the experiments of Group

¹ The writer cannot in an hour, even after considerable practice, make in either German or English, more than five or six normal series of twelve members each and enter each syllable both in the series-book and in the chart. In some of our experiments the series contained 41 and 81 members. Such series can be made relatively uniform by putting several normal series of twelve together, end to end, and if the delicacy of the experiment requires it, these long series can be still farther improved by applying to the component series the Müller and Schumann rules for 'verschärfter normal' (supernormal) series. (*Op. cit.* p. 104.) This last measure, if applied to a series of 81 members, would involve a labor of two or three hours.

V, the bottles were mixed together and drawn as they stood in a mass on the table. It would obviously have secured a more random selection if they had always been drawn in this way.¹ It should be noted, however, that only two out of the many experimenters had the key to the bottle-numbers. This key was purposely withheld by the director in order to minimise the deliberate avoidance and selection of particular smells. The order in which the bottles were drawn did *not* determine the series-order; on the contrary, the bottles selected usually sustained some extra shuffling. The bottles were *not* returned to the shelves until the end of the experimental sitting. Thus, whereas any color in any series was as likely as any other out of the 136 to appear in the very next series, no smell appeared, unless by accident, twice at the same sitting, although any smell might appear on successive days. The reason for this inconsistency of procedure will be stated in the next sub-section (B, II).

b. The operation of chance was limited as follows:

1. In the earlier color-experiments, those of the first year, no color might stand in the same series 'with any of its next neighbors in the color-pyramid.'² Thus the pure-spectral 'true' red might not be combined with its own darker tint or lighter shade, or with the pure spectral orange-red or violet-red, or with the broken-spectral 'true' red. At the end of the first year this rule was given up because it restricted too narrowly both the length and the differentiation of the series. Afterwards, the color-series, like the hit-or-miss series of nonsense-syllables, were drawn altogether at random.

2. In the smell experiments, (1) two extremely similar odors, such as cassia and cinnamon, or eucalyptus and rosemary,

¹ The fact that the bottles were for the most part drawn as they stood in rows in a given order was simply a matter of laboratory exigency. The scents required constant care and frequent renewal, and were also often in use for other purposes than the memory-experiments. Therefore, every person who had used the bottles was required to rearrange them upon the shelves in the order of their numbers so that any given bottle might be located at a moment's notice.

² The color-pyramid of Titchener. For this pyramid in its improved form, see Ebbinghaus, *Grundzüge der Psychologie*, 1905, p.199, and of Titchener, *Experimental Psychology, Students' Manual, Qualitative*, p. 3.

and (2) two extremely weak scents, such as *nux vomica* and our solution of heliotropin, might not be put into the same series. The lists of smells and of pairs of smells which came under these rules were, like the smell-list itself, worked out somewhat gradually. They were not fully in force in the experiments of Group I. To assist the experimenters in carrying out the rules, bits of red and of green paper were pasted to the bottles. Green labels were signals that the smells were weak; red labels bore the numbers of the bottles which might not be introduced into the same series with the ones thus marked. Of course, this device was not used when the subject was not to be blindfolded. In this case, very weak smells and approximate duplicates were excluded from the material.

II. The last sub-section has dealt mainly with the circumstances which interfere with the perfect working of chance in the formation of the smell- and the color-series. It is now necessary to explain why the formation of these series was left to chance so largely as it was. That there must have been considerable variation in difficulty from series to series of the same length is obvious. This variation was especially great in the case of the shorter series. The larger the fraction of the total number of smells and of colors which was included in every series of any given length, the more nearly equal in the long run the difficulty of the series in this group must be. The longest possible series would be identical as regards their members, and would differ only in the order in which these numbers might be arranged.

The formation of the smell- and color-series was left to chance for the following reasons:

a. With so limited a range of material, series of approximately equal difficulty cannot be sufficiently differentiated. The scheme of formation rapidly becomes obvious to the subject who anticipates members of such and such groups and compares their position from series to series. He reflects, for example, 'The bright green I had last time stood in such a place, and the pink in such another and the gray in such another; this green stands here and this pink there,' and so on. The shorter the series, the more obvious the scheme of formation. The

longer the series, the more nearly equal the difficulty even if left to chance.

b. In the case of the smells, the construction of series which are more than roughly equal in difficulty is impossible. The difficulty of memorising two series of smells depends upon at least three factors exclusive of length, of the differentiation of the odor-qualities and of the order in which the smells stand in the series. These factors vary independently, both of one another and of the differentiation of the smells, so that a selection of smells which satisfies a rule based upon one sort of difference may break a number of other rules based upon other sorts of difference. Moreover, all three factors depend upon such variable conditions that an exact control of them is impossible. These factors will be enumerated here but not discussed. They are (1) the ease with which the several smells are identified or apprehended (their *Geläufigkeit*); (2) their strikingness, that is, the degree to which they catch the subject's attention (their *Eindringlichkeit*); and (3) their richness in earlier associations which in the process of reproduction may act either in the way of facilitation or of inhibition. The same three variants appear in the case of the colors and even in that of the nonsense-syllables, but in a much less marked fashion.

c. The reconstruction method is itself so rough that great refinement in series construction is a misapplication of pains. It seems distinctly preferable to use a larger number of roughly constructed series than to use a smaller number of laboriously constructed series.

d. A considerable number of the series employed were very long. Many exceeded thirty in length, and some consisted of 61 and some of 81 members. The longer series are, the greater is the labor necessary to making them even roughly equal in difficulty by any system of rules.

Since in most of the experimental groups we used large numbers of series, we have reasonable ground for hoping that differences in difficulty between the series in those groups balance and cancel by the ordinary law of chance. Large mean variations in the results are, of course, to be expected. The ques-

tion as to which of the three sorts of series is most homogeneous is reserved for Chapter V.

In this chapter, two points only remain to be explained. The first is the fact that we left the intervals between the appearance of the same unit in the different series so largely to chance, merely providing that in the case of the smells, the same unit should not appear twice on the same day. The second is the fact that we did make this last provision in the case of the smells and did not make it in the case of the colors.

The first fact is to be explained as follows: Since there were never more than 136 colors and 129 smells in use, the same unit had to do duty over and over again. But when the same unit is employed repeatedly its whole fringe of accidental associations wears off. Smells and colors soon become like numerals and letters as material to be memorised. It is very easy to learn for the moment a series of numerals, but it is not easy to remember the position of the same numeral in different series. Each new series seems to sweep away the last. Let the reader, in order to realise how his myriads of accidental associations with the numerals inhibit one another, try to recall when and where he *last* saw the figure 9. In our first experiments with smells and colors, it seemed to make little appreciable difference whether the same smell or color occurred on two successive days or not. Therefore, it did not seem worth while to burden ourselves with any elaborate scheme of rotation.¹ The writer does not mean to say that the associations which the individual smells and colors acquired, in the course of the experiments, did not tend to inhibit the formation of new associations, but only that these tendencies are about the same for any smell which the experimenter can select on any given day, whether it has appeared on the day before or not.

The case of the nonsense-syllables is very different from that of the smells and colors. Even for the hit-or-miss series, there

¹ As a matter of fact, the director could never have secured the correct carrying out of such a scheme by all of the student-experimenters and believes that a 'compensating program' half carried out is much less desirable than the free operation of chance. Moreover, the director, who would have had to teach the experimenters how to work out such a program, was herself subject-in-chief!

were more than ten times as many syllables available as there were smells and colors. Since, however, we had no rule to prevent the appearance of the same smell or color on successive days, it seemed better to us in making the series of syllables to be compared with the series of smells and colors to permit syllables to recur on the second day as often as chance would allow them to do so. In this decision we were probably wrong. Just because a given syllable occurs so seldom upon two successive days, the subject is likely to attend especially to any syllable which does thus recur.

The explanation of the second fact, that is, our inconsistency in the case of the smells and colors, is this: In everything which had to do with the formation of these series we proceeded upon the theory that if chance were to operate at all, the more freely it was left to operate the better. Yet on account of the always imminent danger that the subject's sense-organ would be exhausted, it did not seem best to allow the smell-series given at any one sitting to be even in part the same. Furthermore, smells and colors differ in the amount of their individuality. The subject is likely to notice the repetition of any strong, familiar, or unpleasant smell and to compare its place in the two series. On the other hand, the repetition of most of the colors remains almost, if not quite, unnoticed. Smells have an individuality of their own, whereas many of the colors are identified simply as members of scales. Of course, this is by no means true of all of them. White, black, and some of the saturated pure-spectrum colors—notably, red-orange—make landmarks in every series in which they appear. The statement is valid, however, for the two kinds of material at large.

CHAPTER III.

METHOD.

This chapter does not contain a complete account of all the methods used in this investigation. Its purpose is simply, on the one hand, to describe our normal procedure by the reconstruction method, and on the other, to point out the *prima facie* resemblances and differences between this method and the other methods thus far elaborated for the study of memory. The sixth chapter will give an account of the experimental study to which we have submitted the reconstruction method; this third chapter contains merely a preliminary survey of the method in its relations to others.

A. Details of the Experimental Procedure by the Reconstruction Method.

A cursory outline has already been given (page 3) of the method used in the smell-experiments. As stated (page 6), the method employed in the color-experiments, and in the reconstruction-experiments with syllables was exactly analogous. It is not necessary to repeat in detail the description given. This section contains a more detailed consideration of the following points: (I) the manner in which the series were presented for memorising; (II) the manner in which the units were presented for the reconstruction of the series; (III) the requirements which the subjects were obliged to meet in order to reconstruct the series perfectly in the technical sense; and (IV) the length and regularity of the various time-intervals.

I. As regards the manner of presentation, it should be especially noted (*a*) that the series were presented without the use of apparatus, and (*b*) that in the smell-experiments the subjects were (normally) blindfolded.

a. No apparatus *could be* used in the presentation of the smell-series; therefore, no apparatus *was* used in the presenta-

tion of the colors and syllables. Minor details which should be mentioned are as follows:

1. In the presentation of the smell-series, the experimenter handed the bottles to the subject who was allowed herself to take out and put back the stoppers. In presenting the colors and the syllables, the experimenter laid the paper-squares and cards in the center of a small table. With the principal subject, to whom spatial associations became highly important, it was necessary always to use the same table.

This table was 24 inches square. The other tables in use in the color- and syllable-experiments measured 19 x 29 inches. The shorter diameter lay between subject and experimenter. All the tables were covered with black cloth. The size of the tables is relevant to the rate at which the series were given; the uniformity of surface eliminated all avoidable spatial associations (orientation by the graining of the wood and the like).

2. In order that no shadows might fall upon the colors, the experiments with them were made under certain skylights which are available in our laboratory.

3. The subjects were neither required nor forbidden to name aloud the smells and colors or (normally) to read aloud the syllables.

b. The blindfolding in the smell experiments was done with a soft light-weight bandage of black silk or cheese-cloth. The blindfolding was practiced in order to make sure that the smell-series were really and purely smell-series, or, in other words, that no visible peculiarities might help to differentiate the bottles. When the work was begun, no very feasible method of making the bottles uniform in appearance occurred to the experimenters. Moreover, it was thought important that the sensations aroused peripherally by the series should be purely olfactory and tactile, for it is at least possible that the occurrence of smell-images might be facilitated by the elimination of visual percepts and the extra attention concentrated upon the smells by this narrowing of attention. After the interest in smell-imagery lapsed, the blindfolding was continued from one set of experimenters to another, in order that the conditions in this important particular might be the same. The degree of difficulty which the blind-

folding introduced into the process of rearrangement will be discussed later upon the basis of control-experiments.

II. In respect to the manner in which the units were presented to the subjects for the reconstruction of the series, the important points are two: namely, (*a*) that the subjects received the links in chance order, and (*b*) that they were compelled to deal with the series link by link and were not allowed to do so *en masse*.

a. In presenting the colors and syllables, the experimenter shuffled the paper-squares and cards, drew the units one by one and laid them down on the table for the subject to take up and place. The scent bottles were mixed as they stood on the table and the blindfolded subject was then—ordinarily—herself allowed to take them up at random. Thus, in the case of the smells, all danger was avoided of caprice in selection on the part of the experimenter. The order in which the subject received or took up the units was one of the minor factors in determining the difficulty of any given attempt at reconstruction. The more nearly the units came to hand in their original order, the less likely the subject was to forget this order. A point of less importance is the fact that in all three sorts of series the first unit was presented (with the word 'first') to the subject, so that it might act as an initial suggestion or 'starter.' This practice was imitated from the original procedure of Ebbinghaus.¹

b. The meaning of the statement that the subjects were compelled to deal with the series link by link is this: The syllable-cards and the color-squares were not handed over to them in a sheaf or pack so that they might spread the series out before them and look back and forth from one unit to another before any of the units were placed. Neither were they allowed to smell the same scent-bottles repeatedly during any one attempt at rearrangement. Reconstruction under such circumstances would be to a very considerable extent a matter of inference. The difference between pure memory and partial inference may

¹ *Ueber das Gedächtnis*, p. 31, phrase, *nach gegebenem Aufgangsglied*.

be illustrated thus: A subject may argue that 'lek must come after pux because pux was followed by some syllable with a short e and all the other syllables with a short e seem to belong somewhere else.' In this case he does not really remember the sequence pux—lek but only the sequence pux—ě. It is, of course, impossible wholly to eliminate such inferences from the subject's mental processes in reconstruction. Indeed, inferences of the same sort play no small part in reproduction by the most delicate of all the memory-methods—that of right associates.¹ In the reconstruction of series, as the process nears completion, inference becomes more valuable and memory becomes less essential until finally the subject puts the last unit left over into the last vacant place with a satisfied feeling that it 'must belong there.' The value of inference is, however, minimised when the subject receives the links one by one, and is compelled to indicate once for all his best judgment of the position of each with reference to those already placed.

At this point, we encounter an important difference between the three sorts of material. In the case of the colors and syllables, the subjects could see the units which they had already placed; in the case of the smells, they were forced to remember the placing which they had already done. Therefore, in the case of the colors and syllables, more could be done by inference. Furthermore, 'reproductive reinforcement' may have come into play in a much more marked fashion. Reproductive reinforcement is the opposite of reproductive inhibition; it is the working together of two or more associations in reviving some idea which no one of them, taken singly, might be strong enough to reproduce.² It may be illustrated thus: A subject may take up a particular brown and be unable to remember anything in regard to its place or neighbors until the sight of a dark green already placed suggests that this brown came after a light green. Thus the brown and the dark green together

¹ Cf. Müller and Pilzecker, *Experimentelle Beiträge zur Lehre vom Gedächtniss*, Zeitschrift f. Psych. und Phys. der Sinnesorgane, Ergänzungsband, i. pp. 16-17.

² Cf. Ebbinghaus, *Grundzüge der Psychologie*, Bd. i, 2te Auflage, pp. 698-700 on 'assoziative Förderung.' Cf. also Müller and Pilzecker, *op. cit.*, pp. 82-83.

suggest the light green. Of course, reproductive reinforcement can occur in the smell-series also, but there can be no simultaneous perception of different smells. One of the suggesting smell-links must always be a mere memory-image.

III. It is necessary to explain in even tedious detail exactly what is meant by the 'perfect' reconstruction of a series. In the method of complete memorising (the original method of Ebbinghaus, later elaborated by Müller and Schumann), a series is not considered as mastered until it can be repeated (*a*) without mistakes, (*b*) without hesitation, and (*c*) with consciousness of correctness. With our own method, we attempted to apply equivalent rules:

a. A displacement of two units even in the longest series made the reconstruction imperfect. This rule requires no further discussion.

b. Our placing rules were adopted as an equivalent to the rule in regard to hesitation. As may be remembered (see page 3), they were, briefly, that no reconstruction should count as perfect (1) in which the relative positions of any series-members had been corrected by transposition, (2) in which a space had been made between any two members already placed for the insertion of a third, or (3) in which members properly standing next to each other had not been made to touch from the first. These rules are not a perfect equivalent to the hesitation-rule of Ebbinghaus. The principle upon which the hesitation-rule is adopted in the method of complete memorising is doubtless, primarily, that firmly established associations work rapidly. In experiments by the reconstruction method it is impracticable to limit exactly the time given the subject for the placing of the several units. (He has them in his own hand.) The hesitation-rule has, however, a secondary value in insuring that the series is recited from memory without the aid of inference, for even when a series of words or syllables is recited from beginning to end there is opportunity for inference if the subject is allowed unlimited time. Arbitrary as the placing-rules seem, they were adopted after preliminary experiments as the only way of making sure that memory played the major and inference the minor

part in reconstruction. The operation of these rules must be illustrated in detail.

Let us suppose that we are dealing with a series of 26 scent-bottles and let us represent them in order by the letters of the alphabet. The positions which the subject gives to the bottles must indicate from the moment in which he sets them down upon the table positions in the series relative to all the bottles which he has so far placed. The subject knows more or less exactly the length of the series and arranges it from left to right with the middle of the series approximately opposite his own 'median line.' He first receives the bottle *a*, is told that it is the first, and places it to the extreme left of the space required for the series. He then receives *w*, knows that it is one of the last scents, and places it considerably to the right of this space. Next he receives *l*; this he places between *a* and *w*. Next he receives *j*. This he must place between *a* and *l* without letting it touch either. It is not necessary that he should at once put it nearer *l* than *a*. Such a demand would make his task much harder, and would, moreover, put a premium upon absolute space-associations. If, however, he puts the bottle between *l* and *w* or lets it touch *a* or *l*, then the reconstruction cannot be perfect unless he corrects his mistake before he has smelled another scent. (The smelling of another odor might, of course, reveal his mistake.) If he is a practiced subject and knows the series, he will place the *j*-bottle near *l* on the left, rapidly measuring out with his finger the space necessary for *k*. Next comes the bottle *y*; this must be made to touch *w* on the left; if a space is left till the next scent is smelled, the reconstruction cannot be perfect. Thus he proceeds, bottle by bottle. If he has allowed too much space for the series, he must push the short rows together as soon as he has the bottles which belong between them. For instance, *j-k-l* and *n-o-p* may be standing far apart, but as soon as *m* appears he must make a compact row from *j* to *p*. Let us now suppose that twenty-five bottles are placed, and that only one space remains, a space between *r* and *t*. He now takes up his last bottle, and discovers, to his distress, that it is *c* and that he has mistaken *s* for *c* and has already put it in between *b* and *d*. He may and must now put both *c* and *s*

in their proper places, but although this may be the only error of placing which he has made, the transposition makes the reconstruction 'imperfect.'

Our subjects were not required to arrange the series from left to right. Some of them arranged the links from right to left, and at least one subject arranged them in a line perpendicular to her own person. In the case of long series each individual subject was required always to put the same number of units of each kind—scent-bottles, color-squares, or syllable-cards—in one row. Otherwise, the experimenter could not have been sure whether the subject was beginning a new row, or was putting certain units aside for the moment because she could remember nothing about them.

The subjects were allowed temporarily to lay aside a unit during the process of reconstruction. Of course, no reconstruction in which this had been done even once was accounted perfect. Nevertheless the practice was a deviation from the strict link-by-link procedure. It was allowed on the ground that if the subject were compelled to insert a unit at random a source of confusion would be introduced to which many misplacements might be due. It was a mistake, however, to make an exception for such cases of total ignorance. Every error which the subject makes, wittingly or unwittingly, reduces the chance that the rest of the series will be correctly reconstructed. To be sure, if the subject is compelled to place a unit absolutely at random, he does so with great dissatisfaction, and is likely to arrange the rest of the series somewhat recklessly. Still, in a minor degree, every uncertain placing tends to produce this same unfavorable mood. It should be noted, however, that except as a last resort, no subject laid aside a unit unless she had already broken a placing-rule or thought that she had already made some other mistake. 'To do the series all over again' was the penalty of such delay, and unless this penalty had already been incurred, the subject preferred 'to take chances' in placing the unit at once. Scent-bottles set aside might not be re-smelled.

c. No reconstruction was counted perfect unless it was made with some consciousness of correctness, that is, unless the sub-

ject (1) had placed the links, one by one, in what she definitely 'supposed' (conjectured) to be the proper places, and (2) in the end knew of no mistakes which she had made. When a reconstruction was complete, the experimenter asked, 'Is it right?' If the subject said 'Yes,' the experimenter inquired, 'Are you sure or do you only bet on it?' If the subject answered that she 'only betted,' the reconstruction was reckoned as imperfect. If she said that she was 'sure,' the reconstruction was counted as perfect, provided that it was objectively correct. Nevertheless, the word 'sure' covers the most varying degrees of certainty.¹ The observers undoubtedly too often said they were sure when they would have hesitated to 'stake' ten cents on the correctness of the rearrangement. For if the subject merely 'bet,' the series had to be again presented and again reconstructed, and this penalty constituted for the subjects—even for G., the director of the experiments—an insupportable temptation to construe themselves a trifle more certain than they really were. Except in the case of G., almost no cases appear in the records in which the subject had reconstructed the series without breaking the placing-rules and yet only 'bet' that it was right. Cases practically never occur in which the reconstruction was really correct but was nevertheless supposed by the subject to be wrong. We unfortunately omitted to count the many reverse cases in which the subject asserted or 'bet' that a wrong reconstruction was right. Our results, therefore, furnish very few reliable data in regard to the consciousness of certainty. It must, however, be remembered that *the placing-rules practically eliminated the possibility of a perfect reconstruction by guess-work.*

To keep the placing-rules and to be perfectly sure that a completed reconstruction is correct is harder with the colors than with the nonsense-syllables, and harder still with the smells than with the colors. In the case of the syllables, the

¹ On variations in the consciousness of correctness, Cf. Müller and Schumann, *op. cit.*, p. 305; Müller and Pilzecker, *op. cit.*, pp. 9 and 16-18; Ephrussi, *Experimentelle Beiträge zur Lehre vom Gedächtnis*; same Zeitschrift, xxxvii, pp. 90-95.

subject has only to memorise the order of perfectly distinguishable units and to avoid careless mistakes. In the case of the colors and smells, he must not only do this but may have difficulty in identifying some of the units as those belonging in particular places. Moreover, in the case of the smells, he may forget the contents of some of the bottles which he has already placed in the half-formed row. It will be remembered that no subject was allowed during any one attempt at reconstruction, to smell any scent again after she had once placed it and had smelled another bottle. Therefore, the subjects had not only to recall the order of the series, but also constantly to remember the contents of the bottles which they had already placed. The subject of the illustration, with p in his hand, might not be able to remember whether a given bottle in the row before him was n or q . If he set the bottles f and v aside, he would be able afterwards to tell which was which only by the spatial positions which he had arbitrarily given them. The rule under discussion was designed, not only, like the placing-rules, to minimise the tentative, shifting reconstruction of the series, but also to put a premium upon smell-images. It was perpetuated to the end of the smell-work in order that the later experiments might be comparable with the earlier. Breaking this particular rule was considered as a misdemeanor of the same order as turning over the color-squares to look at the symbols on the back. However, it obviously made the conditions of the smell-experiments very different from those of the color- and syllable-experiments, in which the subjects gradually spread the series out before their eyes. This point will be taken up again in Chapter V. It may be noted here in passing that this peculiar difficulty in arranging the smell series has little to do with the fact that the subjects were blindfolded. If the experiment is to be even in the loosest sense an experiment in memorising series of smells, the subject cannot be allowed to *see* any difference in the scent-bottles. Without the eye-bandage, he has still the double task of remembering the series-order and the contents of the bottles which he has already placed.¹

¹ The conditions of the experiments with 'visible series,' on the one hand, and with smells, on the other, might have been made more comparable by either of

IV. In precise experiments in memorising the following time-intervals are of importance: (*a*) The exposure-time of the various units; (*b*) the vacant interval between these exposure-times; (*c*) the interval between the end of one presentation of the series as a whole and the beginning of the next presentation; (*d*) the interval between the last presentation or last of a series of presentations of the series as a whole and the beginning of the reproduction which corresponds to this presentation or series of presentations; (*e*) the time allowed to the subject or required by him for reproduction; (*f*) the interval given for rest between the learnings of different series at the same sitting; and (*g*) the interval between the different experimental sittings. In connection with this list of intervals, two other circumstances may properly be discussed, viz: (*h*) the hour of the day at which the experiments are made, and (*i*) the time-order (*Zeitlage*) of series of different kinds which may be given at the same sitting. In regard to nearly all of these matters, our own experimental conditions, in the reconstruction-work, were irregular, and our records rough.

a and *b*. The exposure-time of the several units in a series and the vacant interval between the exposures together determine the *rate of presentation*. In describing our experiments these two factors must be treated together.

The mere fact that we did not use apparatus in the reconstruction, experiments made the rate of presentation both slow and irregular. Nevertheless, it would be a mistake to suppose that by the practiced experimenters the series were presented with great irregularity. On the contrary, irregularity in the intervals was inappreciable *except as it was created by the subjects themselves* in 'holding on' to a bottle or by remonstrating over the removal of a color which they had not had 'time to see'.

two methods. Either the subject might have been allowed to re-smell bottles whose places had already been definitely assigned, or else the color-squares and syllable-cards might have been covered individually with blank cards as soon as placed. (For certain reasons, covering them would be more convenient than turning them upside down.) The first method we rejected for the reasons stated in the text. The second method did not occur to us when the main body of experiments was made. Its results have been, to some extent, tested by a small set of control-experiments included in Group V.

By the practiced experimenters, the colors and syllables were presented at intervals of $3\frac{1}{2}$ seconds (by some individuals at intervals of 4 seconds) almost as regularly as if the timing had been done with a watch or metronome. As a matter of fact, it was done by the movements of the experimenter's arm. The latter was not allowed both to expose and to remove a color-square or syllable-card with a single outward and inward arm-movement. On the contrary, she was required, when she had laid down a paper or card, to take her hand completely out of the space above the table and to bring it back to take up the paper or card. This movement was almost always made as rapidly as possible, and not only insured to the subject an unobstructed view of the syllable or color but timed its exposure. The exposure lasted between one and two seconds or rather less than one-half of the total interval between the beginnings of successive exposures. The automatic movements of the practiced experimenters were all regulated to *approximately* the same rate in each group of experiments since all these experimenters worked many hours with the principal subject G., who protested at once upon any deviation from the rate to which she was at the time accustomed.

In the smell-series the exposure-time was much more irregular than in the color- and syllable-series, because the subjects took the scent-bottles into their own hands, whereas they were not allowed to touch the color-squares and syllable-cards, and because very different lengths of time are really required to apprehend (*auffassen*) the various units. A rule was made that only 'one good sniff' with each nostril might be taken, but as a matter of fact, a subject was very rarely deprived of a bottle by the experimenter, who could not well take it upon herself to decide the time required for apprehension in the particular case. Indeed, in all three sorts of series we proceeded upon the assumption that uniformity in the satisfactoriness of the subject's apprehension of the units was more important than uniformity in the rate of presentation. Therefore the subjects were allowed at any time to delay the farther presentation of a series until they had satisfied themselves as to the nature of a unit. It must be confessed that in the first presentation of a smell-series,

a subject often retained a bottle until she could name the odor. However, this puzzling over the name, irregular as it made some of the intervals, tended to prevent the review of foregoing units.

In the presentation of the smell-series, the rate was not only more irregular but was also slower than in the case of the 'visible' series. It was retarded simply by the fact that smells are apprehended more slowly—at best—than colors are 'grasped' or syllables read.¹ (The manipulation of the bottle stoppers became after a little practice a very automatic and rapid affair.) Of course, the later presentations of the smell-series took place more rapidly and more regularly than the first presentation because the subjects relinquished the bottles more rapidly and at more regular intervals.

In the case of the subject G., eleven or twelve bottles to the minute was the ordinary rate in the first presentation. This rate would give an average interval of from 5 to 5.5 seconds from the beginning of one exposure to that of the next. The average interval is approximately also the most ordinary interval. Of this total interval, the exposure occupied fully half. In the case of the other subjects, the average interval and the ordinary or normal interval do not correspond at all on account of the large number of intervals greatly in excess of the normal. Certainly the rate of twelve bottles to the minute was rarely, if ever, exceeded with any subject in the first presentation of a series.

At this point, the question may arise as to why the intervals with the three kinds of series were not all made so long that they could be uniform. The reason is this: The intervals—all exceedingly long in comparison with the intervals in most other memory experiments—were, so to speak, already subjectively equal, i.e., they allowed the subjects the same small margin of vacant time. In the smell-experiments, the attention of the subjects was pretty fully taken up in receiving the bottles, and in uncorking, smelling, recorking, and setting them down. In the other experiments, attention was largely absorbed in looking at the colors or reading the syllables and in watching in expectancy the rapid hand-movements of the experimenter.

¹ On the slowness of smell-reaction time, cf. Zwaardemaker, *op. cit.*, p. 202.

If, however, the colors and syllables had been presented as slowly as the smells, the subjects would have had *at least* one full second of vacant time to each unit, an interval during which their mental occupation would have been difficult to control.

The crucial question of exactly what the subjects *did* do during the presentation of a series must be answered in connection with the introspective results. Nevertheless, two possible consequences of the slow rate of presentation should be explained in connection with this preliminary discussion of method. Let us again represent the units of a series by the letters of the alphabet in order. Now, in the first place, if the links are exposed for a long time and at long intervals, then during the exposure, say, of *e*, and during the interval between *e* and *f*, there may be time for a peculiar clinching of the association $\widehat{d}e$. By the aid either of a visual or of a verbal image, the subject may bring the two links into the same mental content, almost as if they were presented simultaneously. This he cannot do if the series is presented very rapidly, not because the link *d* does not *tend* to 'persist,' but because his whole time between *d* and *f* is occupied in 'grasping' *e*. In rapid presentation, he has a set of more or less discrete experiences, *a*, *b*, *c*, *d*, *e*, and so on. In slow presentation,—even if only the intervals and not the exposure times be long,—he may have a set of linked experiences, either *a—b*, *b—c*, *c—d*, *d—e*, etc., or *a—one*, *b—two*, *c—three*, or the like. In the second place, as a consequence of slow presentation, some 'quick-witted' subjects may have an opportunity in the interval, say, between *e* and *f*, to review the sequence *c—d* or even to go back as far as *a—b—c*. This possibility has been already suggested (p. 11). A third and more obvious consequence (already mentioned) is the temptation to use mnemonic devices.

c. The intervals between the several presentations of the series as a whole were unavoidably irregular, because every presentation of a series was followed by an attempt at reconstruction and because these attempts consumed a very variable amount of time. This amount depended (1) upon the length of the series and (2) upon the order in which the subject received or took up the units. The lengthening of the series made the

interval between the presentations longer because the subject had more units to place, because the experimenter had a longer record to make, and because the experimenter had more units to rearrange in the proper order for the next presentation. Thus, in the longer series, a longer interval supervened *between the presentations of the same sequence* both because the *rest of the series* took more time for presentation, and because longer intervals supervened between the several presentations. The longer series had, therefore, in a marked degree the advantage of 'distributed repetitions' (*Verteilung der Wiederholungen*).¹

d. The interval between the presentation and reconstruction of a series was comparatively though not strictly regular. It consisted simply of the amount of time which the experimenter took to mix or shuffle the units to her own satisfaction. Thirtyseconds is an ample estimate for the shuffling of a card- or syllable-series of 41 units. A rather longer time was required for mixing the scent-bottles than for shuffling the colors and cards. During this interval there was neither instruction nor distraction to prevent the subjects from reviewing some part of the series if they cared to do so.²

e. The period required for the reconstruction of a series was not in general timed. This omission was unfortunate. As regards the individual subject, the times have little significance since they vary with the chance order in which the subject was compelled to deal with the several links (cf. page 32). Nevertheless, gross variations as regards the time consumed in reconstruction seem to have constituted one of the most important individual differences between the subjects. There are abundant indications of this fact in the results although no exact figures can be given. In all cases, the reconstruction time greatly exceeded and usually it at least doubled the presentation time, since the subjects were allowed to reflect as long as

¹ See Ebbinghaus, *Grundzüge*, pp. 657-660 and Jost, *Die Assoziationsfestigkeit in ihrer Abhängigkeit von der Verteilung der Wiederholungen*, Zeitschrift f. Psych. und Phys. der Sinnesorgane Bd. xiv, pp. 436-472.

² This was one of the instances in which the director preferred to have no regulation rather than to risk that peculiar lack of uniformity which is entailed by a rule imperfectly kept. It must not be forgotten that the experimenters and subjects were many.

they liked upon the placing of each unit. In general, the first reconstructions of a series required more time than the later, but occasionally at the first attempt a subject would place the units hastily and at random rather than waste time upon a reconstruction which would be in any case incorrect.

f. The intervals between the memorisings of different series at the same sitting were also irregular. When one series had been perfectly reproduced, another was given as soon as the experimenter could make and record it. If prepared before the beginning of the sitting, it was presented at once without more than a two-minute interval for rest. In these experiments, the whole process of presentation and reproduction was so leisurely, that the strain upon the subject was relatively insignificant except in special cases which will be noted later. As many series were given at one sitting as the time allowed. The sitting was ordinarily forty-five minutes long. This period *barely* allowed a series of forty-one smells to be three times presented to the subject G., and three times reconstructed, and these three rearrangements to be properly recorded. A series of twelve smells could be once presented to G. and once reconstructed by her in three or four minutes.

g and *h.* The intervals between the experimental sittings and the hour of the day at which the experiments were made varied much from one subject to another, but were kept uniform from week to week. Details upon these points can best be given in connection with the results.

i. The number of series of different kinds which could be given at one sitting was too small and too variable to make stress upon their order at that sitting profitable. In general, only the influence of practice *from day to day* was considered in arranging a program of series to be memorised. At each sitting, the experimenter proceeded with the program as far as the time allowed. The effect which practice and fatigue might have within the limits of a given sitting were disregarded. Three types of program were in use: first, the haphazard program (more truly a lack of program) in which the order of occurrence of the different kinds of series depends on chance or on the caprice of the experimenters; second, the cyclic program in

which series of the different kinds (say, kind *a* and kind *b*) are used in strict rotation (thus, *a, b, a, b, a, b* and so on); and third, the compensating program (e. g., *a, b, b, a; b, a, a, b; b, a, a, b; a, b, b, a*, and so on); in which the order of occurrence equalises the effect of practice for the different kinds of series.

The most important points in this section are the following: (1) the series were presented to the subjects without the use of apparatus. No apparatus could be used in the smell-experiments. The color- and syllable-experiments were made for purposes of comparison. Therefore, no apparatus was used in them. (2) Vision, apart from visual imagery, was allowed to play no rôle whatever in the smell-experiments. (3) For rearrangement, the links of the series were given to the subject in chance order. This order was one of the minor factors in determining the difficulty of reconstruction. (4) The subject was required, as far as possible, to deal with each link singly, and once for all to indicate more or less exactly its place in the series by putting it into certain definite spatial relations with the links already placed. If the position of each link was not properly indicated before a new link was presented, the reconstruction was counted as imperfect. Thus, no perfect reconstruction could be a shifting, tentative reconstruction. (5) The requirement that the subject should deal with the links singly was, in the case of the color- and syllable-series, limited by the fact that the series were gradually spread out before the subject's eyes in the process of reconstruction. Thus, both inference and reproductive reinforcement were more important in the reconstruction of these series than in the case of the smell-series. (6) In the case of the smells, on the other hand, the subject had the double task of remembering the order of the series and the contents of the bottles which she had already placed. Thus, owing to the elimination of vision, the smell-series both lack an advantage possessed by the other series and are put at a positive disadvantage as compared with them. (7) The links of the smell- and color-series were presented for memorising at rather irregular intervals because time was allowed the subject to apprehend or 'grasp' the unequally

distinct units. (8) With all three sorts of material the presentations were very slow as compared with those in other memory experiments. This slowness was due in part to the lack of apparatus. In the case of the smell-experiments, the rate was retarded by the slowness with which smells are apprehended. In other words, the slow rate was due directly and indirectly to the nature of the material. (9) The slow rate of presentation tends to make it possible for the subject to grasp successive units in pairs, to review parts of the series while the presentation is still in progress, and to interpolate many connecting links between the units. (10) The longer series have by our method in a marked degree the advantage of distributed repetitions.

B. Treatment of the Numerical Results.

Experiments made by this form of the reconstruction method furnish several sets of numerical results. (I.) The experimenter may count (*a*) either the number of errors made upon any given attempt at reconstruction, or else (*b*) either (1) the number of sequences correctly reproduced or (2) the number of members given their true absolute positions in the series. (II.) He may also count (*a*) the number of repetitions necessary to secure the correct, and (*b*) the number necessary to secure the perfect reconstruction of the series. (III.) Finally, in a group of series of any one kind, he may count (*a*) the number of series which are correctly reconstructed, and (*b*) the number of series which are perfectly reconstructed at the first trial or upon any given number of trials.

I. In the number of right cases which are obtained after any given number of repetitions, one has a measure of the *degree* to which the subject has been able to master the series by means of this number of repetitions. We preferred to keep our accounts in terms rather of right cases than of errors, that is, in terms of what the subject did learn rather than in terms of what she did not. We counted two sorts of right cases, namely, (*a*) 'right sequences' and (*b*) 'right positions.' If a series represented by the twenty letters from *a* to *t* be arranged in the order

a—b—d—c—e—f—l—h—i—j—k—g—m—q—r—n—o—s—

$p-t$, then this reconstruction contains seven right sequences,—namely, $a-b$, $e-f$, $h-i$, $i-j$, $j-k$, $q-r$, and $n-o$,—and nine right positions, that is, nine cases in which units—namely, b , e , f , h , i , j , k , m , and t —were put into places corresponding to their absolute numerical positions in the series. The putting of a into the first place was not usually counted as a ‘right position’ because the first link was generally handed to the subject with the statement that it was the first. Thus, in each series, the same number of right positions and of right sequences was possible, and this number was just one less than the number of units in the series.

Both methods of counting have a certain justification, and neither is entirely adequate without the other. This is clear from the illustration. If only the right sequences were reckoned no account would be taken of the fact that the subject knew that t was the last unit in the series; if only the right positions were reckoned no account would be taken of the fact that r was known to follow q and o , n . A reconstruction may furnish half the number of right positions possible without exhibiting any right sequences whatsoever; on the other hand, it may furnish only two less than the possible number of right sequences without exhibiting any right positions whatsoever. Let us suppose that a series consists of only nine members. Then the reconstruction, $a-d-c-b-e-h-g-f-i$ illustrates the first possibility, and the reconstruction, $a-i-b-c-d-e-f-g-h$ the second. Moreover, aside from the fact that neither method of counting alone can fully represent the accuracy of the reconstruction, it was worth while to count both sequences and positions because the results of the two methods of counting often enable the experimenter to check the introspective statement of the subject as to whether she had taken more pains to impress upon herself the sequences, or the positions of the units in the series.

Nevertheless, cases of extreme discrepancy between the results of the two methods of counting are rare. As a rule, the number of right positions exceeds the number of right sequences, but exceeds it only by a margin which is explicable from the fact that nearly every transposition of two members works

greater havoc with the sequences than with the positions. If the two members transposed do not stand next to each other, then we have four wrong sequences to two wrong positions unless one member is the last of the series; if they do stand next to each other, then we still have three wrong sequences to two wrong positions unless the two transposed members are the last of the series. (Note the results of transposing *c* and *d*, and *g* and *l* in the first illustration of reconstruction.) Because the results of the two methods of counting tally so closely, only the numbers of right sequences will be given in the numerical tables, except when the positions have some special significance.¹ However, in every group of series represented both sorts of wrong cases have been counted and the figures could be given.

Between the presentations of the series, the experimenter did not take time to count the right cases, but simply recorded the order in which the series-members were arranged. The numbers of the scent-bottles, the color-symbols, and the syllables were written in proper order in vertical columns at the left of the page. After the several trials, the numbers were written in parallel columns in the order which the subject had given to the series-members represented by them.

In massing the results for different series, absolutely no averaging was done. This is true even when the results were obtained from different subjects. When the numbers of right cases are presented in the form of per cents, the denominator of the fraction reduced to a per cent is always the *total* and *not* the *average* number of right cases possible, and the numerator is always the total number of right cases actually obtained.

II. Little need be said in regard to counting the number of repetitions necessary to a correct and to a perfect reconstruction, although the figures thus obtained are perhaps the most important which we have to offer. Of these numbers the arith-

¹ Sequences are chosen rather than positions because in some cases the subject abandoned the attempt to reconstruct a series after rearranging a few fragments of it. In these fragments, the right sequences might well be counted but the absolute positions of the units were not definitely enough indicated for the placing to be taken into account.

metrical mean and the mean variation have been found for each 'set' or 'constellation' of series. It has not been worth while to find the central value because the numbers themselves which enter into the averages are for the most part so small (not *few* but *small*).

The writer should here emphasize the fact that by the expression 'number of repetitions'¹ and by the expression 'number of trials,' when used in connection with the reconstruction method, the reader is to understand a number of presentations followed each by a corresponding attempt at reconstruction. The writer does not know of any other set of memory-experiments in which *each* of a *number of complete* presentations has been followed by an attempt at reconstruction or reproduction.

In massing the results for different subjects, averages were never averaged. All results, when massed, were treated as if obtained from the same subject.

III. The third method of treating the results numerically requires some explanation and justification. This is the counting of the number of series of a given kind which are (a) correctly and (b) perfectly reconstructed upon the first or upon any given attempt. It may be thought that figures thus obtained can show nothing which would not emerge from the other two sets of figures. This is not wholly true. For other attempts at reconstruction than the first, this sort of counting is indeed of little importance. Nevertheless, when applied to the first attempt, it gives a notion of the subject's memory-span which cannot be given by the first set of figures and may not be given by the second. This point can be made clear by illustration. Suppose that from 10 series of 12 members each, 90 right cases are obtained. This gives an average of 9 right cases for each series. How can one tell from this average whether any of the series in question were right at the first trial or not? Again, suppose that 10 series of a given length are reproduced correctly upon an average of 1.5 trials. This would be the average if 5 of the series were correctly reconstructed at the first and 5 at the

¹ The word repetition is, of course, used inconsistently, like the German *Wiederholung*, to include the *first* presentation of the series.

second trial. In this case, it would be safe to say that the length in question lay just at the edge of the subject's memory-span. But 1.5 would be the average *also* if 9 of the 10 series were correctly reproduced at the first trial, whereas some one series 'ran over' to the sixth.¹ In this second case it would be fair to say that this series-length lay within the subject's memory-span. Even the mean variation gives no adequate notion of the distribution of the numbers about the average. In the first of these two extreme cases, it is 0.5, whereas in the second it is only 0.9.

An elaboration of this third method of treatment consists in counting the numbers of series which are reconstructed at any given trial correctly, but with certain grades of imperfection. We considered the transposition of series-members a more serious fault than the making or leaving of a gap in the line, and any infringement of the spacing-rules a more serious fault than uncertainty in regard to the correctness of the reconstruction.

The figures obtained by this third method of counting were made to furnish per cents and not averages. The principle upon which results were massed was exactly the same as in the first method of counting. There was absolutely no averaging, either of the sums obtained in different sets of series or of per cents.

In brief, we have in the first method of counting as in the methods of right associates, prompting, and retained members, a method of determining the *degree of mastery obtained by any given number of presentations* short of the number necessary to complete mastery; we have in the second method of counting, as in the method of complete memorising, a means of determining *the number of presentations necessary to perfect mastery*; and lastly we have, in the third method of counting, a means of determining *the memory-span* for the serial order of different sorts of impressions.

¹Such anomalous values occur frequently in all memory experiments and cannot fairly be thrown out. Cf. Müller and Schumann, *op. cit.*, pp. 272-274.

C. *Preliminary Comparison with Other Methods of the Reconstruction Method as Used in These Experiments.*

The purpose of this section is briefly to compare our form of the reconstruction method, first, with the reconstruction method of Münsterberg and Bigham, and second, with the various methods of memory-investigation in which series are memorised and their members (not merely the order of the members) reproduced.¹

Ebbinghaus² divides methods of memory investigation into reproduction methods and recognition methods. Accepting this division as fundamental, the writer would farther divide the reproduction methods into (a) methods in which series are memorised and (b) methods in which the subject's more or less accidental associations with various words or objects are tested. These two classes of methods may be called for brevity (a) memorising methods³ and (b) methods of free recall. The methods of free recall, namely, (1) the method of chance word-reaction or *Methode der zufälligen Wortreaction*, of Aschaffenburg and Wreschner and (2) the method of cursory enumeration, or *Methode des fortlaufenden Niederschreibens oder Aufzählens*, of Kræplin, do not concern us here. The methods in which series are memorised may be again divided (1) into methods in which the members of the series are reproduced and (2) the reconstruction method in which merely the order is reproduced.

I. The reconstruction method owes its name to Prof. G. E. Müller,⁴ but, so far as the writer knows, it was first used by Münsterberg and Bigham in their study of 'the action of dis-

¹ The word *method*, as used without qualification in these pages, means a *memory-test method* and includes both an experimental and a numerical procedure. Any of the various *methods* of or for *memorising* which have been studied by Steffens, Ephrussi, and Ebert and Meumann—that is, the G-procedure or learning *im Ganzen*, the T-procedure or learning *in Teilen*, and so on—may form a part of any of the test-methods in which series are memorised.

² *Grundzüge*, p. 645.

³ Not methods of memorising. See note, p. 41.

⁴ This statement is made upon the authority of Pohlmann, *Experimentelle Beiträge zur Lehre vom Gedächtnis*, p. 5.

parate senses in recollection.¹ As a matter of actual fact, our method was not devised in conscious imitation of Münsterberg and Bigham, but was gradually worked out as a method of testing the memory for smells without the use of words. Our aim was to make our method as much like the method of complete memorising as the materials and the object of the experiments would permit. Hence, the essential differences between our procedure and that of Münsterberg and Bigham. The first of these differences lies in the fact that we were not content simply to determine the extent to which the series order could be mastered by one presentation, but also strove to determine the number of presentations and attempts at reconstruction which were necessary to perfect mastery. In the Münsterberg and Bigham experiments each subject saw each series only once. The other essential difference consists in the pains which we took to avoid reconstruction by inference, guess-work, and tentative arrangement. Since Münsterberg and Bigham do not expressly say that they dealt out their color-squares and number-cards one by one during the process of reconstruction, we seem justified in assuming that they gave them over to the subjects *en masse*. In any case, they would certainly have mentioned the fact if they had had anything corresponding to our placing-rules, which, as already noted, were introduced to correspond with the rule about hesitation in the method of complete memorising. Non-essential differences, though matters of important detail, are the difference in the rate at which the series were presented, the fact that Münsterberg and Bigham used apparatus in presenting their color- and number-series, and the fact that they estimated degree of mastery only by counting errors of position ('the number of changes necessary to set the series right').

II. The reproduction methods in the narrower sense, that is, the methods in which *series* are memorised and their *members* and not merely the *order* of the members reproduced, are (1) the method of complete mastery or *Erlernungsmethode*, (2) the

¹ Studies from the Harvard Psychological Laboratory: *Memory*, Psychological Review, vol. i, pp. 34-38.

memory-span method or *Methode der Gedächtnisspanne*, (3) the method of right associates or *Treffermethode*, (4) the prompting method or *Methode von Hilfen*, and (5) the somewhat primitive method of retained members or *Methode der behaltenen Glieder*.¹ This subsection contains (a) a very brief sketch (inserted for the reader's convenience) of these five methods,² (b) an enumeration of the essential points of resemblance between our method and certain of the five, and (c) an explicit indication of the peculiarities of our method. It is unnecessary in this chapter to lay further stress upon the most obvious difference between our method and all the five, namely, the fact that they are reproduction methods in the narrower sense and are therefore suited only to verbal material, whereas our method is a reconstruction method and thus requires that merely the order of the series and not its members should be reproduced (cf. page 51 and Ch. I, page 10). Everything else which it is

¹ The reader is reminded (1) that the method of complete memorising was devised by Ebbinghaus for the pioneer experiments of *Ueber das Gedächtnis*, but was refined by Müller and Schumann (*op. cit.*); (2) that the *Methode der Gedächtnisspanne* was inaugurated by Jacobs (*Experiments in Prehension*, Mind, xiii, 1887), who was the first person to speak of a memory-span, and that it has in recent years been somewhat elaborated by Ebert and Meumann (*Ueber einige Grundfragen der Psychologie der Übungsphänomene im Bereiche des Gedächtnisses*, Archiv für die gesammte Psychologie, iv); (3) that the *Treffermethode* was elaborated by Müller and Pilzecker (*op. cit.*); (4) that the *Methode von Hilfen* was devised and named by Ebbinghaus and tested somewhat farther by Frl. Ephrussi (*op. cit.*); and (5) that the *Methode der behaltenen Glieder* received its name from Ebbinghaus but has been most carefully studied by Pohlmann (*op. cit.*). It is scarcely accurate to say that the *Treffermethode* was devised by Müller and Pilzecker, for in a simple form it had been used by Miss Calkins in her experiments in association. (*Association*, Psychological Review, Monograph Supplement, No. 2.) The *Treffermethode* of Müller and Pilzecker was first described by Jost (*op. cit.*). We need not here concern ourselves at all either with the reproduction-time measurements which are possible in the use of the *Treffermethode* nor with the applicability of the *Erlernungsmethode* to determining the number of repetitions which are 'economised' when series are relearned. The former received from Müller and Pilzecker the full name of *Treffer- und Zeitmethode*, and the latter from Ebbinghaus the name of *Erlernungs- und Esparnismethode*. The writer owes to Bergström the satisfactory translations of *Erlernungsmethode* and of *Treffermethode* used in the text. (*Effect of Changes in the Time Variables in Memorising*. Am. Jour. Psych. xviii.)

² A remarkably compact account of the different methods so far used in memory-investigation may be found in Pohlmann, pp. 2-7.

necessary to say upon this point will be reserved for the sixth chapter.

a. 1. In the method of complete memorising the subject goes over the series again and again (reading, hearing, or reciting) until he succeeds in reciting it without mistakes, without hesitation and with certainty of correctness. The presentations and reproductions all blend or may blend, except only the first presentation of the new series and the last perfect reproduction. At the beginning of any repetition of the series the subject may commence himself to recite it, but as soon as he hesitates or makes a mistake the experimenter presents the rest of the series to him. The experimenter counts the number of presentations necessary to the perfect recitation, usually including both the first presentation and the perfect recitation itself.

2. In the memory-span method, the experimenter finds how long a series can be reproduced in order without omissions or mistakes, or how long a series can be reproduced with only a given *quantum* of error, after a given number of presentations. The number of presentations is usually, but is not necessarily, one.¹ The experimenter begins with series which can be easily mastered with the given number of repetitions, and increases the series-length until the limit which he seeks to establish is reached. The procedure is akin to the method of limits in psychophysics. A descending procedure is sometimes added to the ascending procedure. In this method presentations and reproductions are distinct.

3. In the method of right associates, after the series has been presented to the subject a given number of times, alternate members are presented out of their original order, and upon each presentation the subject is required to name the member

¹ Bergström (*op. cit.*, p. 211). considers the method of retained members as a form of the memory-span method. Ebbinghaus, on the other hand, does not mention the memory-span method, although he does discuss the results of tests of immediate memory (*Grundzüge*, pp. 649-651). In the opinion of the writer, the term memory-span method *should only be* applied to a method in which the number of repetitions and degree of mastery are predetermined and in which series-length is the point to be determined (see text below), and it *may fairly be* applied to any such method, whether the number of repetitions be one or more.

just succeeding or sometimes the member just preceding. The experimenter counts the number of right associates, of partially right associates, of wrong associates, and of 'zero-cases.'

4. In the prompting method, after a given number of presentations, the subject is required to try to recite the series, and the experimenter counts the number of times when it is necessary to prompt or to correct him.

In these last two methods, the last presentation and the one reproduction blend. As Ebbinghaus points out, the one method is 'in a way the negative' of the other.

5. In the method of retained members, after a given number of presentations the subject is required to name as many members of the series as he remembers. The experimenter counts the total number named and notes the nature of the errors. Here again presentations and reproductions are distinct.

The various methods may be briefly compared as follows: In the method of complete memorising, the series-length is fixed; the degree of mastery to be attained is fixed, as perfect—i. e., as the degree necessary to recite the series without mistakes, without hesitation and with certainty of correctness,—and the *number of repetitions* necessary to attain this mastery is found by experiment. In the memory-span method, the degree of mastery to be attained is fixed—ordinarily, but not always, as perfect—the number of repetitions is fixed, and the *length of the series* which can be mastered to the required degree by the given number of repetitions is determined by experiment. In the other three methods, the series-length is fixed, the number of repetitions is fixed, and the *degree of mastery* which can be attained by this number of repetitions is found by experiment. The methods of complete memorising and of the memory-span are alike in requiring the complete memorising of the series; the other three methods are alike in not requiring it. In fact, in these last three methods it is essential that the number of repetitions should *not* be adequate to the mastery of a series of the length given, or at least that it should not be adequate in *both* of two groups of series which are to be compared. The first

two methods determine conditions of mastery; the last three, degree of mastery under different conditions.¹

b. Our method is a kind of compound of the method of complete memorising, on the one hand, and of the method of retained members, on the other, and has in addition a certain kinship to the memory-span method. In the first place, our method, like the method of retained members, determines the fraction of the *total* content presented which can be memorised by a given number of repetitions, and tests this fraction without prompting. Presentation and reproduction are altogether separate affairs. The kinship between our method and the method of retained members is the most obvious of the resemblances under discussion. In the second place, our method, like the method of complete memorising, determines the number of repetitions necessary to complete mastery. In the third place, by finding the percentage of the total number of series of a given length which attain a given grade after a given number of presentations, our method gives some notion of the subject's memory-span. It should be explicitly noted that the writer is not restricting the expression memory-span to the span of the 'immediate memory,'—that is, to the amount which the subject can remember after a single rapid repetition of a series of impressions.² The product of immediate memory is a kind of centrally aroused after-image, and depends rather upon attention-span and upon 'persistence'³ than upon facility in forming associative connections.⁴

Our method has least in common with the method of right associates. In this method, the subject is not required to reproduce the total content presented but only the half of it, and in the reproduction of this half the direct progressive (or else the direct regressive) associations are of almost exclusive importance, whereas in the reconstruction method, the associations between members of the series which do not stand next to each

¹ On the general distinction between methods which determine the conditions and methods which determine the degree of mastery, cf. Ebbinghaus, *Grundzüge*, p. 648, and Bergström, *op. cit.*, p. 210.

² Ebbinghaus, *Grundzüge*, pp. 649-650.

³ See p. 204.

⁴ Cf. Ebert and Meumann, *op. cit.*, pp. 204-206.

other, the 'criss-cross' associations, are of far more importance than in any of the other methods. Furthermore, the method of right associates furnishes more, and our own method less, opportunity for accurate measurements of the reproduction-time than any of the other methods under discussion.

c. Our method has two important peculiarities. One of these peculiarities has already been noted in the last paragraph but still requires some farther discussion. In the reconstruction method, the *direct regressive* associations (such as \widehat{ba} or \widehat{fe}) and the 'cross' associations between units in the series which do not stand next to each other (such as \widehat{eh} or \widehat{gd}) are far more important than in the reproduction methods proper. In these methods, the *direct progressive* associations (such as \widehat{ab} and \widehat{ef}) are paramount. Two factors which are essential to the reconstruction method give relatively great importance to 'regressive' and 'cross' associations. The first of these is the premium put upon notions of absolute position in the series; the second is the projection of the whole series into spatial relations. In our experiments, the direct regressive associations were especially fostered by the placing-rules which made it quite as important for the subject to remember what a given link came after as what it came before. When a series is once learned by the reconstruction method, the units are bound together by *many threads of almost equal strength*, and these threads may leap over several intervening units and bind links which are relatively distant to each other.

The other respect in which our method stands alone is in requiring one reproduction (in the looser sense) of the series as a whole for every presentation of it as a whole. In the method of retained members, although reproduction and presentation are wholly separated, yet each series is tested but once, whether it is presented once or half a dozen times. The question arises as to whether or not in our method the *attempts at reconstruction* themselves contribute so much to the mastery of the series that one ought not to speak of the degree of mastery as conditioned, wholly or mainly, by the given number of *presentations*. The discussion of this question will be reserved for the critical exami-

nation of our method which will be made in Chapter VI on the basis of experimental results.

It may be noted here that if the question is answered in the affirmative, the explanation by multiplied tests is proved valid.¹ On the other hand, if it is answered in the negative, there would be no crucial objection in the methods of right associates, prompting, and retained members, to making a test of mastery after *each* of a series of presentations. Moreover, it might be admissible in the method of complete memorising to *prompt* the subject at each hesitation or mistake (instead of presenting the rest of the series *in toto*) and thus to gauge the *progress due to each repetition*.

¹ See p. 9.

CHAPTER IV.

NUMERICAL RESULTS OF EXPERIMENTS IN MEMORISING THE SERIAL ORDER OF SMELLS AND OF COLORS.

This chapter deals with the numerical results of the first three groups of experiments, which were all made by the reconstruction method and with smells and colors as material. The chapter falls into two sections.

A. Tables of Results with Explanation and Discussion.

I. Tables I—VI.

See tables on following pages.

II. Explanation of Tables.

a. The abbreviations with their meanings may be listed as follows:

L stands for *series-length* and *ML* for the *mean length of the series* in any one set.

N stands for the *number of series* (not the number of right cases possible) in any one set. *Nr* stands for the total number of series which *finally* were reconstructed *correctly* (rightly) and *Np* for the total number which *finally* were reconstructed *perfectly* in any one set.

RS stands for right sequences. *RS%* stands for the percentage which the right sequences, actually obtained upon any given trial in any set of series, form (taken together) of the right sequences which were numerically possible in that set upon that trial. (See illustration on page 68.)

RP and *RP%* will be used in a strictly analogous fashion for *right positions*.

W stands for *number of repetitions*. (The symbol *W*, initial of *Wiederholungen*, is selected for the sake of uniformity with the Ebbinghaus and Müller work.) *Wr* stands for *average number*

TABLE II
Experiments of Group I., Subgroup 2. Material: Smells

Subject.	Period.	Total Number of Rs possible.	L: 5, 6, 7, 8						L: 9, 10, 11, 12						L: 13, 14, 15, 16								
			ML	N	Nr	Wr	Np	Wp	RS % W = 1	ML	N	Nr	Wr	Np	Wp	RS % W = 1	ML	N	Nr	Wr	Np	Wp	RS % W = 1
E.	1	338	6.7	40	39	2.7	39	3.2	10	7	7	4.0	7	4.6	
	2	304	7.6	14	14	2.9	14	3.1	48	10.1	14	14	4.0	14	4.6	40	14.4	5	0	0	12
	3	553	7.2	13	13	1.8	13	2.1	75	10.6	32	30	2.3	29	2.5	50	14.8	12	11	3.0	11	3.2	37
F.	1	458	6.8	48	48	2.1	48	2.5	10.1	13	13	3.0	13	3.8	
	2	425	7.1	17	17	1.6	17	2.1	77	10.2	25	24	2.4	24	2.8	44	14.3	7	6	3.3	5	3.4	30
	3	314	7.7	6	6	1.2	6	1.3	93	10.8	15	13	2.2	13	2.3	42	15	9	8	2.6	8	2.9	32
Ha.	1	279	6.4	36	36	1.3	35	1.6	9.8	5	5	2.0	5	2.8	
	2	588	7.2	33	33	1.2	33	1.3	89	10.3	41	41	1.6	41	1.7	81	14.5	12	12	1.9	12	2.3	65
He.	1	559	7.	50	50	1.3	50	1.4	10.	22	22	1.9	22	2.1	
	2	496	7.1	11	11	1.2	11	1.3	96	10.5	24	24	1.2	24	1.4	93	
Hu.	1	296	6.6	45	44	1.8	43	2.3	
	2	438	7.1	21	21	1.8	21	2.0	73	10.1	28	25	2.8	25	3.4	58	
	3	651	7.4	10	10	1.4	9	1.8	80	10.8	21	20	1.2	20	1.6	84	14.9	12	10	2.8	10	3.2	40
Wa.	1	202	6.8	27	27	1.2	27	1.5	
	2	567	7.2	29	29	1.0	29	1.1	100	10.2	32	32	1.2	32	1.4	93	
	3	557	10.8	17	16	1.3	16	1.7	85	15.1	10	8	1.0	8	1.3	94

TABLE III
Experiments of Group II, Subgroup 2. Material: Smells

Subject.	Period.	Total Num- ber of R S Possible.	L	N	Nr	Wr	MV	Np	Wp	MV	R S % W=1	L	N	Nr	Wr	MV	Np	Wp	MV	R S % W=1
D.	II, 1	1741	31	35	33	2.0	0.4	24	2.2	0.3	80	61	11	10	2.8	0.5	6	3.2	0.6	43
	II, 2	760	41	9	9	2.0	0	5	2.0	0	65	81	5	2	3.0	0	1	3.0	27
Gr.	I	297	12	27	24	2.6	1.0	20	2.5	0.8	49
	2	297	12	27	26	1.9	0.5	26	2.1	0.6	62
	3	225	26	3	3	3.0	0	2	3.0	0	41	51	3	0	0	17
R.	I	429	12	39	39	2.3	0.8	39	2.4	0.9	59
	2	610	12	39	39	1.5	0.5	39	1.6	0.5	79
	3	350	26	8	6	2.3	0.4	6	2.3	0.4	33	51	3	1	3.0	0	18

TABLE IV
Experiments of Group II, Subgroup 3 and Group III., Subgroup 3

Material.	Subjects.	Period.	Number of R S possible	I								II								R S % W = 1	
				L	N	Nr	Wr	MV	Np	Wp	MV	L	N	Nr	Wr	MV	Np	Wp			
Smells	8 beginners	1	2515	12	88	69	2.7	1.1	55	2.9	1.1	43	18	91	44	3.3	1.0	25	3.4	1.1	27
		2	1147	12	44	42	1.7	0.6	33	1.7	0.6	69	18	39	30	2.5	0.7	14	2.7	0.7	49
		1-2	3662	12	132	111	2.3	1.0	88	2.4	1.1	51	18	130	74	2.9	0.8	39	3.1	0.9	34
Colors	15 beginners	1	3304	12	118	117	2.2	0.6	108	2.6	0.8	50	18	118	116	3.4	0.9	108	3.9	1.1	32
		2	2940	12	105	104	1.8	0.5	100	2.1	0.5	70	18	105	104	2.7	0.9	100	3.0	0.8	44
		3	2016	12	72	72	1.6	0.5	71	1.8	0.5	75	18	72	72	2.3	0.6	71	2.6	0.7	51
		1-2-3	8260	12	295	293	1.9	0.5	279	2.2	0.6	63	18	295	292	2.9	0.8	279	3.2	1.0	41

TABLE V.
Experiments of Group III, Subgroup I. Material: Colors. Subject G.

Period.	Total Number of RS Possible.	ML	N	Nr	Wr	MV	Np	Wp	MV	RS % W = 1	Period.	Number of RS Possible	ML	N	Nr	Wr	MV	Np	Wp	MV	RS % W = 1
I, 1	310	7.3	6	6	1.2	0.6	6	1.3	0.6	92	II, 1		22.6	19	19	2.0	0.3	19	2.3	0.5	56
		10.6	19	19	1.4	0.5	19	1.9	0.5	83			26.9	48	44	2.5	0.6	41	3.2	0.8	38
		13.7	15	15	1.9	0.5	15	2.1	0.3	56			29.9	10	8	2.6	0.5	6	3.2	0.6	38
I, 2	473	11.6	8	8	1.4	0.5	8	1.6	0.6	84	II, 2	2360	41	37	28	3.1	0.5	22	3.8	0.5	24
		14.5	20	20	1.5	0.7	20	2.2	0.8	86			81	11	2	4.5	0.5	0	11
		17.9	7	7	2.1	0.2	7	2.1	0.2	56											
II, 1	2439	15	8	8	1.8	0.4	8	2.4	0.7	69	II, 3	1480	41	11	11	2.5	0.5	11	3.4	0.7	31
		18.5	22	22	2.5	0.8	22	3.3	1.1	65			81	13	12	4.4	0.6	3	6.0	1.3	11

TABLE VI.
Experiments of Group III., Subgroup 2. Material: Colors

Subject.	Period.	Total Num- ber of R S Possible.	ML	N	Nr	Wr	MV	Np	Wp	MV	RS % W=1	ML	N	Nr	Wr	MV	Np	Wp	MV	RS % W=1
Hc.	1	336	6.9	12	12	1.5	0.6	12	1.8	0.6	80	10.3	20	20	1.7	0.6	20	2.0	0.5	73
	2	661	11.2	16	16	1.3	0.4	16	1.4	0.5	87	14.2	32	32	1.9	0.6	32	2.2	0.7	67
Hu.	1	335	6.9	13	13	1.3	0.2	13	1.2	0.3	90	10.4	23	23	1.6	0.5	23	2.2	0.6	60
	2	408	11.6	10	10	1.5	0.5	10	1.9	0.5	81	14.1	23	23	2.1	0.9	23	2.4	0.6	54
Sm.	1	349	6.8	11	11	1.4	0.5	11	1.4	0.5	83	10.5	19	19	2.0	0.3	19	2.2	0.5	54
	2	531	11	20	20	1.9	0.7	20	2.2	0.6	64	14.3	25	25	2.3	0.4	25	2.6	0.6	38
C.	1	154	12	14	14	2.4	0.5	14	2.9	0.7	43
	2	757	12	26	26	2.0	0.3	26	2.2	0.4	55	18	27	27	2.6	0.7	27	3.1	0.5	37
	3	1681	20.6	21	20	2.4	0.5	19	2.7	0.7	32	30.5	20	18	3.1	0.7	17	3.3	0.8	24
	4	460	41	17	11	2.6	0.5	11	3.2	0.5	20
N.	1	138	12	10	10	2.2	0.7	5	2.4	0.6	56	31	4	4	2.3	0.4	4	2.5	0.5	37
	2	420	12	15	15	2.3	0.4	10	3.9	0.7	22
	3	1573	20.5	16	16	2.7	0.7	16	3.0	0.6	30	18	15	15	2.8	0.6	15	3.4	1.1	30
	4	41	41	19	14	2.9	0.3	12	3.3	0.4	15	30.5	17	15	3.1	0.7	14	3.4	0.5	23

of repetitions necessary to a correct reconstruction. Wp stands for average number of repetitions necessary to a perfect reconstruction. $RS\%$, $W = 1$ stands for percentage of right sequences at the first trial.

MV , of course, stands for mean variation.

The abbreviations listed will have the same meaning in all the tables of this monograph.

b. In explaining the general arrangement of the tables, the following points must be noted:

1. The work of each subject has been broken up into 'periods' according to the time-order of the experiments. This division was made to bring out the effect of practice. The periods are not, in general, equal divisions of time. Details as to the division will be given below. In the case of the subjects G. and D. the Roman numerals in the period-column give the academic-year periods of the reconstruction-experiments with a particular material, and the Arabic numerals give the subdivisions of these periods. Thus in Table I, the expression I, 4 means the fourth period of the first year and II, 1 the first period of the second year. Between these periods lay the long vacation; therefore we should expect a fall in the practice-level between I, 4 and II, 1. In general, the higher the numeral for the period, the higher the practice-level should be. On the other hand, sets of experiments given in the same period are all *approximately* upon the same practice-level.

2. The figures given in the column headed 'Number of Right Sequences Possible' are found by adding the right sequences possible for all the series of the period *upon the first trial*. In some cases, series are taken into this count which are not included in the sets of experiments covered by the columns to the right. In some periods, especially with the subject G., 'scattering' experiments were made with series of many lengths. It is not possible to find space in the tables for all these experiments, yet it seems important to indicate in some compact fashion the relative amount of practice obtained in the several periods. In general, the discarded series-lengths lay some just below and some just above the lengths included in the tables.

3. By 'set' of experiments is meant what the German writers mean by 'Konstellation,' that is, a number of experiments whose results of any one kind (value of Wr , $RS\%$, or what not), taken together, make a unit or datum in the argument. In these first groups of experiments which have to do merely with the effects of practice and of series-length, a 'set' includes all the experiments upon a given subject with series of a given length or small group of lengths at the same practice-level.

To determine the effect of *practice*, one must examine the results obtained for series of the same length at different periods, working *downward* through the tables. On the other hand, to determine the effect of *series-length*, one must study the results obtained from series of different lengths in the same period, working *crosswise* through the tables.

4. The arithmetical work of massing series of different lengths in one set was done as follows: The mean length of all the series in the set was found, the numbers of possible and of actual right sequences in all the series were added together, and the values of Wr and Wp were found as if all the series in the set had been of the same length. It was, of course, by the 'scattering' of the series-lengths that such massing was made necessary. When no other arrangement is specified, the series are massed in groups of four,—the lengths 5, 6, and 7 with 8, 9, 10, and 11 with 12, and so on. A simple number (with no fraction) under ML of course indicates that the series in a set were all or nearly all of exactly the same length.

5. The reason why the percentage of right sequences is given only for the first trial is this: When the value of Wr is as small as it is in most of these experiments, the percentages of right sequences even on the second trial approximate too closely to be interesting. The relative amounts accomplished by successive repetitions will be discussed in the next chapter.

6. The reason why the figures under N , Nr , and Np are not always the same is that the end of the experimental sitting often came before a series, once presented, had been perfectly or even correctly reconstructed. When the number under Np is very small as compared with the corresponding number under N , nothing can be argued except that the time allowed for the

experiments was not long enough for dealing with so long a series. No series might be partially memorised at one sitting and finished at the next. This rule was rigidly enforced and in cases where it was broken, the results of the second sitting was thrown out on account of the distribution of the repetitions (*Verteilung der Wiederholungen*). In Table I, there is no room to make separate columns for Nr and Np . When these numbers differ from the number of series in the set they are inserted over the corresponding values of Wr and Wp after the expression $N =$.

To illustrate some of the statements in the last few paragraphs, let us take the results of the subject N from Table VI. Period 1 was given to preliminary practice. Ten series of the length 12 were given, making a total of 110 possible RS , 11 to a series. There were 24 actual RS on the first trial, putting all the series together. $24/110 = 21.8$ per cent = $RS\%$. Besides these series, two of the length 15 were given tentatively. These contained 28 possible RS . $110 + 28 = 138$, the number in the third column. In Period 2, the lengths 12 and 18 were compared. Thus this period contains two 'sets' of series. There were 15 series of each length. $(15 \times 11) + (15 \times 17) = 420$. In Period 3, the lengths of about 21, of about 31, and of 41 were compared. There were 8 series of the length 20 and 8 of the length 21, giving a mean length of 20.5. There were 9 of length 30 and 8 of length 31, giving a mean length of 30.5. $(8 \times 19) + (8 \times 20) + (9 \times 29) + (8 \times 30) + (19 \times 40) = 1573$. Of the 19 series of the length 41, only 14 ever attained to correct and only 12 to perfect reconstruction. If, however, one compares the values of Wr and Wp in this set of experiments with the values obtained by this subject in other sets, it is clear that the dropping out of so many series was due to the ending of the sitting (that is, to the limitations of the undergraduate schedule).

c. Certain rather tedious particulars must now be given in regard (1) to the general scheme of the experiments, (2) to the academic status of the subjects other than G., (3) to the division

of periods, (4) to the hours of the experimental sittings,¹ (5) to the time-order (*Zeitlage*) of the series, and (6) to the massing of the work on the beginners. The following scheme gives details upon some of these points:

GROUP I. Material: Smells.

Plan: To study smell-images and to reach the practice-limit in memorising order.

Subgroup 1. Subject: G.

Date and sittings: Periods I. 1-4: January-June, 1902; 12 per week.

Periods II. 1-5: September, 1902-May, 1903;

12-14 per week.

Subgroup 2. Subjects: E., F., Ha., He., Hu., and Wa.

Date and sittings: October, 1902-April, 1903; 2 or 3 per week (for each subject).

GROUP II. Material: Smells.

Plan: To study the effect of series-length.

Subgroup 1. Subject: G.

Date and sittings: Period III.² 1-2: September, 1903-May, 1904; 4 per week.

Subgroup 2. Subjects: D., Gr., and R.

Date and sittings: September, 1903-May, 1904; 2 or 3 per week.

Subgroup 3. Subjects: 8 beginners.

Date and sittings: 4 weeks, spring of 1904; 4 per week.

GROUP III. Material: Colors.

Plan: To study the effect of practice and of series-length.

Subgroup 1. Subject: G.

Date and sittings: Periods I. 1-2: January-June, 1903; 1 per week.

Periods II. 1-3: September, 1903-May, 1904; 6 per week.

Subgroup 2. Subjects: (1) He., Hu., and S; (2) C. and N.

Date and sittings: (1) January-June, 1903; 1 per week.

(2) September, 1903-May, 1904; 3 per week.

Subgroup 3. Subjects: 15 beginners.

Date and sittings: 4 weeks, spring of 1904; 4 per week.

¹ All statements as to hours and weeks refer to scheduled appointments. It should be understood both that subjects and experimenters sometimes 'cut' these appointments, and that the longer programs were broken by the short vacations and the mid-year examination period.

² *Period III* means the third year-period of G.'s work with smells. See p. 66.

1, a. The experiments of Group I broke the ground for all the succeeding experiments. They were of a tentative and unsystematic character. The experimenters intended 'to reach the practice-limit and then to try the effect of series-length.' It was supposed that practice would soon cease to increase except in a negligible degree. Ebbinghaus says in *Ueber das Gedächtnis*: 'The experiments fall into two periods; they were made in the years 1879-1880 and 1883-1884 and in each case lasted fully a year. The definite experiments of the first period were preceded during an ample length of time by tentative experiments of a similar kind, so that as regards all the results which are communicated, the time of increasing practice may be considered as *essentially passed over* (*wesentlich überwunden*). At the beginning of the second period care was taken to renew practice.'¹ Moreover, we had the authority of James for supposing that the effect of practice in memorising is exceedingly limited,² and it should be noted that our first experiments were made years before those of Ebert and Meumann, to say nothing of the still more recent experiments of Fracker. As a matter of fact, however, practice never did cease to increase. The experiments of this group always seemed preliminary, and the experimenters went on and on in a somewhat desultory fashion, giving the subjects longer and longer series and at the same time harking back to shorter series to see how well they could be done at the given practice-level. Hence, the 'scattering' of the series-lengths which is especially characteristic of this group of

¹ P. 45. The italics are the present writer's. Müller and Schumann, who made no such extended experiments upon any one subject as Ebbinghaus made upon himself, noted a continuous effect of practice in almost every series of experiments, and found no warrant for the assumption that a subject could be brought to a maximal degree of practice by steady work for a whole semester. Nevertheless, they did not find the effect of practice so great as to prevent them from reckoning the first and second halves of a series of experiments as homogeneous. The different series of experiments were, of course, preceded by enough preliminary experiments to raise practice beyond the levels of its first rapid increase. *Op. cit.*, pp. 327-328. See also pp. 88, 107, and 265.

² *Principles of Psychology*, pp. 663-668. Professor James holds only that one's native retentiveness cannot be improved. Nevertheless, his tone is such as distinctly to discourage memorising drill (stigmatised in one place as 'torture' and in another as 'heroic'), and he has been quoted as an almost classic authority against it.

experiments. The scattering was increased by the desire of the experimenters, in the interests of smell-imagery, to minimise number-associations. The experimenters did not announce the length of the new series, but 'jumped' from one length to another in a random fashion.

So great was the scattering of series-lengths in this group that it is not possible, even by massing in fours, to represent in the tables all the lengths employed. Nor is it possible in the tables which cover this group, Tables I and II, to find space for the mean variations from the average numbers of repetitions. These variations, however, run exactly as they do in the other experiments. Tables III to VI give an adequate notion of their size and irregularity.

b. The experiments of Group II were designed to make good the defect of Group I by comparing the effect of different series-lengths at the same practice-level. To the subjects whose results are massed, the lengths 12 and 18 were given without preliminary practice and according to a compensating program. The subjects who worked throughout the year were given one or two periods of preliminary practice. This training was given in the case of the new subjects, to accustom them to the smell material and to secure some method in their procedure. In G.'s case, it was given to raise the practice-level to that of the preceding year. After this preliminary work, series of two different lengths were given alternately. The length was known to the subject at starting. These series were long and therefore few. If the effect of series-length were to come out clearly, neither of the lengths compared might be so short that the subject could often learn the series with a single presentation. In each case, the subject required an hour and a half to learn the series of greater length. This time was obtained by putting two sittings together. The series were given alternately because it was impossible to work a 'compensating program' into the subjects' schedules.

c. The experiments of Group III, like those of Group I, cover nearly two academic years. The first year of Group III coincides with the second year of Group I, and partakes of the same general character. With the colors as with the smells the

experimenters made the same futile attempt to reach the practice-limit, indulged in the same scattering of series-lengths and made the same attempt to minimise number-associations. The second year of Group III corresponds with the one year of Group II and has the same general character. The experiments of the third sub-group in each period are exactly parallel.

The smell-series of Group II, were, apart from length, easier than the series of Group I, because better precautions were taken to avoid approximate duplicates (see page 26). On the other hand, the color-series of the second year were, apart from length, harder than those of the first. (See page 25.)

2. The subjects in the second sub-groups of experiments were all, except S. and D., members of a second-year class in psychology. S. was a graduate student and the pioneer experimenter in the smell-work. D. was a student in a third-year course, and had already served the year before in the reconstruction-experiments of Group VI. Gr. and R., C. and N. experimented upon each other as well as upon G. The meaning of the term 'beginners' has been explained in the preface.

3. The division of periods can best be discussed table by table. Table I covers all the work done upon the subject G. in these groups of experiments. In Periods I., 1-4, the principal experimenter, Miss Mary C. Smith, and the subject G. were occupied in working out the details of the reconstruction method used in all the later experiments with concrete material. In the first two periods, the subject was allowed to correct her work *ad libitum*, and to smell over again scents which she had already placed. If the first part of a series was reconstructed correctly, then on the next repetition the series was presented only from the point at which the subject's memory failed.¹ In the third period re-smelling was prohibited and the series were repeated always from the beginning. The placing-rules and system of grades were now invented and went into effect in Period 4. The method was then virtually complete. Periods II, 1-3 all fell

¹ This practice was in imitation of the method of complete memorising; it was dropped as needlessly confusing to the subject who had to have the series already pretty clearly in mind to recognize the 'passage' at which the experimenter would begin to repeat.

into the fall term of the second year. During this term the subject was suddenly given some series which were very long in comparison with the series to which she was then accustomed. These series are not taken into the tables (excepting into the second column). They had, however, considerable effect upon the subject's method of procedure. Period 1 covers the time before these series were given; Period 2, the time during which they were given, and Period 3 the time from the end of Period 2 to the Christmas vacation. Between Periods 3 and 4, there was a break of almost two months in the training. Period 4 ran from the early part of February till a change took place in the subject's method which enabled her to cope with long series as she had never been able to deal with them before. Period 5 covers this time of rapid progress. It is dated as beginning April 7 and ending May 7. Period III, 1 was devoted to making good any loss of practice during the second summer vacation, and falls into four sub-periods. The lengths massed in the table were *not* used turn about but in succession. First series of 12 were used; then of 16 and 18; then of 20, 21 and 24; then of 36 and 38 (not included in the tables); and finally of 41. The subject knew at starting the approximate but not always the exact length of the series. Period III, 2 covers the time when the lengths 41 and 81 were given alternately.¹

In two periods in Table I., the grouping of series-lengths is not by fours. One is Period III, 1 where the grouping is according to the practice-levels. The other is Period I, 2 where the grouping is in pairs, 5 with 6, 7 with 8, and so on. Even in this period the subject began to learn the series in groups of four; has done so ever since, and has transferred the same method to the color-and syllable-series.

Table V covers the color-experiments upon G of Group III, Periods I, 1 and 2 are divided from each other arbitrarily to bring out the effect of practice. The first forty series given are included in Period I. Period II, 1 of the color-work is analogous to Period III, 1 of the smell work. The

¹ Such numbers as 31, 41, and 81 were taken for series-lengths in order that we might have such numbers as 30, 40, and 80 for the possible right sequences and positions.

different mean-lengths of the table represent slightly ascending practice-levels. The division between Periods III and IV corresponds to a slight time-break in the experiments.

In Table II, in Table IV, and in that part of Table VI, which covers the work of the subjects He., Hu., and Sm., the division of periods, though not quite arbitrary, is made upon slight grounds which it would be tedious to detail. In Table II the percentage of right sequences is not put down for the first practice period, because in this period the subjects very frequently refused to try to reconstruct the series, after a single presentation, and were not required to try.

In Table III, and in the latter part of Table VI, the division of periods was made according to the series-lengths used.

In the first year of the smell-experiments the subject G. worked at 9:00 a.m. and at 4:30 p.m. on the six weekdays. In the second year, she worked at 9:00 a.m. and 9:00 p.m. on the six weekdays and sometimes, but not always, on Sunday. The Sunday work was interpolated *not* to secure more result but expressly to test the effect of the day's rest. The appointments were kept with notable, though not unbroken, regularity. The sittings lasted from half an hour to an hour. In the third year of the smell-experiments most of the work was done between 2:30 and 4:30 p.m. It was not possible in the color-work of G. nor in any of the work done by other subjects, to keep the hours of the day even approximately the same from day to day, but the hours were kept uniform from week to week.

5. It has already been stated (page 44) that the effect of practice (and of *ennui*) could be considered only from day to day in making up the program of series-lengths to be used. In all cases where series of 20 or over 20 members were used, the program was a simple cyclic or alternating one, if there was a definite program at all. A compensating program for long series cannot be worked into an undergraduate schedule—at least, at Wellesley. In the work of the beginners with series of 12 and 18, a compensating program was carried out. In the smell-experiments of Group I and in the first year of the color-experiments, series of different lengths were given with great irregularity of order. In the work of G., the different lengths

were distributed evenly enough in the several practice-periods. But in the work covered by Table II, and in the work of He., Hu., and Sm. presented in Table VI, the series-lengths were distributed somewhat unevenly in the practice-periods. The experimenter gave rather more shorter series at the beginning and rather more longer series towards the end of the period. Therefore, the different sets of experiments which are supposed to be upon the same practice-level are not absolutely so; the longer series have *very slightly* the advantage.

6. The work of the beginners is massed exactly as if all the series had been obtained from the same subject. The irregularities in the value of N are due to the throwing out of many series.

III. Discussion of Results.

From the tables just discussed, one may draw the following conclusions:

a. *The number of repetitions necessary to a perfect reconstruction (Wp) is extremely small as compared with the number necessary in the method of complete memorising for the perfect recitation of a series of nonsense-syllables.*

With the subject G. in the smell-experiments, the value of Wp never actually exceeds 2.8, even for series of 41 and 81 members. Not all the series of 41 and 81 were ever mastered, but it is clear from the actual values of Wr , Wp , and $RS\%$ when $W = 1$, that all the series given could have been mastered in three or four repetitions, if the experimental sittings had been long enough. In the color experiments, the value of Wp never exceeds 3.3 except for series of 41 and 81. The three series of 81 which were ever perfectly reconstructed give a value of 6.0. At the outset, smell-series of 11 were correctly, though not perfectly, reconstructed with an average of 2.2 repetitions; at the end of the second year, when the subject's practice was at its maximum, series of 30 were correctly reconstructed with an average of 1.8, and perfectly reconstructed with an average of 2.2 repetitions. At the outset of the color-experiments Wr , with series of from 13 to 16 members, is 1.9 and $Wp = 2.1$.

The results of the other subjects show amply that these small

figures do not depend upon any peculiarity of G. Since 12 is a favorite series-length with the German investigators, let the reader look at the results for this length in the first period of Gr., and R. (Table III), of C. and N. (Table VI), and of the various beginners (Table V). Let him look also at the results of Wa. (Table II).

The very small number of repetitions necessary for memorising is the most striking peculiarity in our results. One may compare with the figures given the concise statement of Ebbinghaus: 'Whereas I am able almost without exception to recite series of 6 nonsense-syllables without mistake after a single presentation, I can recite series of 12 syllables (read rapidly) only after from 14 to 16 repetitions, series of 16 only after 30, and series of 36 only after 55.'¹ In the Müller and Schumann experiments, the average number of repetitions necessary to learn for the first time normal series of 12 syllables lies between 10 and 20. The Ebert and Meumann subjects at their maximal degree of practice learned series of 12 syllables by the ordinary procedure in the method of complete memorising with an average of 9.45 repetitions. This ordinary procedure is to read the series through, from beginning to end, with equal intervals between the syllables (the 'G-procedure'). Ebert and Meumann introduced three different variations into the procedure in reading. With the most favorable of these variations, at the maximal degree of practice, the subjects learned series of 12 with an average of 7.25 repetitions. One of the subjects, under these conditions learned with an average of 4.25 repetitions.²

Although the last figures quoted approach our own for the series length 12, yet when one examines the work of our subjects with series of 12 at the very outset of their work and looks, on the other hand, at their work on higher practice-levels with series of 20, 30 and 41 members, one is unavoidably impressed with the small number of repetitions required for learning the series.

b. *The relative largeness of the mean variations is due to the*

¹ *Grundzüge*, p. 651. Cf. *Ueber das Gedächtnis*, p. 64.

² Ebert and Meumann, *op. cit.*, pp. 134, 140, and 142.

relative smallness of the numbers averaged. If one averages 1, 1, 1, 1, and 2, one will get a mean value of 1.20 and a mean variation of .32 which is slightly over a fourth of the average. Yet numbers of repetitions cannot differ from one another by *less* than 1.

c. *The number of repetitions required for a perfect reconstruction (Wp) is not greatly in excess of the number required for a merely correct reconstruction (Wr).* When Wr is from 1.1 to 1.5, Wp never exceeds 1.9; when Wr is from 1.6 to 2.0, Wp exceeds 2.4 in only two cases; when Wr is from 2.1 to 2.5, Wp never exceeds 2.9; when Wr is from 2.5 to 2.9, Wp never exceeds 3.4; when Wr is from 3.0 to 3.3, Wp never exceeds 3.9. Very often the two values differ only by one or two tenths. Their approximation shows that the numbers averaged very often coincide, or, in other words, that series were very often reconstructed perfectly when they were first correctly reconstructed. In general, there is more divergence between the two sets of values in the color- than in the smell-experiments. Compare the results of G. in Table I with those in Table V, the massed results for smells with the massed results for colors in Table IV, and the smell-work of Gr. and R. in Table III with the parallel color-work of C. and N. in Table VI. The explanation of this greater divergence will be sought in the next chapter.

d. *In all the sets of smell-series, with four exceptions, the subject G. learned upon the first presentation at least 60 per cent of the sequences.* These four exceptions are the set of length 81, the first set of length 41, the set with the mean length 26.1 in Period II, 3 and the set with the mean length 23.2 in Period II, 2. That these last two cases are anomalous may be seen by looking at the results for ML 27.8 in Period II, 2, and for ML 23.2 in Period II, 1. In the second set of length 41, the percentage of right sequences on the first trial is 68.4.

e. *The effect of practice is marked with both materials and with all the subjects except D. whose results are not so presented as to bring it out.* To see the effect of practice in the ease of learning the same series-length in different periods, let the reader look again at the results of Gr. and R., Table III; of C. and N., Table

VI and of the beginners, Table IV. R., for example, was given 78 series of length 12. With the first 39, $RS\%$ ($W = 1$) was 59, Wr was 2.3 and Wp 2.4. With the second 39, $RS\% = 79$, $Wr = 1.5$ and $Wp = 1.6$.

With G. practice is less evident with the colors than with the smells, and is obscured in passing from Period I, 2 to Period II, 1 by increase in the difficulty of the series.

f. *In the case of the longer series practice affects the number of right sequences obtained at the first trial more than it affects the number of repetitions necessary to a correct reconstruction.* See the smell-work of G., Table I, Periods III, 1 and 2, length 41; the color-work of G., Table V, Periods III, 2 and 3, lengths 41 and 81; the color-work of C., Table VI, Periods 3 and 4, lengths 21, 31, and 41; and the work of the beginners, Table IV, for the smell- and color-series of the length 18 as compared with those of the length 12. By 'the longer series' are meant series which are long for the practice-levels at which they are given. Neither the rule nor its reverse applies consistently in the case of the shorter series.

g. *Within the limits of these experiments practice was unbounded in the direction of enabling the subject to memorise longer and longer series with a very few presentations.* At the outset of the smell-work G. correctly reconstructed 71 series of length 11 with an average of 2.2 presentations; in the latter part of the third year, she reconstructed 13 series of the length 41 correctly with exactly the same average number of presentations. See also the work of D. Just how many presentations would have been necessary at the outset to enable G. and D. to learn series of 41 can now never be known. It simply did not occur to us to give them such long series at the outset. It would probably, however, have been impracticable to deal with such series in the limits of the longest possible experimental sittings. This fact is shown by the results of certain experiments upon beginners with series of the length 25. These results will be quoted in the next chapter.

The effect of practice in our experiments cannot be said to be greater or less than its effect in the experiments of Ebert and Meumann or of Fracker. Our experiments are not comparable

with these others. Ebert and Meumann tested the effect of practice in learning series of nonsense-syllables upon their subject's facility in learning a variety of material. This material was for the most part verbal but included series of meaningless characters (made, for example, by combining straight lines and semi-circles). The memory of the subjects was tested before, between, and after the two practice-periods. Fracker tested the effect of practice in learning the order of four tones upon his subjects' ability to memorise various kinds of material, some of it concrete but none of it olfactory. The reconstruction method was not used even with the concrete material. The order of the concrete series-members was stated by the subjects in verbal symbols, mostly numbers. The memory of the subjects was tested before and after the training-experiments. The results both of Ebert and Meumann and of Fracker seem to show that facility acquired in learning one sort of material may be transferred to the learning of other kinds of material, whatever may be the nature of this facility and its physiological explanation. Our own experiments, on the other hand, were not originally designed to bring out the effect of practice, and the groups of experiments treated in this chapter did not happen to be so arranged as to bear at all upon the transfer of practice. The subject G. began the color-experiments a full year after the smell-experiments. She did not at the outset memorise the color-series as well as she had memorised the first smell-series, and never learned color-series as well as she could learn smell-series of the same length. However, this difficulty of the color-series for G. may have been so great as more than to cancel the effect of training with the smell-series. Hu. began to work with the colors three months later than with the smells. The subject had much difficulty in distinguishing the odors, and this fact is sufficient to explain her greater facility in learning the colors. None of the other subjects of these groups worked with both sorts of material.

h. The effect of practice is not evanescent. With G. the practice-level in the smell-work did not fall perceptibly during the summer vacation between Periods I, 4 and II, 1, nor during the two months pause between Periods II, 3 and II, 4. In fact,

the subject memorised considerably better in Period II, 4 than in Period II, 3 although the intervening pause was filled in part by a rather severe illness. The work of D., which is shown in Table III, was done after a pause of seven months. Yet it evidently is upon a high practice-level.¹

i. *In the direction of bringing longer and longer series well within the one-repetition span, the effect of practice is apparently not unbounded.* For all series-lengths from 12 up, practice seems to end with reducing the value of Wr to 1 or 2 with some tenths. Even with the subject G., the lengths 12, 16 and 20 could never uniformly be memorised with one repetition.

j. *On the higher practice-levels the difference in the numbers of repetitions necessary to the correct and to the perfect reconstruction of series of different lengths is less than proportional to the difference in length.* See the work of G. with smell-and color-series of the lengths 41 and 81 in Tables I and V, the work of D. in Table III, and the work of C. and N. with color-series of 21, 31, and 41 in Table VI. On the lower practice-levels the differences in Wr and Wp are almost exactly proportional to difference in length. We find this relation in general maintained in the work of the beginners, Table IV.² However, in the first sets of smell-series, even in the work of the beginners, we find the difference in Wr and Wp less than proportional to the difference in length. This is probably because in this period the figures largely represent the work of the better memorisers. (The values of N , Nr and Np show that many series were never correctly or perfectly reconstructed.) The differences in Wr and Wp are never more than proportional to the difference in length except in those sets of experiments in which series differing in length, and long enough to create difficulty at the subject's practice-level, were given to the subject without announcement of their length. Such sets of experiments belong only to

¹ Upon the lastingness of the effect of practice, compare Ebert and Meumann, *op. cit.*, pp. 193-196. See also Müller and Schumann, *op. cit.*, p. 328.

² For example, in the first sets of color-series, Wr is 2.2 for length 12 and 3.4 for length 18. Wp is 2.6 for length 12 and 3.9 for length 18. The real lengths of the series to be learned were 11 and 17 respectively (see p. 47). We have, therefore, the following proportions: 11:17 :: 1.00 : 1.55; 2.2: 3.4 :: 1.00 : 1.55; 2.6 : 3.9 :: 1.00 : 1.50.

the first two years of the investigation (see pp. 70-71). Most instances of the disproportion under discussion are found in the work of G., Table I, Periods II, 1-3. The disproportion is, in all cases, very slight.

k. *At all practice-levels, the subjects nearly always learned at the first presentation as many sequences out of the longer as out of the shorter series.* This fact may be seen from the tables by comparing the percentages of right sequences for different lengths, at the same practice-level. The fact, however, can be brought out more clearly by figures which are not contained in the tables.

l. *No exact correlation can be made out between the number of repetitions necessary to correct reconstruction (Wr) and the percentage of right sequences upon the first trial, ($RS\%$, $W = 1$).* When Wr is below 1.4, $RS\%$ (when $W = 1$) is never below 84 per cent and in the case of G. it is never below 88 per cent; when Wr is below 1.7, $RS\%$ is never below 75 per cent except in one set of experiments; when Wr is below 2.1, $RS\%$ is with one or two exceptions, never below 60 per cent. No closer relation can be observed. Striking instances of the lack of correlation may be seen in the work of D., Table III and C. and N., Table VI. For example in the work of D., for the length 31, $RS\% = 80.1$ and $Wr = 2.0$, whereas for length 41, $RS\% = 64.7$ and Wr again = 2.0. The lack of correlation is undoubtedly due in large measure to the fact that the subjects exerted themselves more upon the second trial when they knew that they had accomplished little at the first, whereas they were often reckless upon the second trial when they knew that they had virtually mastered the series upon the first.¹ When a number of repetitions is large, such variations of attention are of relatively little importance, but when the number is 2, 3, or 4, 'moral causes' may dislocate all the natural relations of the numerical results. Another reason for the lack of correlation will be suggested in the next section.

¹ Of course, the subjects were not told how well or ill they had done at each trial, but they themselves knew how far a given reconstruction had been a matter of guess-work.

At least upon the higher practice-levels, differences in series-length affect the numbers of sequences obtained in the first trial more than they affect the numbers of necessary repetitions. This fact shows that the learning process is not in general a very gradual one, or, in other words, that the values of Wr are not swelled by a large number of reconstructions of which each contains only a small amount of error. In this connection it may be remarked that practice does not consistently affect Wp more or less than it affects Wr .

B. Discussion of Results Not Represented in Tables I-VI.

The most important data which can be added to the information in the tables are as follows:

I. *There is no sharp and sudden limit to the length of the series which the subject can sometimes reconstruct correctly or perfectly after a single presentation.* This fact may be illustrated by the following statements: Some smell-series of the length 12 were given the subject G. upon nearly all of the higher practice-levels. Even in Period III, 1, she succeeded in reconstructing only 21 series out of 27 correctly and 19 out of 27 perfectly after one presentation.¹ Yet this same subject could often reconstruct much longer smell-series correctly and even perfectly at the first trial. For example, in Period II, 4 out of 32 series of lengths from 25 to 28, 13 were reconstructed correctly and 6 perfectly at the first trial. Out of 25 series of lengths from 29 to 32, 3 were reconstructed both correctly and perfectly at the first trial. In Period II, 5 out of 12 series of the lengths from 29 to 32, 3 were reconstructed correctly and 2 perfectly at the first trial. In Period III, 2 out of 14 series of the length 41, 2 were reconstructed correctly and 1 perfectly at the first trial. The subject D., out of 35 series of the length 31, reconstructed 9 correctly and 1 perfectly on the first trial. No other subject had sufficient practice to perform such feats as did G. and D. Nevertheless, the fact that any subjects could occasionally achieve them shows that our experiments deal with a kind of

¹ These results cannot be explained by a possible fall in the subject's practice-level during the summer vacation. Similar results were obtained with small groups of series of the lengths 12 and 16 in the middle of the second year.

memorising which is very different from learning to recite in succession rapidly presented nonsense-syllables.

II. *Subjects with no practice whatever can in general reproduce after a single presentation the serial order of not more than four smells and five colors, even when the initial unit is given.* This assertion is based upon the results of the last subgroup of experiments in the second and third groups. These experiments were made by a combination method, a cross between the memory-span method and our own reconstruction method. The subjects were given first longer and longer series and then shorter and shorter series. Each ascending and descending step was of one unit. Each series was presented but once, whether it was mastered at the first presentation or not. The results were massed, so that each 'case' in the statement given below is one series with one subject. The series used with the different subjects were for the most part the same, and were carefully arranged to avoid the presence of virtual duplicates in the same series and to secure a variety of units from series to series. As many series were given at a sitting as the time allowed. The subjects were beginners. In the color-experiments, the subjects numbered 43 and in the smell-experiments 35. In these two sets of subjects 26 were the same persons. The results are contained in the following table:

TABLE VII
Smell-Series
Subjects: Beginners. W = 1

<i>L</i>		3	4	5	6	7	8	9	9	8	7	6	5	4	3
<i>N</i>		35	35	35	35	35	35	35	35	35	35	35	35	35	35
Series correctly reconstructed	No.	34	28	14	14	7	6	2	3	3	5	12	27	23	35
	Per cent	97	80	40	40	20	17	6	9	9	14	34	77	66	100

TABLE VII—Continued
Color-Series

<i>L</i>	5	6	7	8	9	10	10	9	8	7	6	5	
<i>N</i>	43	43	43	43	43	43	43	43	43	43	43	43	
Series correctly reconstructed	No.	39	20	17	7	7	7	8	10	15	20	25	28*
	Per cent	91	47	40	16	16	16	19	23	35	47	58	65

*This result was due to haste at the end of the last experimental sitting.

In regard to these figures, it should at once be stated that the results for the longest series represent in general the work of different subjects and not of the same persons. Thus, the 21 cases obtained with the first color-series of 8, 9 and 10 members represent the work of 17 different subjects, and in no case did the same subject memorise all these series. In the light of this statement it is clear that the subjects did not come suddenly upon a limit to the number of smell- and color-units whose order they could memorise with a single presentation.

The figures have, farther, a twofold significance. In the first place they show that the number of color- and smell-units whose order could be memorised by unpracticed subjects with a single slow presentation was small in comparison with the numbers of nonsense-syllables, monosyllabic words and figures which educated adults can reproduce without special practice after a single rapid presentation. Ebbinghaus states the number of nonsense-syllables as 6 or 7, the number of monosyllabic words as 8 or 9 and the number of figures as from 10 to 12.¹ In the second place, the figures show that the effect of practice, in memorising the order of smells and colors slowly presented, must be enormous. Before practice the number of units whose order can in general be memorised by one presentation falls far short of the figures just quoted from Ebbinghaus. After practice, the case is violently reversed.

III. *The percentages of series in any given set which can be correctly and perfectly reproduced after one presentation—%SCR*

¹ Ebbinghaus, *Grundzüge*, pp. 649-650.

and %SPR—are correlated only in the roughest fashion with the numbers of repetitions necessary to correct and to perfect reconstruction (Wr and Wp) and with the percentage of right sequences obtained upon the first trial, ($RS\%$, $W = 1$). For example, we have such combinations as these below. The illustration is taken from the smell-work of G. in Period II, 4.

ML.	N.	RS%, $W = 1$.	Wr .	Wp .	% SCR.	% SPR.
19.3	12	93.4	1.3	1.9	83.3	50.0
23.6	26	81.8	1.8	1.9	34.6	30.8
26.6	32	76.2	1.8	2.4	40.6	18.8

IV. *The most frequent fault in reconstructions which were correct without being perfect was the changing about of series-members.* The placing-rules about 'making and leaving gaps' gave relatively little trouble even when the subjects were blindfolded. However, in the first two years of G.'s smell-work this error in placing was more common than transposition, a fact which shows that blindfolding was at first an appreciable handicap to this subject.

V. *The percentage of right positions obtained upon the first trial in general, but by no means always, exceeds the percentage of right sequences.* In the case of G., the positions are in excess in most sets of smell-series of the first year, in all sets of smell-series of the third year, and in all sets of color-series. In the smell-series of the second year, however, the sequences are very frequently in excess. In Period II, 3, for example, we have the following percentages:

ML.	N.	RS%, $W = 1$.	RP%.
15.3	12	91.8	92.4
19.0	21	79.9	79.6
22.8	21	77.0	75.9
26.1	11	43.5	39.5

These figures illustrate the small differences ordinarily found between the numbers of right sequences and right positions. However, even a small excess in sequences is noteworthy for the

reason stated on page 48. In the case of the other subjects, out of the sets of series included in the tables, the sequences are in excess of the positions only in seven altogether.

In general, when the series are relatively hard for the subject to learn, the positions tend considerably to exceed the sequences. For example, we find this relation in the longest color-series given to the subject G. (Table VI). In Period II, 2, $L\ 4I$, $RS\% = 23.6$ whereas $RP\% = 40.4$; in the same period, $L\ 8I$, $RS\% = 11.0$ whereas $RP\% = 22.9$; in Period II, 3, $L\ 4I$, $RS\% = 30.7$ whereas $RP\% = 43.3$; in the same period, $L\ 8I$, $RS\% = 11.3$ whereas $RP\% = 31.3$. In the smell-series of $L\ 8I$ given to D., Period II, 2 (Table III), $RS\% = 27.0$ and $RP\% = 40.5$. The positions also considerably exceed the sequences in the work of the beginners included in Table V. The excess is greater in the series of 18 than it is in the series of 12.

VI. *In general, the subjects tended to learn a larger absolute number of positions in the longer series, although, as already noted, they tended to learn about the same absolute number of sequences out of series of different lengths. The most compact illustration of this fact is found in the following results which were obtained from the beginners:*

Smells

Period.	L.	N.	AVERAGE NUMBER PER SERIES OF	
			Sequences.	Positions.
I	12	88	4.7	5.7
I	18	91	4.6	6.1
2	12	44	7.5	8.5
2	18	39	8.4	9.8

Colors

I	12	118	5.5	6.4
I	18	118	5.6	6.9
2	12	105	7.6	8.5
2	18	105	7.5	9.5
3	12	72	8.3	8.9
3	18	72	8.6	10.9

VII. *The number of positions learned at the first presentation is not remarkably large, small as is the number of repetitions necessary for complete memorising.* We may fairly compare the massed results of our own fifteen beginners, for color-series of 18 members, with the massed results of the five subjects of Münsterberg and Bigham for color series of 20 members. The percentage of right positions for our subjects was 51.9 which implies a percentage of 48.1 in errors of position. The corresponding percentage of error for their subjects was only 28.4. Our series of 18 were really shorter by three units than their series of 20 (see page 47). Our colors were presented one to about every $3\frac{1}{2}$ seconds; theirs were presented one to every two seconds. It is impossible to tell from the report given just how many series composed entirely of colors were learned by each of their subjects. The experimenters sought 'to avoid any disturbance by mere training.'¹ The present writer traces the large excess of error on the part of our subjects to two causes. In the first place our color-series were much harder to learn. Our series were drawn by lot from a set of 136 colors, many of them much alike. Münsterberg and Bigham used only 20 colors in all (so the writer infers) and could well afford to have them all easily distinguishable. In the second place our subjects learned enough series of the same kind to suffer from 'confusion,' as the effect of 'associative' and 'reproductive inhibition' is popularly called. Each of our subjects learned on the average $19\frac{2}{3}$ series of 18 colors and $19\frac{2}{3}$ series of 12 colors. The mutually counteracting effects of practice and inhibition will be discussed in the next chapter (page 137 squ.).

Briefly expressed, the most important general conclusion from the numerical results of this chapter is as follows: *We are dealing in our experiments with a form of memorising in which practice tends to reduce the number of necessary repetitions to the same number for longer and longer series. This number is surprisingly small.*

Our results in regard to series-length are diametrically opposed to the conclusions reached by Ebbinghaus with his

¹ Page 36.

differing material and method. The following passage is freely translated: "The alteration in one's ability to reproduce is surprising when the number of series-members exceeds even by a little the maximal number which one can just master with a single presentation. . . . We should expect to find ourselves remembering about as many links as we can invariably memorise ('span, *umspannen*') in the case of shorter series, and forgetting the rest, but what we do find is that our incapacity for the greater performance damages our capacity for the smaller and that the number of members retained after a single presentation is greatly reduced. For example, out of series of even twelve nonsense-syllables, one can often repeat only the first and last members. Often in the case of longer series not even these members will 'adhere.' If the reproduction of the whole series is required, it is necessary to pile up the repetitions, and this number of repetitions needed increases with surprising rapidity as the series length increases." The figures quoted upon page 76 follow these statements. Ebbinghaus then adds the data obtained by Binet and Henri from the arithmetical prodigy Dimandi, who could learn 10 figures in 17 seconds, but required $2\frac{1}{4}$ minutes to learn 20, 25 minutes to learn 100, and 75 minutes to learn 200. In speaking of the limits of 'immediate memory,' Ebbinghaus remarks: "For the same individual under the same circumstances, the figures show a prettily sharp line of demarcation. I myself, for example, can perfectly reproduce 6 nonsense-syllables practically without exception, whereas I can only seldom succeed in repeating eight without mistakes. . . . When bodily development is complete, the figures remain almost constant; in my case, they have not altered in more than 20 years."¹

So far as the writer knows, the effect of series-length in memorising has scarcely been investigated apart from the individual study made by Ebbinghaus in *Ueber das Gedächtnis* and apart from experiments in immediate memory.

The respects in which our results differ from those of Ebbinghaus are three: In the first place the number of repetitions necessary to memorise did not increase with surprising rapidity as

¹ Ebbinghaus, *Grundzüge*, pp. 650-651.

the series-length increased. On the contrary, it increased with surprising slowness. In the second place, the subjects were in general able to remember after one presentation of the longer series quite as many sequences and rather more positions than they retained after one presentation of the shorter series. In the third place, there is no sharp limit to the length of the series which the subject can reproduce after a single presentation. Without anticipating the discussion of Chapter VI, it is evident that in our experiments we have nothing to do with the limits of immediate memory or with its span.

CHAPTER V.

THE MENTAL PROCESSES INVOLVED IN MEMORISING SERIAL ORDER.

The mental processes involved in memorising and in reproducing the serial order of smells and of colors presented slowly differ radically from the processes involved in memorising and reciting series of words and nonsense-syllables presented rapidly. This fact is evident from the statements of the last few pages. The purpose of this chapter is to scrutinise the mental processes of our subjects with the help of control-experiments and of introspective data.

It is at least theoretically possible that the peculiarities of our results were due to the concrete (non-verbal) nature of the material. The truth of this supposition has been rendered somewhat improbable by the work of other experimenters which seems to show that non-verbal material is relatively difficult to memorise. Münsterberg and Bigham found that the average percentage of error in the reconstruction of series of 20 colors was 28.4 as compared with a percentage of 18.0 in the reconstruction of series of numerals under the same experimental conditions.¹ Ebert and Meumann found that the learning of meaningless figures, such as combinations of dashes and semicircles, was much harder for their subjects than the learning of series of nonsense-syllables. On the first practice-level, 2.11 readings per syllable were required, on the average, for learning series of 12 nonsense-syllables and 3.83 exposures per figure for learning series of 12 figures. The corresponding figures for the second and third practice-levels were .83 and 2.33, .48 and .90.²

¹ *Op. cit.*, p. 37. For purposes of memorising, numbers may of course be considered as verbal material.

² Ebert and Meumann, *op. cit.*, pp. 48-49, 178 and 182. The *Erlernungsmethode*, not the reconstruction method, was used. The writers do not state exactly *how* the subjects were required to reproduce the marks. Verbal description, logical analysis, mnemonic devices, and drawing with the fingers were all forbidden in the learning.

None of these experiments bear upon the effect of differences in series-length. The experiments of Münsterberg and Bigham were not extensive and did not include a comparison of colors and nonsense-syllables with the same test-method.¹ Thus, we may be pardoned alike for making and for discussing our fourth group of experiments whose primary purpose was to test the relative difficulty of memorising the serial order of smells, colors and nonsense-syllables under the same conditions.

*A. Numerical Results of Experiments in Memorising in Turn
the Serial Order of Smells, Colors, and
Nonsense-Syllables.*

I. Tables of Results with Explanation and Discussion.

a. Tables VIII and IX.

See pages 92-93.

b. Explanation of Tables.

1. The abbreviations in these tables are precisely the same as those used in Tables I to VI (see pp. 59 and 66) with the addition of the abbreviation N.S. for nonsense-syllables.

2. The arrangement of the numerical data in the tables is also practically the same (cf. pp. 66-68), although the Roman numerals in the period-column for the subject G. in Table VIII require special comment. The experiments represented in the two tables were all made in the same year, 1904-1905, but this year was for G. the fourth year of the experiments with smell-material, the third year of the experiments with color-material, and the first year of experiments with nonsense-syllables. In the periods marked with the Arabic 1 the series-lengths massed were 14 and 16, on the one hand, and 19 and 20, on the other.

3. In explaining the tables, it remains only to give an account of the different subgroups of experiments: In Subgroup 1,

¹ In the later experiments of Bigham (Psych. Rev. i, pp. 453-461) colors and nonsense-syllables were brought into comparison, but the color-series were reconstructed and the syllable-series reproduced. It was easier to reconstruct the color-series than to reproduce the syllable-series.

TABLE VIII
Experiments of Group IV. Subgroup I

SUBJECT.	MATERIAL.	PERIOD.	NUMBER OF R S POSSIBLE.	ML	N	Nr	Wr	Np	Wp	RS % W=1	ML	N	Nr	Wr	Np	Wp	RS % W=1
G.	Smells Colors N. S.	IV, 1 III, 1 I, 1	401 433 434	14	10	9	1.8	9	2.0	81	19.9	7	7	1.7	6	2.3	76
				14.2	11	11	1.3	10	1.6	86	19.9	7	7	2.1	7	2.1	65
				14.2	11	11	1.7	11	2.0	78	20	7	7	1.7	7	2.0	68
Bo.	Smells Colors N. S.	IV, 2 III, 2 I, 2	520 480 600	41	7	4	2.3	4	2.3	74	81	3	0	0	27
				41	6	5	2.8	2	3.0	39	81	3	0	0	8
				41	9	9	2.3	8	2.6	55	81	3	3	4.0	0	27
	Smells Colors N. S.	1 1 1	320 320 320	9	12	12	2.1	12	3.3	63	17	12	7	4.3	2	6.5	25
				9	12	12	1.5	12	2.2	80	17	12	12	3.9	12	4.5	35
				9	12	12	2.0	12	2.4	65	17	12	12	2.7	12	3.2	37
	Smells Colors N. S.	2 2 2	208 416 416	9	6	5	2.0	5	3.0	67	17	10	8	3.3	7	3.4	36
				17	8	8	2.4	8	3.1	45	33	9	7	5.0	5	6.2	13
				17	8	7	2.3	7	2.6	32	33	9	7	3.9	6	4.3	15
Kl.	Smells Colors N. S.	1 1 1	232 237 237	9	7	7	3.4	6	6.2	45	17	7	3	3.7	1	6.0	21
				9	7	7	1.4	7	1.9	77	17	8	8	2.3	8	2.4	56
				9	7	7	1.6	7	1.9	77	17	8	8	2.4	8	2.9	48
	Smells Colors N. S.	2 2 2	240 464 464	9	10	10	1.5	10	1.8	68	17	10	3	3.3	0	28
				17	9	9	1.6	9	1.9	75	33	10	10	3.9	8	4.5	17
				17	9	9	1.4	9	1.9	88	33	10	9	2.7	9	3.1	37
Wo.	Smells Colors N. S.	1 1 1	210 210 210	9	7	7	1.3	7	1.6	91	17	7	7	2.0	7	2.1	52
				9	7	7	1.3	7	1.4	86	17	7	7	1.7	7	2.0	72
				9	7	7	1.1	7	1.3	93	17	7	7	2.6	7	2.6	30

TABLE IX
Experiments of Group IV. Subgroup 2.

Experiments of Group IV. Subgroup 2.													Subjects: 15 beginners															
MATERIAL.	PERIOD.	NUMBER OF RS POSSIBLE	$R.S$ % $W=1$													$R.S$ % $W=1$												
			L	N	Nr	Wr	MV	Np	Wp	MV	Np	Wp	L	N	Nr	Wr	MV	Np	Wp	MV	Np	Wp						
Smells..... Colors..... N. S.....	I	1132	12	38	26	3.5	1.4	23	3.4	1.4	28	18	42	15	4.0	1.3	15	4.1	1.3	19								
	I	1171	12	40	40	2.6	1.0	39	2.9	0.9	56	18	43	35	3.9	0.9	33	4.5	1.0	25								
	I	1177	12	39	34	2.5	0.9	33	2.7	0.9	54	18	44	44	3.5	1.1	40	3.8	1.2	31								
Smells..... Colors..... N. S.....	2	481	12	19	13	1.8	0.6	12	2.0	0.6	50	18	16	13	2.7	0.6	12	2.8	0.4	42								
	2	543	12	20	20	1.7	0.9	19	1.9	0.9	73	18	19	19	2.7	0.7	19	3.2	0.7	46								
	2	543	12	20	20	2.0	0.9	19	1.9	0.8	72	18	19	19	2.8	0.6	18	2.9	0.7	44								
Smells..... Colors..... N. S.....	I and 2	1613	12	57	39	2.9	0.9	35	2.9	1.1	35	18	58	28	3.4	1.1	27	3.5	1.1	26								
	I and 2	1714	12	60	60	2.3	1.0	58	2.6	1.2	62	18	62	54	3.5	1.3	52	4.0	1.3	32								
	I and 2	1720	12	59	54	2.3	0.9	52	2.4	0.9	60	18	63	63	3.3	1.0	58	3.5	1.0	35								

represented in Table VII, the subjects were G., Bo., Kl. and Wo. Bo. was a graduate student in a third-year course in psychology; Kl. was a graduate student in a second-year course, and Wo. an undergraduate in a second-year course. In Subgroup 2 both subjects and experimenters were beginners.

In the case of G., Bo., Kl. and Wo., the experiments extended from October to May, but were interrupted in G.'s case by control-experiments. G. served six times a week. As in the earlier work, two sittings were put together when series of 81 members were given. Bo., Kl. and Wo. worked each three or four times a week as subject and once a week as experimenter upon G. The beginners worked toward the end of the academic year and in general four times a week for four weeks. In the case of all the subjects, even of G., some of the work was done in the morning and some in the afternoon, but the hours were the same from week to week.

In Subgroup 1, the division of periods is according to the length of the series used. For example, in the case of Bo., the time during which series of 9 and 17 members were used in alternation with all three materials constitutes the first period. In Subgroup 2, the series of Period 1 includes the first three series of each kind given to each subject. Irregularities in the values of N are created by the throwing out of series. The massing of the beginner's work was done exactly as the massing was done in the third subgroups of Groups II and III. (Cf. p. 75.)

In Subgroup 1, the series of different lengths were given in a cyclic but not in a compensating order. The order prescribed was as follows: "shorter syllable-series, shorter color-series, shorter smell-series, longer syllable-series, longer color-series, longer smell-series, and repeat." In Subgroup 2, the order was compensating as regards the arrangement of lengths, but merely cyclic as regards the arrangement of materials. The prescribed order was as follows:

$$a = N.S. 12, C. 12, S. 12$$

$$b = N.S. 18, C. 18, S. 18$$

$a, b, b, a; b, a, a, b; b, a, a, b; a, b, b, a$, and so on. (See page 45.) In both groups the regularity of the program was

somewhat broken by accident. The subjects knew the length of each series before it was given. As already stated (p. 24) the syllable-series were of the hit-or-miss variety.

C. Discussion of Results.

Conclusions which may be drawn from the tables are as follows:

1. *The serial order of the nonsense-syllables slowly presented is quite as easy to learn as the serial order of smells and colors slowly presented.* To the subject G., the color-series were distinctly harder than either of the other kinds. In learning the shorter series of smells and nonsense-syllables about the same number of repetitions was necessary, but upon the first presentation more smell-sequences were learned than syllable-sequences. Nevertheless, in the very few series of 81 which were given, the nonsense-syllables had distinctly the advantage.¹

For the subject Bo. the smell-series were much the hardest; in the shortest series the colors had the advantage over the syllables, but in the longer series the syllables had, on the whole, the advantage over the colors. The case of Kl. was much the same. The smell-series were most difficult; the colors had somewhat the advantage among the shorter series; the syllables had decidedly the advantage among the longer series. With both Bo. and Kl. the results for the two sets of length 17 are inconsistent. The figures look as if with Bo. practice had more effect in the case of colors, and with Kl. more effect in the case of the syllables, but the numbers of series in the sets are so small that the difference may be accidental. In the case of Wo. there was much 'scattering' in the series-lengths given so that but few of the results obtained can be represented in the table. The results which do appear in the table were introduced to exhibit the exceptionally low values of Wr and Wp and the high values of RS obtained by this subject. Wo. was one of the subjects who showed greatest facility in memorising. To Wo., the smell-series after slight practice were distinctly the easiest,

¹ The nine series of 81 represent 18 experimental sittings. The series in each set are relatively few in this group of experiments because three different sorts of material were used.

although the table does not show this fact. Results which do illustrate it are these: Five series of 65 smells, 5 series of 65 syllables and 4 series of 65 colors were given. No values for Wr or Wp were obtained. The values of $RS\%$ ($W = 1$) were 23.1 for the smells, 6.6 for the colors and 6.3 for the syllables. Whether the colors or the syllables were harder for this subject cannot be decided.

The beginners, at the lower practice-level, found the smells much the most difficult material, and the syllables slightly harder than the colors. At the higher practice-level, the differences are remarkably small, except that among the shorter smell-series relatively few sequences were learned. Wr is, however, lower for the smells than for the syllables. Among the shorter series the colors are slightly the easiest; among the longer series the smells are perhaps slightly the easiest.

Inconsistent as these results seem, it is not impossible to interpret them. We may suppose that the material which best catches and holds the attention is easiest to memorise except in so far as the units are hard to discriminate. Smells have most intrinsic interest for some subjects; colors for others; syllables for none. The concrete series, moreover, present more variety than is presented by the syllables. Hence, in the case of the shorter series, in which relatively few difficulties of discrimination can in any case occur, either smells or colors tend to have the advantage. Smells, however, are much harder than colors for most untrained subjects to discriminate; hence, among the shorter series, the colors are generally left in the lead. It should be noted that G., the trained subject, has an exceptionally good discrimination for smells and an exceptionally poor discrimination for colors, when colors are presented in succession and at a considerable interval from one another. In the case of the longer series, on the other hand, those series are apt to take the lead which involve fewest difficulties in discrimination. Now, a color-series of any considerable length is sure to contain some units which are hard to discriminate. Hence, the longer the series, the more the syllables gain and the colors lose. The syllables, however hard to memorise, are always perfectly easy to identify ('tell apart'). Whether this

interpretation be valid or not, the fact remains that the numbers of repetitions necessary for the correct and perfect reconstruction of the syllable-series were of about the same magnitude as the number of repetitions required in the case of the smells and colors and that the slight fluctuation in the relative values was due to individual differences.

2. *With the new subjects practice had most effect in the smell experiments, and next in the color experiments, yet its effect in the syllable-experiments is marked.* The work of Kl. is an exception to this statement. Here practice had most effect in the case of the syllables and very little in the case of the longer smell-series. However, the number of series compared is very small. With all the subjects, improvement in the case of the smells is almost undoubtedly in large part an improvement in discrimination rather than in memorising.

3. *The results of G. seem to show that practice gained in the earlier experiments with smells and colors was transferred to the learning of the nonsense-syllables.* It is noteworthy that the three series of 81 syllables were all correctly reconstructed after the fourth presentation.

4. *In general, the syllable-series show, with increase in series-length, less increase in numbers of repetitions necessary to correct and to perfect reconstruction than is shown by either the smell-series or the color-series.* Taking the tables as a whole, it is evident that in the case of the easier series—i. e., of the series in which even the larger values of Wr and Wp are small—the difference in the values of Wr and Wp for different series-lengths is less than proportional to the difference in series-length, but that in the case of the harder series it is either roughly proportional or rather more than proportional.¹

In general, in these experiments, difference in series-length made more difference in the numbers of necessary repetitions (both in Wr and Wp) than it made in the first three groups of experiments (the groups described in the last chapter). This

¹ The smell-series given to the beginners and to Kl. in the first period seem to constitute exceptions to the rule just stated. However, if one examines the values of N and Np one sees that the exceptions are of no importance. Evidently only the easiest of the longer series attained to correct reconstruction at all.

divergence in the results seems to be due to the fact that the subjects in the group of experiments with which we are now dealing had very little practice with any one sort of material. (This statement is not to be interpreted as telling against the transferability of practice except in so far as practice consists in the accurate sensing of the material to be memorised. Naturally, training in the discrimination of smells cannot be transferred to colors.) It is noticeable that it was precisely in the smell- and color-series, in which considerable training in discrimination is required, that the increase of difficulty with series-length was greatest.

5. *In general, the number of repetitions necessary to a perfect reconstruction approximates the number necessary to a merely correct reconstruction in the case of the nonsense-syllables rather more closely than in the case of the colors but scarcely more closely than in the case of the smells.* There is great irregularity in the relative values of Wr and Wp . On the whole, however, it is evident that the mere discrimination-difficulty in the case of the concrete material may be easily over-rated except as it may distract attention from the order of the links. For the sake of brevity a statement may be added here which is not derived from the tables and which, therefore, properly belongs in the next subsection. Correct reconstructions were reckoned imperfect on account of the transposition of members as often in the case of the syllables as in the cases of the smells and colors. This fact shows that such transpositions are in large part due to a tentative method of reconstructing the series and are not wholly due to mistakes in the identity of series-members.

6. *In this group of experiments the subjects, in many cases, learned on the first presentation a number of sequences out of the longer series which was smaller absolutely than the number learned out of the shorter series given at the same level.* This appears from the fact that the difference between the percentages of right sequences is often more than proportional to the difference in series-length. The disproportion does not hold for the sets of series which were easier for the individual subjects. Curiously enough, it does not hold for the sets of smell-series given to the beginners in either period. In the second

period, the difference is distinctly less than proportional to the difference in the length of the series. The ratios given on page 000 show that also in the smell-experiments upon the beginners in Group II the difference between the numbers of right sequences was, in the second period, less than proportional. This peculiarity in the smell-experiments may not be accidental.

II. Discussion of Results Not Represented in Tables VIII and IX.

The following conclusions may be drawn from data not included in the tables:

a. In every set of series, with three exceptions, the percentage of right positions obtained at the first trial exceeded the percentage of right sequences. The three exceptions are the smell-series of the lengths 14 and 81 learned by the subject G., and the color-series of 18 members learned by the beginners in the second period. The first two exceptions are of no importance owing to the small numbers of series in the sets and the slightness of the excess.

b. In the harder series, the excess of positions is much more marked than in the shorter.

c. In some cases, not quite so large an absolute number of positions was learned out of longer series given at any one practice-level as out of the shorter. (Compare pages 81 and 86.)

These last two conclusions and also the last conclusion of the preceding subsection (page 98) may be illustrated from the work of the beginners. The average number of sequences and positions learned per series were as follows:¹

¹ The statements of the two preceding paragraphs are not illustrated as well by these figures as by the results of Bo. and Kl. These figures are chosen because the numbers of series represented are better worth considering.

<i>Period.</i>	<i>Material.</i>	<i>L.</i>	<i>N.</i>	<i>Sequences.</i>	<i>Positions.</i>
1	Smells	12	38	3.1	4.1
1	"	18	42	3.2	4.2
2	"	12	19	5.5	6.9
2	"	18	16	7.2	7.3
1	Colors	12	40	6.1	6.8
1	"	18	43	4.3	5.2
2	"	12	20	8.1	8.6
2	"	18	19	7.8	7.4
1	N.S.	12	39	5.9	6.2
1	"	18	44	5.3	6.0
2	"	12	20	8.0	8.2
2	"	18	19	7.5	7.9

As regards the conclusion lettered *b*, the figures show that the positions exceed the sequences (1) much more decidedly in the smell-series, ruling out those of length 18 in Period 2, than in the color- and syllable-series; (2) rather more in Period 1 than in Period 2, and in Period 1 rather more in the longer series than in the shorter. It may be seen from Table VI that the smell-series of length 18 given in Period 2 proved as easy as the color- and syllables-series of the same length. Therefore the smallness of the excess in positions is not surprising when the smells are compared with the colors and syllables. This small excess in positions in the case of the smell-series of length 18 and the actual deficit in positions in the case of the corresponding color-series of length 18 do, however, appear anomalous when the longer and the shorter series of this period are compared. In explanation, one can only say that irregularities must be expected when the numbers of series are so small and when the series themselves vary so much in difficulty. At least, the general trend of the first four groups of experiments is to show that the subjects grasped more at notions of absolute position whenever the task was hard, whether the difficulty was due to the material, to the length of the series, or to want of practice. Moreover, the results of the fifth group of experiments show beyond question that the tendency to learn 'by positions' was

no stronger in the case of the visible series at large than in the case of the smells.

One may now turn back to the work of the beginners for illustrations of the conclusions lettered *c* and of the last conclusion of the preceding subsection. One finds in the case of the longer color- and syllable-series as compared with the shorter, some falling off in the absolute number of sequences and positions learned. The drop, however, is very slight and affects the sequences more than the positions. It does not exist at all in the case of the smell-series (cf. page 99). It affects the colors much more than the nonsense-syllables.

It may be added that, in the results of the better trained subjects taken together, one finds a considerable number of instances where the sequences drop considerably with increase of series-length and yet the positions rise. For example, in the work of Bo., Period 1, in the 12 smell-series of length 9, 5.0 sequences and 5.8 positions were learned on the average, whereas in the 12 series of length 17, 3.9 sequences and 6.2 positions were learned. The results of the corresponding color-series are parallel. In the syllable-series, on the other hand, more sequences as well as more positions were learned in the longer series.

In brief, one may conclude from the statements of this section that as judged by their results the mental processes involved in memorising serial order are not essentially different, whether the material be smells, colors, or nonsense-syllables, primarily verbal or non-verbal.

This conclusion does not conflict with the findings of (1) Münsterberg and Bigham or of (2) Ebert and Meumann. (1) Münsterberg found numerals much easier to memorise than colors. But numerals are particularly easy to memorise on account of their tendency to combine into unit-groups and the ease with which they are apprehended. (2) Ebert and Meumann found meaningless combinations of lines harder to memorise than verbal material. But such figures are peculiarly hard to memorise for a reason which will be stated later. Here it is important to notice that the mere fact of being verbal

does not put the nonsense-syllables at a marked disadvantage nor give them a marked advantage over the concrete material.

B. Interpretation of the Numerical by the Introspective Results of the Reconstruction Method.

The purpose of this section is to interpret, as far as possible, the numerical results of the reconstruction experiments by means of the introspective observations which were made in connection with them. As a preliminary to the discussion of the introspective data, the writer wishes to make a specific classification of the kinds of associations which may coöperate in the memorising of series (or serial order). This classification may be schematised as follows:

ASSOCIATIONS.

- A Primary = Principal (*Hauptassoziationen*).¹
 - I. Direct intrinsic.
 - a. Progressive. b. Regressive.
 - II. Extrinsic, viz: spatial or numerical associations.
- B Secondary = Supplementary (*Nebenassoziationen*).¹
 - I. Indirect intrinsic ("criss-cross").
 - a. Progressive. b. Regressive.
 - II. Extrinsic, viz: auxiliary, mnemonic.
 - a. Aids to attention. b. Aids to linkage.
- C Representative associations.
 - Extrinsic.

Principal associations are those in virtue of which taken alone (or merely with certain essential representative associations) the subject is able to reproduce the series in the manner required. When the test of mastery is the recitation of a series, in order, from beginning to end, then the only principal associations are those proceeding from each link to that next succeeding (the direct 'progressive' associations) and those running between

¹The terms *Hauptassoziationen* and *Nebenassoziationen* are appropriated from Ebbinghaus. Cf. *Grundzüge*, p. 664.

each link and the idea of its absolute place in the series.¹ In the common form of the methods of right associates also, these two associations only can be considered primary. When, however, the test of knowing the series is the reconstruction of the order—when, strictly speaking, only mastery of the order is tested—then the associations running out from each link to the link next preceding, the direct regressive associations, are quite as important as the direct progressive. These three kinds of association, therefore —(I, *a*) the direct progressive, (I, *b*) the direct regressive, and (II) the associations running from the series-members to notions of absolute position and back again—will here be considered as principal or primary.² Associations between members of the series will be called *intrinsic* associations; associations which run outside the series will be called *extrinsic*.

Under supplementary associations are included (I) the associations between members of the series which are not in juxtaposition, the indirect intrinsic associations, and (II) the 'aids' (*Hilfen*) or auxiliary associations already mentioned. The auxiliary associations may be subdivided, according to Ephrussi,³ into such associations with particular links as cause the attention to dwell on them singly,—'aids for the attention,'—and associations which serve as actual roundabout connections between members of the series—'aids to linkage.'

¹ Ebbinghaus considers only the direct progressive association, 'which far surpasses all others in strength,' as 'principal,' but according to the definition of principal associations given by the present writer, place or number associations must also be included, because it is theoretically possible for the subject to reproduce the series by attaching each unit to the corresponding number idea. (Of course, when it is said that these may be the only associations which come into play, the fact that the number ideas are themselves associated, two with one, three with two, and so on is disregarded.) Ephrussi considers the association of a link either with the link next following or with its place in the series as strictly mechanical memorising, i. e., as memorising without 'aids' of any sort. (*Op. cit.*, p. 16.)

² It is noteworthy that the position associations found in learning a series must function backward if they assist in reciting. If a subject says to himself *lig—three* in learning a series of syllables, he must say *three—lig* if the position notion helps him in recalling it.

³ *Op. cit.*, p. 77. The aids or *Hilfen* are divided according to their effect into *Aufmerksamkeitshilfen* and *assoziative Hilfen*.

Representative associations are those which proceed from the members of the series to images which, in the subject's review of the series, are or may be substituted for the links themselves—which act as 'counters' for the links. In the case of the smells, such counters are absolutely necessary to memorising since, as we shall see, true smell-images are at best somewhat rare phenomena. The counters *are* the series in so far as the series is recalled, and since the counters are the series, the links between the counters themselves must be reckoned sometimes as principal and sometimes as supplementary, although the associations between the series-members and the counters themselves are representative. In memorising verbal material representative associations cannot be said to occur (unless indeed the visual, auditory, and kinæsthetic factors in the percept or image of a word be counted as three separate mental contents). The representative associations differ widely from the ordinary auxiliary associations in this way, that whereas the auxiliary associations seem to lapse, as the learning of the series progresses, yet some one representative image for each link persists and every link tends to acquire its own counter.¹

Let us take, by way of illustration, a smell-series of which the first five members are rose, tar, caraway, ginger and almond. Then the associations directly connecting caraway with ginger, caraway with tar, and caraway with the word *three* are all primary whether the experiences of ginger, tar and caraway be smell-percepts, smell-images, word-images, or visual images of gingercake, tar-barrel, and sugared caraway seeds. The associations between the smell of caraway and either the word *caraway*, or the mental picture of pink, white, and yellow 'seeds' if so stereotyped as to serve as a symbol, are representative. The associations running from caraway to rose and from caraway, through the word or picture *cookie*, to ginger are supplementary. The cookie-association is auxiliary and is an aid to the linkage of caraway and ginger. An association of tar with images of a tar-spotted white pinafore and an angry mother may be an 'aid to attention.'

¹ See page 126.

The proper purpose of this half-chapter is the statement of the introspective data obtained in connection with the reconstruction experiments. This statement falls into two divisions. The first division bears mainly upon the subjects' method in memorising, and is based in large part upon written answers to specific questions. The second division contains an analysis, partly introspective and partly inferential, of the factors which determine the difficulty of memorising the three sorts of material. The introspective matter of this second section is furnished largely, though not wholly, by the writer, the subject G. Some of the more important statements of the subjects have been checked by sets of control-experiments. The results of these experiments will be given not by themselves in a separate section, but in direct connection with the introspective declarations upon which they bear.

I. The following are the chief points of interest in regard to the subjects' method of memorising: (*a*) the character of the images which represented the links in the series, (*b*) the kind of associations which bound together the links in the series, (*c*) the degree and manner in which the series-members were divided into unit-groups, and (*d*) the nature of the subjects' consciousness of practice. Lists of questions to be answered from introspection were issued to the subjects at or near the end of each series of experiments. In reporting the data thus obtained, the statements of G., the representative subject, will be noted first under each heading. It does not seem, in general, necessary to discuss the process of memorising apart from the process of reconstruction.

a. In the case of G., the images which represented the smells were in no case olfactory. Pictorial representatives—such as the visual image of a sprig of pennyroyal for the smell pennyroyal—were scanty and schematic. The smells were represented in a somewhat stereotyped fashion by their names (or by the names of other smells for which the subject mistook them.) G. had the advantage over all the other subjects in being able to name with considerable accuracy any smell in the list. For purposes of memorising, to be sure, it makes no

difference whether the smells are named properly or not, provided that the subject is contented with the names he gives, always names the same smells in the same way, and does not apply the same name to more smells than one. However, if the subject must depend largely on verbal images, a real poverty in names is a great handicap.

For G. every verbal image is highly composite, containing three elements, tactile, auditory and visual. The visual image is not, however, an image of a written or printed word, but is a color of synæsthetic origin. A color-image is so inherent in every verbal percept or image that it can scarcely be called an associated image in the ordinary sense. When a word is read, an auditory image, a kinæsthetic image, and a color-image fuse with the visual presentation. When a word is heard or remembered, only the auditory, kinæsthetic and color-elements are present. The color-image entirely displaces the proper visual image of the printed or written word which can be evoked only piece-meal, indistinctly and with difficulty. (This subject can never visualise more than one printed letter at a time.) The word-colors are consistent and well saturated, but are not sharply outlined or very nicely differentiated from one another, although they are sometimes parti-colored. Word-images which are the same in color are differentiated by their auditory and tactile elements. In some cases, the word-color of a smell-name is modified in actual dealing with the scent by the known color of the smell-substance. For example, the word *spearmint* is pink but the spot of color which stands for the smell is pink and green. In memorising, the importance of the color-images is this: The subject projected them into spatial relations so that 'in imagination' they formed a line or lines (see page 121) of ill-defined oblongs running from left to right, in the order of the series, either in a darkish space and at a convenient reading-level and distance from the subject's eyes,¹ or upon the

¹ When G. is in daylight and not blindfolded, the word-colors are not seen on a dark background, but they do usually seem to be seen at reading-distance and level. When the subject is actually reading, the colors flicker, as it were, about the written or printed letters and yet they do not seem to be on the paper. In reading black characters the word-colors are brought out vividly only by stress of attention to the sounds represented, but they are always present except when

vaguely imaged table. When the series was presented, the line of colors was gradually formed. In reconstructing the series, the subject read off fragments of the line of color-symbols with the aid of the auditory and tactile word-symbols, and represented the order with the bottles somewhat as a piano-player reads the notes 'with his fingers.' The comparison is rough, for the musician reads straight across the page, whereas G. read a fragment, now here now there, as the scents were taken up in chance order and suggested their neighbors. Only a small part of the line—a bit of the left part, right part, or center—was ever seen at one time. The subject's color imagery is vivid but is never complex. When questions of spacing were considered, the bottles themselves were imaged vaguely as standing in various places on the table, but, curiously enough, they were almost never imaged as having contents of different colors. They were often pictured as empty, and the space in which they were arranged was quite as clearly tactile as visual.

Two short series of experiments were made to control the subjects' assertions that she memorised the series in words, and largely by the aid of the name-colors. If these statements are true, then the serial order of the smell-names should be memorised at least as easily as the order of the smells themselves, and the order of words which are more highly colored than the smell-names should be memorised still more easily.

The first set of control-experiments was made in the spring of 1904. Three kinds of series were given: (1) series of smell-names, (2) series of English Christian names, and (3) series of words ending in the suffix *ation*. Of all words, such Christian names as have been familiar to the subject from childhood are most deeply colored. *Robert*, for example, is a full carmine

attention is focussed upon the actual black lines. The letter *S*, in so far as it consists of two curves, is black, but its sibilant sound is almost as saturated a salmon pink as a salmon pink geranium, though darker than the flower. Word-colors, however, for some unknown reason, are often quite different from the color of the component letters. *George*, for example, is the color of a heliotrope blossom although it contains not a single letter which verges on violet. With this subject pseudochromæsthesia was well developed in childhood and is undoubtedly hereditary. Word-colors never amount to actual hallucinations, but in fatigue colored hypnogogic phenomena are sometimes intrusive.

red and *Julia* is a lustrous purple. On the other hand, 'abstract' words, especially those acquired after early childhood, ordinarily have least color. *Unification*, for example, begins with a bright dash of green, but trails off practically into a dingy streak. Each list of words numbered 128, like the list of scents which was in use at just that time. The normal reconstruction method was employed. The words were written on the kind of cards used in the reconstruction experiments with nonsense-syllables. So many different series-lengths were used that it is impossible to present the results both precisely and compactly. Adopting the rough expedient of massing the results for the various series-lengths, we obtain the following values:

<i>For the</i>	<i>N.</i>	<i>Nr.</i>	<i>Wr.</i>	<i>MV.</i>	<i>Np.</i>	<i>Wp.</i>	<i>MV.</i>
Smell names.....	14	14	1.9	0.5	11	2.3	0.5
Christian names.....	9	9	1.2	0.3	7	1.4	0.5
Common nouns.....	9	8	1.5	0.5	7	1.9	0.2

The series-lengths were the same for the three kinds of words but ranged from 8 to 41.¹ Though the number of series is small, yet the figures seem worth giving because the mean variation is so very small in view of the variation in series-length. The experiments were, however, in a measure vitiated by the wealth of auxiliary associations which cropped up in connection with the nouns in *ation* and with the Christian names. Furthermore, as the subject herself planned the experiments, her attention was drawn strongly to the word-color, so that even the long common nouns with their uniform endings were pretty strongly colored. But at least the results of the experiments do not contradict the statement which they were designed to test.

The second lot of control-experiments, which was made in the spring of 1906, was combined with another lot whose purpose does not concern us here. Seven sets of experiments were made: (1) Experiments in which series of smells were presented and reconstructed in the ordinary manner with the subject

¹ In each of the nine lengths used one series of Christian names and one of proper names was given. In five of the nine lengths, two series of smell-names were given; in the other four, one only.

blindfolded; (2) experiments in which series of smells were presented and reconstructed in the ordinary way except for the removal of the eye-bandage; (3) experiments in which smell-names (on the ordinary cards) were presented and reconstructed; (4) experiments in which series of smells were presented in the ordinary way but reconstructed in terms of the name-cards as representatives; (5) experiments in which series of name-cards were presented, but in which the reconstruction was done in terms of the smells represented by the names; (6) experiments in which series of colors were presented and reconstructed in the ordinary way, and (7) experiments in which series of alternate colors and smells were presented and reconstructed. The last two kinds of experiments may, for the moment, be left out of account. The normal reconstruction method was used. The series-length was 41. We have the following values in the different sets of experiments:

<i>For the</i>	<i>N.</i>	<i>Nr.</i>	<i>Wr.</i>	<i>MV.</i>	<i>Np.</i>	<i>Wp.</i>	<i>MV.</i>
First set.....	3	1	2.0	0
Second Set.....	3	3	2.3	0.4	2	2.0	0
Third set.....	3	3	2.3	0.4	2	2.5	0.5
Fourth set.....	3	3	2.0	0	3	2.0	0
Fifth set.....	3	3	2.3	0.4	1	2.0

The length 41 was chosen that the values of *Wr* and *Wp* might be large enough to exhibit differences. Therefore, the number of series given was necessarily small, for two series could not be mastered at a sitting of three quarters of an hour.¹ With so small a number of series one can conclude from the figures merely that at the practice-level at which this subject had arrived, it made very little difference whether she was dealing with the actual scents or with their names.

In brief, one may conclude that G. memorised the smells in terms of representative images which were at once verbal and capable of a definite spatial arrangement.

¹In explanation of the values of *Np* the reader is referred back to p. 67. This table illustrates the fact that when *Np* falls short of *Nr*, *Wp* may fall short of *Wr*.

G. remembered the colors mainly in the form of color-images. These images, however, were poorly differentiated and were eked out with color-words, which must be classed as representative images. Representative images, however, were much less important in the case of the colors than in the case of the smells. On the one hand they were less essential, and on the other they were less adequate. They were less essential because even in the case of G., whose color-discrimination is very poor, color-imagery is vastly better developed than is concrete smell-imagery. They were less adequate because popular color names are not sufficiently differentiated to describe 136 different colors and technical color-names are difficult to apply. The subject was perfectly familiar with the Milton Bradley system of names and symbols, but had too bad an absolute memory for color to make much use of it. 'It was easier,' wrote the subject, 'to try to remember the colors out and out than to think out names for them.' Concrete representative images scarcely occurred in the case of the colors. When a square of colored paper suggested the image of another object with the same color, this image was practically always an auxiliary image (*Hilfe*) and fell away as memorising proceeded to completion.

G. visualised the syllables as oblong spots of color. In the case of the smells, these color-images must be counted as representative, since they seem to have attached originally to the names and to have been transferred from the names to the smells themselves. On the other hand, the pseudochromæsthetic images of the syllables, however explicable genetically, must be considered as parts of the units of the series, not as extraneous additions. Their union with the visual presentations and with the unfailing auditory and tactile representations is by fusion, and not by association, except in the widest sense of that term.

The results obtained from the other subjects will now be presented. Fairly satisfactory introspective records are in existence for 45 out of the 46 beginners who made relatively extended experiments with smell-series,¹ including 19 beginners

¹ The memory-span experiments are not included. No introspective records were obtained in connection with them.

who experimented with mixed smell and color series in certain control experiments (see pages 142-144 below). Out of these 45 subjects, 18 explicitly disclaim ever having true smell-images; 6 describe the process of memorising without any reference to smell-images; 15 assert that they have true smell-images in the case of some few scents, either very familiar, such as peppermint, or very disagreeable, such as pyridin, but ascribe little importance in memorising to these images; and only 6 claim that they found smell-images distinctly serviceable in memorising. In regard to this claim the writer is extremely skeptical. This skepticism does not grow merely out of the disinclination which one has to believe in anything which is not a part of one's own experience. It is based partly on the results of certain control-experiments (not to be described here) which were made in connection with the questionnaire investigation mentioned on page 2, and partly on the fact that one of the most intelligent of just these six beginners makes a remark which discredits the claim in question. But whether this claim is based on sound introspection or not, it is significant that only 13.3 per cent of the beginners ascribed much importance in memorising to smell-images. Of these 6 beginners only one claims that she memorised wholly in terms of olfactory images; 3 of the 6 acknowledge that they memorised the smells to some extent by names (or descriptive words) and 2 that they often thought of the smells in part by their numbers in the particular series. Among the 45 subjects considered, including these 6, 40 subjects memorised the smells more or less largely by name and 3 by number (using the series-numbers more or less as names). Only 1 subject used chiefly concrete visual images to represent the smells, but seven others report that concrete visual images were useful.

As to the adequacy of the names used, the following data may be presented: All the subjects were required at the end of the experiments to name the list of smells used, giving the name which they had used in the experiments if they could not give the right name. The following statistics refer only to the 83 selected scents referred to on page 15. The records of 18 representative subjects are considered, making a total of

1494 (18×83) attempts at naming. In 26.9 per cent out of the 1494 cases approximately the right name was applied; in 23.5 per cent a name not correct, yet the name of a smell really similar from the olfactory point of view (as e.g. *cloves* for *cassia*); in 21.6 per cent a correct class-name (as e.g. *spice* for *cloves*) or a descriptive adjective; in 3.9 per cent a name explicable by similarity of associations (as e. g. *ether* for *carbolic*), or of secondary concomitants (such as pungency); in 0.8 per cent a color-name perhaps indicative of pseudochromæsthesia; in 0.1 per cent the number on the bottle which indicated the material (and which must have been learned from the experimenter); in 14.9 per cent a name inexplicable by the writer; and only in 8.2 per cent no name at all.

All of the better trained subjects who have left satisfactory records claim to have memorised the smells mainly by name. No records exist for Ha., R. or Wa. All except He. and Hu. asserted that they sometimes had or 'could get' smell-images. Only Kl. (who memorised the smells very badly) claims that the images were an assistance in memorising the series. D., whose achievements have been noted on page 82, named about half the smells correctly.

Satisfactory records exist for 48 of the 49 beginners who made relatively extended experiments with color-series (or mixed smell- and color-series); of these 48 subjects 29 claim that they memorised the colors both in terms of concrete color images and of color words. Of these 29, 4 found the concrete imagery more important than the verbal, and 11 found the verbal imagery more important than the concrete, although 1 of these 11 remarks that this relation tended to be reversed with practice. Out of the 48 subjects considered in all, 12 report almost purely concrete and 7 almost purely verbal imagery. In one case the concrete imagery was largely representative. The subject purposely visualised trees, flowers, and shrubs as placed in a row to represent the colors.

Satisfactory records exist for only 6 out of the 8 better trained subjects who worked with colors. With C. and N. both verbal and concrete imagery were important. With C. the imagery became increasingly verbal. The imagery of Kl. was predomi-

nantly and the imagery of Bo. almost purely concrete. On the other hand, the imagery of Wo. was almost purely verbal.

The terms in which the syllables were remembered may be very briefly discussed. The images of the syllables were necessarily verbal, and this study is not concerned with the sense-type of verbal images, but merely with the existence of true olfactory images, and with the degree to which the concrete material was visualised in verbal terms. Of the 18 relatively untrained subjects, counting Bo., Kl. and Wo. with the 15 beginners, all except Bo. claim that they visualised the syllables. Three report imagery which was exclusively visual and 4 assert that the visual imagery was of paramount importance. Only Bo. and one other subject claim that auditory or tactile imagery was more important than visual. No synæsthetic imagery was reported.

It is impossible to correlate the character of the imagery with the facility of the different subjects in memorising. The amounts of practice vary too much in the different cases and other experimental conditions are too rough.¹ The point at issue here is simply the comparison of the images which represented the smells, colors and syllables. In view of the introspective reports as to the terms in which the three sorts of material were memorised, it is not at all surprising that the nonsense-syllables gave approximately the same results as the concrete material. *For, on the one hand, the concrete material was imaged largely in words, and on the other hand, the syllable-cards were quite as palpable as the color-squares.* Thus, the syllables were given a certain concreteness by the demands of the reconstruction method and became, as it were, names for cards which could be handled and laid in particular spots on the table. Nevertheless, some difference really exists between the processes of memorising the colors and the smells, on the one hand, and the syllables on the other. Neither the smells nor the colors are differentiated for any subject simply by their names. The experience

¹ The figures at their face-value seem to show that the subjects who depended most on concrete imagery learned worst. If this is true, it is, of course, because the concrete images are ill-differentiated.

which the subject lives through when a series of syllables is presented is, so to speak, single-strand as compared with the double-or triple-strand experience usually produced by a series of colors or smells. The smells themselves, as presented, form one strand, their names may form another, and the visual images of objects associated with them another. These second and third strands are entangled with one another and either may be broken but the one or the other is usually present. When the subject reviews the series, he follows these two subsidiary strands, and not the first, which for the most part slips from his grasp. If both the second and third strands break, he can as a rule, review the series no farther. In the case of the colors also most subjects have a fairly complete strand of names which is the more essential the worse the visual memory may be. Now this duplication of threads involves a disadvantage and an advantage. The disadvantage lies in the fact that until the names for the smells and colors have become stereotyped and the representative associations rapid, attention may be distracted from the connection of the links by the search for 'counters.' The advantage is that just in virtue of their representative associations the members of concrete series may be fringed with an extra set of aids. Carbolic and chloroform may as smells suggest the connecting link 'hospital' and the subject may also note the sequence of two initial *cs* in the names. Again the actual colors red-orange (almost a 'brick-red') and black would scarcely in themselves suggest 'Princeton' but the name-sequence might well do so. The writer is inclined to think that although the necessity of naming the smells and colors is a disadvantage to the untrained subject, the habit of noting *both* the series-member and its name is an advantage to the trained subject. It seems to the writer, from experience, that the peculiar difficulty of memorising meaningless characters lies in the facts (1) that the percepts are so purely—and, as it were, flatly—visual, without any auditory or kinæsthetic supplements (unless one is allowed to outline them with one's fingers) and (2) that compact verbal counters for them are hard to invent. On the other hand the writer believes that words, quite apart from the more artificial auxiliary associations and from

from their tendency to combine into groups, would be easier to memorise than nonsense syllables because with words so many concrete images go hand in hand. Here again the series-strand is double.

In brief, representative associations and complex images seem to be advantages in memorising. The smells and colors, however, had counterbalancing disadvantages which will be explained below.

b. In considering the manner in which the subjects linked together the members of the series, one should note particularly (1) the nature of the primary associations, and (2) the degree to which the subjects used auxiliary associations (*Hilfen*).

1. G. asserted that she 'memorised almost equally in terms of sequences and of positions.' The numerical results bear out this contention. (See page 85). As each series-member was presented, the subject made two or three closely successive but distinct efforts. She tried to visualise at least one sequence—i. e., the given member with its predecessor, and if possible in the later presentations, with its successor—and to visualise in connection with the given series-member either its number-word or its spot on the table or both. The efforts to visualise smell- and syllable-sequences were all directed toward word-color. To the auditory and tactile phases of the word-image little attention was consciously directed. The subject was distinctly conscious of the clinching together of series-members described on page 42 as a possible result of the slow rate of presentation. When a unit-group was visualised, one or two members would be clear and the rest slurred as if out of the visual focus. Except for occasional attempts to visualise a unit-group as a whole, G. did not review, between exposures, any sequence but the last. Attempts at reviewing while a presentation was in progress rarely paid, although the subject could sometimes safely anticipate sequences. The attention of this subject is of the stable type, and cannot be jerked from one content to another without a total break-down.¹

¹ With this subject any attempt at immediate self-observation in memorising ends in prompt disaster, parallel to that of the centipede in the familiar rhyme.

Number-words were always important. G. paid no strict attention to counting the series-members one by one in words, yet she was always greatly disturbed by any conscious miscount, since wrong counting necessitated the redistribution of the series-members in the groups presently to be described. Moreover, the color of the number-word was often compared with the color of the paper-square or the color which represented a smell or syllable. Tansy is represented by a green spot. The word *nine* is green and the same color flickers about the Arabic (not the Roman!) symbol. Thus the occurrence of tansy in the ninth place could not escape observation.

In the earlier smell-experiments, G. did not systematically associate the smells with particular places on the table. As a methodical practice, the making of these table-associations for the smells developed at the end of Period II, 4 and distinguished Period II, 5 from this earlier period. (See page 73.) At the end of Period II, 4 the subject had been working with the colors, once a week, for about four months. From the first, she associated the colors (which she could see spread out before her in the process of reconstruction) with spots on the table. Probably, this method of spatial projection was transferred from the colors to the smells. At all events, the subject was conscious in Period II, 5 of a great increase in facility in memorising the smells, and ascribed this increase to her newly acquired system of attaching the smells as presented to images of the spots on the table where they should stand when placed by her in the order of presentation. Although the subject was blindfolded, the place associations were both tactile and visual. Her own median line was the line of orientation and any shift in her position was confusing, as she measured distances by reaches. The visual place-notions were schematic. The subject represented herself by a dark line. This system of spatial-projection, when once developed, was found useful, was carried throughout the smell-experiments and over into the reconstruction experiments with syllables, and has strongly affected the subject's procedure in memorising by other methods.

All the series were arranged by G. from left to right. If the series of smells exceeded 36 in number, the scent bottles in

excess were placed in a row behind the others, that is, farther away from the subject. The color-squares were arranged in rows of twelve and the syllable-cards in rows of twenty, the numbers which could lie without over-lapping in one row across the table used. In the case of the colors and syllables, however, the earlier members of the series were placed farther away from the subject than the later. This difference in arrangement between the smell series and the others was not made deliberately. Long series were used first in the case of the smells, and the length of the row was determined mainly by the subject's convenience in reaching the ends. When the series exceeded 36, it seemed 'natural to slip the extra bottles in behind the others.' On the other hand, it seemed equally 'natural' to this subject to arrange the series which she could see as the words run upon a printed page. When a book lies flat upon a table, the top lines of the page are farther away from the reader's body. This subject would not have been able to alter these arrangements without great, if temporary, confusion.

The subject G. claimed that she not only as a rule memorised the series-members themselves—in their nature or content, so to speak—in *learning* the order, but that she memorised them *in order to learn* the order. In the spring of 1905, at the end of the fourth group of experiments, control-experiments were made to test the subject's assertion that she memorised not merely the places but the nature of the series-members. Six sets of experiments were made, experiments of the first four sets in turn, and experiments of the last two sets later, in alternation. These experiments were as follows: (1) series of nonsense-syllables (hit-or-miss variety) were presented and reconstructed in the ordinary way; (2) series of nonsense-syllables were given in the ordinary way but were reproduced (not merely reconstructed) by the subject in order in writing; (3) series of smells were presented and reconstructed in the ordinary way; (4) series of smells were presented in the ordinary way but were named by the subject in order in writing; (5) series of nonsense-syllables were named to the subject at intervals of 5 seconds, and were reproduced in writing; (6) series of smells were named and reproduced in the same way. The series were all

of the length 41. In the written reproductions the subject was not limited as to time but any correction or interpolation was counted as equivalent to breaking a placing-rule. Thus in these series also a distinction was made between a merely correct and a perfect reproduction. We have the following values in the different kinds of experiments:

For the		<i>N.</i>	<i>Nr.</i>	<i>Wr.</i>	<i>MV.</i>	<i>Np.</i>	<i>Wp.</i>	<i>MV.</i>	
PRESENTATION,	Written	{ (1) N.S. reconstructed.....	14	13	2.1	0.2	12	2.2	0.3
		{ (2) N.S. reproduced.....	14	11	2.7	0.5	9	2.8	0.5
		{ (3) Smells reconstructed.....	13	9	1.9	0.4	3	2.0	0
		{ (4) Smells reproduced.....	13	13	2.3	0.4	6	2.5	0.5
	Oral	{ (5) N.S. reproduced.....	10	10	3.0	0.2	7	3.0	0.3
		{ (6) Smells reproduced.....	10	10	2.1	0.2	8	2.1	0.2

These figures seem to bear out the subject's assertion. It was somewhat harder for her to reproduce the series verbally *in toto* than to reconstruct the order, but the difference in the numbers of repetitions necessary is surprisingly small. Reproducing the syllables was both absolutely and relatively harder than reproducing the smell-names, but this fact is entirely explicable by the difficulty of apprehending the syllables in such a way as to insure accurate images of them. It was especially hard to apprehend the syllables in the fifth set of experiments, not because the subject is dominantly eye-minded, but because the experimenters read badly. At this time, the subject had had almost no practice in memorising syllables except by the reconstruction method, and none of the experimenters had had practice in reading series aloud. It took longer to deal with the scent-bottles than with the syllable-cards; hence, the relatively small values of *Np* in the third and fourth sets of experiments.

Satisfactory introspective records exist for 41 different beginners who served as subjects in the reconstruction experiments, not counting the experiments with mixed series which must be considered apart as regards the matter now under discussion. Of these 41 subjects, 12 memorised almost exclusively by noting sequences, 18 almost exclusively by noting positions, and 11

by noting both or sometimes one and some times the other. Of these 11, 5 laid more stress on sequences and 3 on positions. Of the remaining 3, 2 memorised the syllables 'by sequence' and the smells 'by position,' 1 of the 2 memorising the colors by sequence and the other memorising them by position. Sixteen of the 29 subjects who memorised to a considerable extent 'by position' have left records as to terms of the position-idea. Six noted the numbers of the series-members, 8 associated the series-members with definite spots on the table, and 2 did both. Taking the beginners' records as a whole, one cannot maintain that the kind of primary association formed (sequence or position-number or place-association) depends upon the nature of the material used.

Of the better trained subjects who have left satisfactory records, F. and Hu. memorised almost entirely by sequence (learning in both cases 'a string' of smell names); E., He., Gr., C., and Bo. memorised almost entirely by position, and D., N., Kl., and Wo. memorised both by sequence and position. Associations with spots on the table were important to E., N. Kl. and Wo. Of these four subjects only Wo. showed marked facility in memorising.

One may make one remark in regard to the apparent advantages of the different methods. No subject who memorised concrete series-members by sequence only showed any facility in memorising. Learning by sequence only seems to be the poorest method. Perhaps one may hazard the conclusion that in the matter of sequences *versus* positions the eclectic method of G., D. and Wo. is the best.

Nearly all the subjects other than G. stated that they were accustomed to review the series, more or less, while a presentation was in progress. Reviewing seems to have been more common than anticipation. Some subjects who memorised by sequences seem to have proceeded in the style of 'This is the House that Jack Built.'

2. The subject G. did not use auxiliary associations systematically, but was accustomed to grasp at them in difficulties—especially if fatigued, confused or working under distraction. She was distinctly less dependent on them than were most of the

other subjects. These associations rarely amounted to the elaborate sentence- or story-type of mnemonic device and were more often aids to attention than aids to linkage. The only device used habitually was the assignment of certain smells and colors to stand for particular relatives and acquaintances. When the subject felt particularly well, happy, and 'detached,' auxiliary associations cropped up luxuriantly 'of their own accord' but accomplished little since, under these apparently favorable conditions, the subject was likely to place some of the units thoughtlessly. The criss-cross associations referred to on page 57 were especially important with G. on account of her method of grouping (see page 121 below).

In discussing the question of auxiliary associations, the records of the 19 beginners who worked with the mixed series may be added to those of the 41 beginners who worked with unmixed series. Of the 60 different beginners thus taken into account, 32 used auxiliary associations to a large extent and 26 to a very small extent, if at all. In other words, there were 53.3 per cent of 'ingenious' and 43.3 per cent of 'mechanical' memorisers. The other two subjects were among those who worked with all three sorts of material. One of them reports that she turned the syllables into sentences but used no 'aids' with the colors and smells. The other used aids with the colors, but not with the syllables and makes no report in regard to the smells. Good specimens of auxiliary associations are the connecting of black and pink by the 'red eyes of a mourner' and green and pale yellow by 'the green grass a hunting-dog runs over and the corn-meal mush he is fed on.' Considering that the series were presented very slowly and that the beginners were not forbidden to use these aids, the percentage which did use them does not seem large.

Of the better trained subjects only D., Gr., and Wo. appear from the records to have been of the ingenious type. The writer remembers, however, that the constant habit of Ha. was to connect the smell-names by a 'story.'

At the end of this discussion of the methods of linkage, it seems proper to sum up the answer to the question in regard to

the way in which the subjects occupied the relatively long intervals between the moment when they apprehended a series-member and the beginning of the next exposure. (See pages 11 and 42.) It has just been shown that the formation of auxiliary associations was by no means a universal habit. It has also been shown that G. was accustomed to note absolute positions and to image the sequences to which the last presented member belonged; if the member were *d* to image *c—d* and if the presentation were not the first to image or to try to image *d—e*. G. did not review earlier sequences; if she had any attention to spare she anticipated sequences. On the other hand, most of the other subjects did at least some reviewing while a presentation was in progress. This reviewing had two consequences. In the first place, criss-cross associations must have been strengthened by the dragging forward (so to speak) of earlier series-members into temporal juxtapositions with later ones. In the second place, each presentation of the series amounted to more than a single traversing of each sequence. The very worst inaccuracy of our method lies in the fact that we cannot determine just how many times a given sequence was traversed by a subject during any single presentation of the series. It seems probable that laborious reviewing is incompatible with the careful and complex clinching of sequences and of position-notions upon which G. relied. Examination of the introspective records seems to bear out this conjecture, but the data are too rough to be thrown into statistical form.

c. From almost the beginning of the experiments, the subject G. grouped the series-units in fours. (She had long been accustomed to count by fours any sort of units in a row.) In the case of the smells, not only were the first and second and the third and fourth units paired off by rapid visualization, but often also the first and fourth and the second and third, a pairing which reminded the subject continually of the means and extremes of an arithmetical proportion. The effort to see the group-units as 'wholes' often consisted just in this rapid pairing. In the case of long smell- and color-series, these group-units of four were united into lines of twelve which seemed to

constitute natural divisions of the series each with 'a center and two wings.' In the lines of color-spots which represented all three sorts of series there were no blank spaces between groups. The subject seemed to herself to group the images by the kind of broken eye-movement which, in counting, groups the stitches in a row of plain knitting or the palings in a fence.

Of the 41 different beginners who served as subjects in the experiments with unmixed series, 24 claim that they broke up the series into unit-groups of uniform length (or of uniform length as far as the total series-length permitted). Of these 24, the writer believes that some half dozen misunderstood the question. (See, moreover, page 128 below on the doubtful spontaneity of the grouping in many cases.) Five subjects deny that they did any grouping at all. The other 12 report the habitual selection of certain striking members of the series as 'pillars' or landmarks and the attaching of other members to these supports or centers.

Of the better trained subjects all but D. and C. formed unit-groups of the same length. D. and C. grouped by setting up landmarks at certain fixed positions in the series. D., for example, divided her series into rows of 10, and attached the second and third units to the first, the fourth, sixth and seventh to the fifth, the eighth to the seventh and the ninth to the tenth. With the majority of all the subjects, the length of the even unit-groups was four, but three was also a common length. The even division of the series seems to answer to rhythmical reading in the method of complete memorising; the landmark method of grouping does not.¹

¹ The writer has examined a considerable number of records (including her own) to determine whether or not the subjects' habits of grouping left unmistakable traces upon the numerical results. This is not the case. It would be impossible to guess at the size of the unit-groups from any accumulation of errors at particular points in the series as reconstructed. This hiding of the lines of cleavage between the groups may be due to the heterogeneous character of the material (see page 141) which introduces special difficulties at different points in different series, or it may be due to the special effort which the subjects made to join the unit-groups securely one to another.

The results show that the subjects tended, like subjects in the method of complete memorising, to learn the series from the two ends toward the middle and more rapidly from the beginning than from the end. This tendency, however, is

d. Ebert and Meumann analyse the practice acquired in memorising meaningless material into the following factors (the order is that of the present writer): (a) Habit of rhythmical reading; (b) increasing keenness of apprehension; (c) increasing stability of attention; (d) increasing economy in the application of attention; (e) change in the affective tone of the learning-process from unpleasant to pleasant; (f) change in the organic sensations connected with it and in its motor accompaniments,—particularly the lapsing of unnecessary muscular contractions; (g) increasing complexity in the images which represent the series-members,—so, for example, that the eye-minded subject becomes ear-minded and lip-minded as well; and (h) the subsidence of the tendency to catch at extrinsic associations,—even of those between the series-members and the notions of their absolute positions in the series.¹ Before practice, consciousness (*Bewusstsein*) is like a scatter-brained commander who brings up all sorts of auxiliary troupes which afterwards prove superfluous. With practice, the learning process becomes simpler. Consciousness takes the most direct means and discards secondary measures. Learning becomes ever more ‘mechanical.’

Fracker holds that the development and the consistent use of one’s own imagery is ‘the central or most essential element’

not marked because the series were learned *in toto* with so small a number of repetitions. Rather less than half of the subjects specifically questioned noted that they had purposely paid special attentions at one presentation to certain fragments of the series and at other presentations to others. This practice was called among us piecemeal (*stückweise*) learning. It is hard to draw the line between the learning of the series in disconnected parts, on the one hand—a practice which is certainly disadvantageous—and the economical distribution of attention, on the other hand—a knack which comes with training and is essential to rapid learning. Probably in so far as our subjects, including G., indulged in piecemeal learning, the practice was serviceable.

¹ Ebert and Meumann, *op. cit.*, (a) and (b), pp. 204 and 213; (c) and (d), 204; (e), 215–216; (f), 202–203 and 215; (g), 204 and 228; (h), 204–215. The writers say (p. 215) that the central factor in practice is to be regarded as a will-phenomenon, since without the will to improve no progress is possible. In the opinion of the present writer such a will-phenomenon is altogether disparate from the factors enumerated in the text and need not enter into this discussion. Ebert and Meumann speak—whether carelessly or with intention—of pleasure as both an index to increase of psychological energy and as a factor in producing it (p. 216).

in improvement in memorising and in the transference of practice from one material to another.¹ The kind of imagery meant is in large part that which is called by the present writer *representative*. Coover and Angell find the essential elements of practice in the steadying and economical distribution of attention and in the 'habit of stripping the essential process of unnecessary and complicating accessories.'² Upon the importance of this last point all these students of practice and its transfer agree.

In dealing with the statements of our own subjects, the experience of G. will as, heretofore, first be detailed; and later, the fragmentary remarks of the other subjects will be summarised.

1. Before discussing the nature of the training which G. acquired in these experiments, it is necessary to note the practice-level at which she began. This subject had in childhood a verbal memory which was somewhat extraordinary, and as a result she was often 'shown off' by her parents when three or four years old, and later, in school-exhibitions. She thus acquired a certain vanity in regard to memorising feats. Hearing (at twelve or thirteen) that children in the mission schools of China often learn the whole of the New Testament *verbatim*, G. emulously set out to do likewise, began valiantly and successfully with the genealogy in the first chapter and actually learned considerable portions of the Gospels and Epistles. As a Sophomore in college she found Loissette's memory-system an absurdly round-about and laborious procedure. She was at this time (in her nineteenth year) conscious of habitually memorising almost without auxiliary associations. A year or two after graduation, during recovery from a tedious illness she learned "In Memoriam" *in toto*. Some years later, tests in the Cornell laboratory showed that she retained a facility in

¹ *Op. cit.* pp. 94-97 and 101.

² Coover and Angell, *General Practice Effect of Special Exercise*, Am. Jour. Psych., xviii, pp. 328-340, especially p. 340. Cf. also J. R. Angell, *The Doctrine of Formal Discipline in the Light of the Principles of General Psychology*, Supplement to University of Michigan Bulletin, New Series, ix, No. 8, especially pp. 9, 10 and 13.

memorising which was certainly above the average.¹ It is impossible to compare G. with the other subject of these experiments. Her case must stand alone both on account of the degree of her previous training and on account of the overtaxing of her memory during the experiments themselves—a burdening which sometimes seemed actually to paralyse her memory by anticipatory ('associative' or 'generative') inhibition.²

The factors in practice which are specified by Ebert and Meumann will be taken up severally, and after them, that development of representative imagery which is emphasised by Fracker.

(a) Rhythmical reading in the method of complete memorising finds in our method its counterpart in grouping. With G. elaboration of grouping was one of the very most important factors in practice. The system of grouping in fours developed soon after the beginning of the experiments. In Period II, 2, of the smell-work, when very long series (of 30 members and over) were first given (see page 73), the subject began to group more elaborately and to pay sharp attention to joining the initial and terminal members of groups, and to binding the terminal unit of each group to the initial unit of the next. Learning to cope with longer and longer series seemed tantamount to learning to group in a more and more complex fashion, e.g. to bind three groups of four smells into a larger group of 12, and five groups of four syllables into a larger group of twenty. Sudden increase in series-length seemed to produce a certain shock which would break up even the first groups of series-members. The surmounting of this shock seemed to lie just in the recovered ability to group.

(b) The subject was not conscious of any improvement in apprehension except in the case of the nonsense-syllables where

¹ See S. E. Sharp: *Individual Psychology*, Am. Jour. Psych., x, pp. 32-42, especially Table VI. The G. of these experiments was the person here under discussion. Other experiments made in the same year showed that G.'s discrimination for color- and brightness-differences was exceptionally poor. See I. M. Bentley: *The Fidelity of the Memorial Image*, same Journal, xi, pp. 37-38.

² See note p. 137, below.

it was well-marked. She was familiar with the smells when the experiments began.

(c) and (d) The subject was clearly conscious of learning to save her 'energy,'—i.e. her high degree attention—after the first presentation, for those units which she had not yet grasped and linked, and in particular for the joining of the unit-groups. She was not conscious of learning to control her attention better but only of learning to distribute it better.

(e) On the whole the subject always enjoyed the experiments, much as an enthusiastic player enjoys a bridge-party in spite of the bad hands he may get and the mistakes he may make in the course of the evening. Ennui never sets in. Since the task of memorising always had intrinsic interest, no great improvement in attention could be expected.

(f) The subject did not in the reconstruction experiments detect in herself superfluous innervations. However, in the longer smell-series the double effort to remember the series-order and the contents of the bottles already placed was often so exhausting as to produce muscular tremor, disagreeable chest-sensations and even slight nausea (symptoms which did not immediately pass away). The subject never had these feelings when dealing with the visible series.

(g) The direct images of the series-members did not appear to become more definite or more complex. The subject did not, to any marked degree, improve in color discrimination; she did not gain at all in ability to image the forms of the letters in the syllables; and she never acquired any concrete smell-imagery.

(h) The degree to which the subject used auxiliary associations was not a matter of practice. She had very long passed the stages at which they are first eagerly invented and then dropped. As already stated, (page 119) she was accustomed in difficulties to fall back upon them as a reserve force but she never went much out of her way to summon them.

(i) The subject had, from the first, a pretty complete system of representative images for the concrete series-members and was not conscious of much development in this particular. She agrees with Fracker (apparently as against Coover and

Angell, who appear to make no distinction between mnemonic and representative images) that these images do not fall away with practice but remain a *sine qua non* in the successful memorising at least of concrete material.

(j) One more factor in G.'s practice remains to be noted: This is the growth and persistence of spatial associations (here classed as primary).¹ Ebert and Meumann found that the tendency to grasp at position-associations fell away with practice. In our experiments, however, the habit of forming spatial associations was fostered by the reconstruction of the series in external space and by the slow rate of presentation. The subject G. believes that she was enabled to perform the feats described on page 184 of the next chapter just by this practice in forming spatial associations together with the practice obtained in grouping. *With G. 'spatial projection' and grouping were the essentials of practice.*

In the case of this subject the mechanising with practice of the memorising-process was more marked than its simplification. G. never in these experiments (never, within her recollection, in any memorising) made useless muscular contractions or employed many auxiliary associations. Therefore, at the start, the learning process was relatively simple. On the other hand, G. noted to the last (almost as this page goes to press) both the sequences and the absolute position of the series-members and sometimes noted the position both in terms of a number-word and of an imaged table-spot. It is impossible to say that she always noted with equal clearness both the sequence and the position of every series-member. Probably she did not. The whole process is very rapid, and introspection is fatal to its taking a natural course.

2. The subjects other than G. were not all questioned in regard to the consciousness of practice. Of those who were questioned, many expressly deny that their method of memorising changed. Several subjects instance grouping and several instance the forming of auxiliary associations as factors in practice. In the

¹ Notions of absolute position in the series (see p. 4) may or may not be images of places on a table or a sheet of paper, but for G. in all the later experiments the most important notions of position were spatial.

case of some of the beginners both these procedures received a certain impetus from a class-discussion which took place during the course of the experiments.¹ Two or three of the subjects, note that in the case of the color-series the imagery became more concrete and direct with practice. Wo. notes that although in placing a color or smell it was necessary at first to recall its name, yet afterwards the name-link between the concrete impression and the notion of position tended to drop out. On the other hand, C., who like Wo. memorised readily, notes that the images which represented the colors tended to become more verbal as her color-terminology improved. A number of subjects remarked toward the end of the experiments that at first they 'had not known how' to learn the series. When asked in what 'learning how' had consisted, the subjects usually instanced either the naming of the smells and colors, or 'getting used to' (learning to apprehend) the smells or syllables, or learning 'to tell the smells apart,' or grouping or piece-meal (*stückweise*) learning, or more than one of these factors. No factors which fall under the general head of the 'effort-consciousness' were spontaneously mentioned.

II. The purpose of the paragraphs immediately following is to enumerate and, in part upon an experimental basis, to discuss certain differences between the three kinds of material. In this discussion, the primary difference between verbal and non-verbal material is not included. The differences which will be considered lie (a) in the ease with which members of the three kinds of series are apprehended—their *Geläufigkeit*, (b) in the ease with which they are distinguished from one another, (c) in their attention-value or *Eindringlichkeit*, (d) in their richness in earlier associations, (e) in their homogeneity, (f) in the degree to which the difficulty of the series depends upon

¹ Experiments with beginners must be mainly demonstrative in purpose. In so far as the experiments are an humble kind of research, such discussions are unfortunate. Nevertheless, a certain artificial raising of the practice-level in the case of the beginners does not derange the argument of this report. As regards practice, the writer is not trying to prove that 'innate retentiveness' may be bettered, but is merely trying to show how much more one can accomplish with a better technique, however acquired.

the order of the members, and (*g*) in the fact that the colors and syllables are visible and the smells invisible.

a. Some pains must be taken to explain just what is meant by difference in the ease with which the members of a series are apprehended.¹ The ease with which a smell is apprehended depends in part upon its intensity and in part upon its familiarity but is not wholly determined by either or both. Anise—named paregoric by the subjects at large—is a strong and familiar smell, but is nevertheless apt to give pause for an instant to the best observers. The third factor in determining the ease with which a smell is apprehended seems to be its relative uniformity under different conditions. Some smells, notably the artificial perfumes, are liable to perversion through the exhaustion of the subject's sense-organ; others, notably the essential oil of anise, smell different at different temperatures. The 'apprehensibility' of a smell does not depend upon its distinguishableness from other scents. Eucalyptus and rosemary are very liable to be taken for each other, but neither perplexes the subject unless the other is in the series. Anise, on the other hand, has no 'twin' on the list. The ease with

¹ The verb *apprehend* is used as the equivalent of the German verb *auffassen* and differs in meaning from the verb *perceive*, in its ordinary sense, as *auffassen* differs from *vernehmen*. To apprehend is to perceive in the completest fashion—in such wise that the mere peripheral experience receives associative supplements, sensory or motor or both. To apprehend an object it is not necessary to name it. Nevertheless the naming of a scent, like the pronunciation of a written word, is the seal and attestation of apprehension. The difference in the ease with which members of series are apprehended is one of the most important differences between materials for memorising and is a difference to which, so far, scant justice has been done. Frl. Ephrussi says in regard to her experimental comparison of the relative advantages of 'Lesen im ganzen' and 'Lesen mit gehäuftem Wiederholungen': "Die Gesichtspunkte, zu denen die bisherigen Untersuchungen über die 'Oekonomie des Lernens' geführt haben, liefen darauf hinaus, dass sie betonten, es sei das Lernverfahren so zu gestalten, dass die Assoziationen gemäss ihrer Abhängigkeit von der Wiederholungszahl und der Art ihrer zeitlichen Verteilung möglichst günstig ausfielen. Meine Versuchsergebnisse zeigen, dass die Assoziationsstärke, welche unter bestimmten Umständen erzielt ist, zugleich auch von der Geläufigkeit der zu assoziierenden Vorstellungen abhängt in dem Sinne, dass mit der grösseren Geläufigkeit des Lernmaterials die grössere Assozierbarkeit verbunden ist." (*Op. cit.*, pp. 61–62). Incidentally, however, relative familiarity had already been recognized as affecting the degree of difficulty in memorising nonsense-syllables. Cf. Ebbinghaus, *Ueber das Gedächtnis*, pp. 134–135; and Müller and Schumann, *op. cit.*, p. 176.

which a color is apprehended depends mainly on its saturation, and this is at least in part because the grays and the less saturated color are more liable to momentary perversion through contrast-effects than are the more saturated colors. The ease with which a syllable is apprehended seems to depend primarily upon its resemblance to words which the subject is accustomed to reading or hearing. Differences in the ease with which the subject pronounces certain letters may also come into play. When the syllables were presented to the eye, G. had difficulty in 'grasping' those with a final *r*, *f*, or *th* sound, and those containing the vowel *o* when the final consonant was not followed by the silent *e*. Among German syllables, she has trouble with those containing *o*, *u*, *ö*, and *ü*. Such difficulties would seem to depend upon relative effort or uncertainty in pronunciation. For example, the sound ordinarily, if not correctly, given by Americans to *o* in the word *dog* is totally different from the sound of *o* in the word *dot*. When the syllable is meaningless there may be slight hesitation as to which precedent to follow. The hesitancy which G. had already experienced in earlier experiments with nonsense-syllables, led in these experiments to the use of the silent *e* to minimise the difficulty in reading.

The subject G., at least, grasped both the smells and colors more slowly than the syllables. A certain evasiveness seems to be a direct consequence of the simplicity of the colors and smells. This evasiveness, however, does not imply greater variation in 'apprehensibility' than is shown by the syllables. Undoubtedly, G. 'grasped' the smells more easily from the beginning than did most of the other subjects. It seems pretty safe to say that for all the subjects 'syllables, colors, smells' is the order of *Geläufigkeit* or 'apprehensibility' but that the degree of difference is a matter of individuality.

b. As regards sensible discrimination, G. had most difficulty with the color-units. The majority of the other subjects had most trouble with the smells. The writer is inclined to think that in their case the trouble in discrimination was rooted in the trouble in apprehension. The untrained subjects com-

plained repeatedly that 'that all the smells were just alike' except for a few 'very bad' ones, and sometimes that the smells were 'intangible,' or 'impossible' to 'get hold of.' But as a matter of fact there was much more objective excuse for confusing colors than for confusing smells. It will be remembered that the formation of the smell-series was not left absolutely to chance, but that certain highly similar smells might not be introduced into the same series. The writer has spent much time to little purpose in studying the cases in which each of two smells or colors was put in the place of the other. This examination throws no light upon questions of smell-classification. It does, however, show first, that the colors, much more often than the smells, were transposed from confusion due to real similarity, and second, that only a minority of the cases in which two smells were transposed can be explained by similarity of any sort. The following statement will illustrate the nature of the smell-transpositions: Any transposition of two series-members may be due to accident. When, however, a reconstruction is not the subject's first attempt at rearranging the series and when this reconstruction is right except for a single transposition, the presumption is at its maximum that the transposition is due to confusion. Under these conditions, *asafoetida* and some other smell were transposed 20 times. In two cases only this other smell was really a similar one—in one case onion, and in the other fish-glue. In four cases the other smell was also very unpleasant; twice it was valerian and twice pyridin. Once the other smell was eucalyptus, once rosemary, once cinnamon, twice ginger, once bay-rum, once spearmint, once sassafras, once cocoa-butter, once lavender, once almond, once tar, once naphtholine and once kerosene. This sample shows how little in regard to the qualitative kinship of smells can be gleaned from the results of beginners. The sample also bears out the conjecture that difficulty in smell discrimination may be largely a matter of having very vague but highly affective smell percepts.

'Syllables, then colors or smells' was the order of 'discriminability.' Difficulties in discrimination and apprehension put the colors and smells at a very decided disadvantage as com-

pared with the syllables. There is very little difficulty in discriminating the syllables when properly apprehended or 'read.' Like syllables, to be sure, are sometimes confused when a series is very imperfectly memorised, but like colors are often confused in reconstructions which are otherwise perfect. Hence, the divergence between the values of W_p and W_r in the color-series. (See pages 77 and 98.) Nevertheless, there is no great difference in the difficulty of memorising the three sorts of material. Therefore, one of the two following alternatives must be true: either the concrete materials have certain counterbalancing advantages over the syllables, or else memorising by the reconstruction method is so easy that the nature of the units makes very little difference.

When the units of a series are hard to apprehend or hard to discriminate from one another, the learning of the series is made more difficult in two ways. In the first place, the subject is likely not to secure a series of definite and well differentiated images. In the second place, attention is so taken up by each link as it comes that there is none left over for the sequences or for notions of position. G. was very frequently conscious of this fact, and when working with long series, was often conscious during the first presentation of 'trying to find out just what colors, syllables or smells were in the series and of letting the order go.' The numerical results show that other subjects besides G. practiced this particular economy of attention. In the case of the easier series, nearly all the subjects learned more sequences at the first than at any other presentation. Of the 'good' memorisers only Wo. seems to have given her attention habitually at the first presentation to apprehending or 'taking stock' of the series-members rather than to connecting them. G. excelled the other subjects more in the amount learned upon the first presentation than in the speed with which an entirely correct reconstruction was achieved. But when series were hard for any subject—hard on account of material or on account of length—then more 'new' sequences were mastered at the second than at the first presentation. This fact may be illustrated with the series of 41 colors and 41 smells learned by G at

about the same time (in the spring of 1904) in the second year of the color and third year of the smell work. In the case of the smells 68.4 per cent of the sequences were reproduced at the first trial, and 95.2 per cent at the second. Thus roughly speaking we may say that 68.4 per cent of the smell-sequences were learned on the first presentation and 26.8 per cent at the second. In the case of the colors, 25.3 per cent of the color-sequences were reproduced at the first trial and 81.1 per cent at the second. Thus we may say, roughly, that 25.3 per cent of the sequences were learned on the first trial and 55.8 on the second. The colors were notoriously hard for G. to discriminate. In short, one may conclude that in the case of a long or otherwise hard series the presentation during which the subject does most of his memorising must be preceded by at least one presentation during which the subject is for the most part occupied with that apprehending which is the conscious condition of discriminating the series-members.¹

c. A third difference between the three kinds of material is their relative attention-value or strikingness (*Eindringlichkeit*). The strikingness of a smell depends upon its intensity and upon its affective value apart from intensity. The intensity of certain scents has its correlate in the saturation of certain colors. Any one of the pure spectral colors from violet-red through yellow to blue-green 'catches the eye' of the subject, whether it contrasts in hue or brightness with its neighbors or not. So far as memorising is concerned photometric value cannot be considered as the direct analogue of smell-intensity, for the paler tints are not especially striking. Nevertheless, it seems to be the difference in brightness which makes the spectral colors

¹ In learning by the method of complete memorising series of syllables presented to the eye, Ebert and Meumann (*op. cit.*, pp. 43-45) distinguish five stages: (1) *Orientieren*; (2) *apperzierendes Lesen*; (3) *rhythmisierendes Lesen*; (4) *antizipierendes Lesen*; and (5) *probierendes, kontrollierendes Lesen*. In the stage of orientation, mistakes in reading occur; in the second stage reading is smooth and monotonous. In our own experiments, the stages corresponding to the second and third of these five stages were blended, but in the difficult series the stage of orientation was sharply marked off. On the relatively large amount ordinarily accomplished by the first presentation, see Ebbinghaus, *Grundzüge*, 2d edition, p. 652, with the references given on p. 651.

from red through yellow to blue-green so much more 'striking' than the spectral colors from blue to blue-violet. The pleasantness and unpleasantness possessed by certain smells, apart from their intensity, have correlates in the pleasingness and unpleasantness of certain colors, apart from their saturation. Of course the nonsense-syllables do not vary in strikingness in the same degree in which the smells and colors vary. Nevertheless, their variation is by no means to be overlooked. The strikingness of a syllable as a whole depends largely upon the strikingness of its component letters. Müller and Pilzecker conclude (from an analysis of the partially right cases which they obtained by the *Treffermethode*) that the following circumstances make a letter easier to remember; (1) its auditory or visual strikingness (*Eindringlichkeit*), (2) its rarity in ordinary speech, and (3) the difficulty of pronunciation. By those of their subjects who were of strongly auditory type, *eu* and *ö* among the vowels were best remembered. *Eu* is not very common and its sound is prolonged. *Ö* resembles it and was in the reading pronounced with especial pains. *Aa* was best remembered by subjects of weakly auditory type. Among consonants *sch* was best remembered by both classes of subjects. For G. the strikingness of a syllable depended much upon the vividness of its color. The other subjects made no comment upon the relative attention-value of the various syllables.

However, although the syllables differ considerably from one another in attention-value, yet they have very little intrinsic interest as compared with the colors and the smells. As a German victim has put it, nonsense-syllables are '*schrecklich sinnlos*.' In the matter of attention-value the concrete material has a certain offset to the ease with which the syllables are apprehended and discriminated. To most of the subjects the æsthetic value of the colors was much less important than the affective value of the smells. Many persons seem incapable of being appreciably pleased or displeased by a small square of colored paper. With G. the case was reversed. The affective value of odors, except in a few instances, had almost entirely worn off before these experiments had begun. On the other hand, although a color was seldom noted for its ugliness, yet the

prettiness of certain colors and color-combinations was a perennial source of satisfaction. Nevertheless, the affective value of the colors proved no great offset to the difficulty of distinguishing them from one another.

In considering the affective value of the concrete material, the writer has examined about 300 cases in which not more than two sequences were correctly reproduced in the reconstruction of smell- or color-series which involved each at least nine possible right cases. The object of the scrutiny was to determine why these particular sequences had 'impressed themselves' on the memory of the subject. In only about 11 per cent of the total number of cases could the singling out of these sequences be explained by the pleasantness or unpleasantness of one or both members, or by the pleasing or displeasing combination of two members. In about 47 per cent of the total number of cases, the sequences in question stood at or near the beginning or at the end of the series. Most of the other cases among the smells are explicable by the great strength or familiarity of both odors; most of the other cases among the colors are explicable by similarity of hue.

From these negative results the writer concludes not that affective value is an unimportant factor in memorising, but that in the course of these experiments the affective value rapidly wore off the colors and smells.¹ Moreover, the results presented, in their very nature, must fail to do justice to the importance of such affective value as persisted. When a few sequences are especially easy to remember, more 'energy' is left over for the others—i. e., attention remains more stable—and the whole series may be so nearly memorised that in the objective results the mastery of no particular fragment stands out. Nevertheless, the greater attention-value of the smells and colors does not seem to have been a very important offset to the dis-

¹ The writer dissents utterly from the conclusions reached by Miss Kate Gordon in regard to affective value as a factor in memorising certain designs. See *Arch. für gesammte Psychologie*, iv, pp. 437-458. Miss H. D. Cook in criticising the argument in the *Psychological Review*, seems to have pointed out the real reason why the experiments in question had a negative outcome, namely, that it was the design as a whole which was pleasant or unpleasant, whereas the subjects were required to reproduce the details.

tinctness and the discreteness of the syllables. The order of advantage as regards attention value is 'colors or smells, smells or colors, syllables.'

d. The fourth difference between the three sorts of materials lies in the relative richness of the units in associations which are extrinsic to particular series. Associations with series-members which are extrinsic to the series itself may either help or hinder the process of memorising. (1) If the ideas associated with the different members have nothing to do with their neighbors or their places in other series, then they will act as 'aids to attention' or 'aids to linkage.' The association of violets with a particular person draws special attention to the perfume; the idea 'Gloucester' forms an associative link between the smells of whale-oil and tar. (2) Occasionally, the memory of the position of a series-member in another series may be an aid to attention. (3) As a rule, however, associations formed with the members of a given series in earlier series, whether they produce actual memory-images or remain mere physiological tendencies to remember, will hamper the memorising of the new series through anticipatory inhibition, or will hamper its proper reconstruction through reciprocal inhibition, or will have both effects. If on a given day lemon is the tenth smell of a series and comes after amber, and on the next day is the thirteenth smell and comes after something else, then any one of several things may happen. On the one hand, the subject may be confused by the difference in position. Then, in the first attempts at reconstruction, he may not 'remember anything about' lemon, or he may choose to put it after civet or some other smell resembling amber, or he may put it in the tenth place regardless of its new neighbors. On the other hand, as was stated, the old associations may aid in memorising the new series. When the new series is presented the subject may rapidly and accurately compare the two positions and thus make out of the past history of the lemon-smell an aid to attention. Such comparisons are, however, never made except by good subjects under favorable circumstances. As a matter of fact, the same scent and color occurred very often indeed from series

to series, since not more than 129 scents and 136 colors were ever in use at any one time, and since fewer than thirty scents were rarely given, even to the most unpracticed subjects, at any one sitting. Thus, it became impossible to trace the history of a smell from series to series. Indeed if 'retroactive inhibition' had not kept 'wiping the slate' in a manner which will be noted, the legible recording of the new series would have been impossible.¹ In short, such vestiges of old series as remained over from day to day were, in general, only a source of confusion.

The nonsense-syllables were no poorer than the colors and smells in serviceable associations, and were much freer from troublesome associations. At first thought one is inclined to suppose that the colors and smells must come laden with extrinsic associations and to dwell on the meaningless character of the syllables. This view, however, is not just. All the common colors have been factors in so many different experiences that the associations cancel and the color becomes almost like a numeral or letter. (Cf. page 28.) Much has been said of the reminiscent power of smells, but the popular notion has sustained rebuttal rather than verification when tested by experiment.² The truth seems (to the writer) to be that

¹ The distinction between the three kinds of inhibition which the present writer calls *retroactive*, *anticipatory*, and *reciprocal* is taken from Müller and Pilzecker, *op. cit.*, pp. 138-140, 144-157, and 194-198, and from Ebbinghaus, *Grundzüge*, pp. 686 and 694-695. Anticipatory inhibition is called by Ebbinghaus *associative* and by Müller and Pilzecker *generative Hemmung*. Reciprocal inhibition is called by Ebbinghaus *reproductive* and by Müller and Pilzecker *effectuelle Hemmung*. Retroactive inhibition is the breaking up of an old association by a new one, or set of associations by new ones, as, for example, when the learning of a new series of smells 'drives the old series out of one's head.' Anticipatory inhibition is the hampering or hindering of the formation of a new association by an old one, as when, for example, the memory *lemon-amber* makes the sequence *lemon-strawberry* 'hard to learn.' Reciprocal inhibition is the mutual interference of two associations both fully formed and both attaching to the same experience. (It is the Kilkenny-cat form of inhibition.) For example, the conflicting associations *lemon amber* and *lemon strawberry* may so 'block each other that the subject's 'head is a blank.' Or if the first association is the stronger, the image *amber* may appear after some delay and may or may not be followed after an interval by the image *strawberry*.

² See Heywood and Vortriede: *Some Experiments on the Associative Power of Smells*, Am. Jour. Psych., xvii, pp. 537-541, and Bolger and Titchener under same title, same Journal, xviii, pp. 326-327.

unaccustomed smells have reminiscent power, and that they tend to recall the same images every time they occur, whereas the commonplace smells of our daily life have no more reminiscent value than the blue of the sky or the click of a typewriter. They have so many associative connections that very few associated images appear in consciousness apart from the stereotyped images which are serviceable in practical life. For G. the reminiscent as well as the affective value of most of the smells had worn off before these experiments were begun. The subjects who were dull in apprehending the scents are not likely to have associated very definite images with them. On the other hand, G. is convinced from self-observation and from the remarks of the other subjects that the syllables, when clearly apprehended, were especially fertile in verbal associates. Furthermore, G. urges that in their freedom from troublesome associations, the syllables had an immense advantage over the smells and colors. As the smell and color experiments proceeded month after month, G. felt that anticipatory inhibition tended strongly to counteract the effect of practice. The subject often compared her 'mind,'—i. e., the background upon which she projected her color-images (see page 106)—to a school black-board which is dusty and blurred at the end of a week but is washed clean and black for Monday.

With G. retroactive inhibition was obvious. Each series was remembered fairly well until the next was given. That is to say, the subject could name hours afterwards the members of the last series given, and the associations involved would remain almost undisturbed for weeks.¹ But as soon as a new series was given, the eraser (the subject actually visualised a blackboard eraser in this connection) was drawn over the old series. A 'smudge' might remain to make the new series illegible but the old one could no longer be read off.

One may sum up the case in regard to extrinsic associations by saying that the order of advantage is the following: 'Syllables, then smells and colors.'

It seems proper to introduce here certain data which bear

¹ *In Bereitschaft*—ready to function.

both upon the degree of practice to which G. had attained and upon the effect of anticipatory and reciprocal inhibition.

In each of the nine periods belonging to the first group of experiments, the results were massed, not only according to series-length, but also according to certain of the general conditions under which they were obtained. In the first place, the results obtained at each of several grades of 'physical condition' were massed apart. The physical condition was graded *A*, if the subject was free both from general fatigue and from more specific discomfort. It was graded *B* if she was 'tired' but was free from discomfort not ordinarily covered by that elastic term. It was graded *C* or *D* if she had a headache or any other positive ailment. In the second place, the results obtained in the hours following 9:00 a.m., 4:30 p.m. and 9:00 p.m. were massed apart. Lastly, in the four periods of the first year, the results obtained on Monday and Tuesday, those obtained on Wednesday and Thursday, and those obtained on Friday and Saturday were massed in separate sets. The following conclusions may be drawn from the figures thus reached:

1. The facility of this subject in memorising depends to a surprisingly slight extent upon her physical condition. In the earlier part of the work, she memorised rather more readily at *A*, but in the last two periods of the second year, she learned somewhat more readily at *B*. The very first and last periods of the nine offer fair specimens of the differences. In the first period, G. learned 81 series at the condition marked *A*, 73 series at *B*, and 34 series at *C*. (Her condition at the end of any hard day's work was marked *B*; the letter indicates no pathological state.) The average series-lengths were respectively 10.1, 9.9, and 9.9. The average numbers of repetitions necessary to correct reconstruction were 1.7, 1.8, and 1.9. The percentages of right sequences obtained on the first trial were 71.6, 67.7, and 69.3. In the last period of all, G. learned 11 series at *A*, 25 series at *B*, and 4 series at *C*. The average lengths were respectively 33.2, 33.8, and 30.8. The numbers of repetitions necessary to correct reconstruction were 1.9, 1.8, and 1.8. The numbers necessary to a perfect reconstruction were 2.0, 2.3, and 2.0. The percentages of right sequences obtained at the first trial were 71.5, 80.5, and 63.9.

2. The subject memorised more readily in the morning than in the afternoon or evening, except in the last two periods of the second year. In the very last period she learned distinctly better in the evening. The experiments made in the daytime were made in the laboratory; those made in the evening were made at home. Evidently, the freedom from interruption secured at home tended to counterbalance the fatigue at the end of the day. The subject's physical condition was ordinarily graded *B* in the evening. Thus the fact that at this time she memorised better in the evening explains the fact that she appeared to memorise better when tired.

3. The subject learned more easily on Monday and Tuesday than on the later days of the week. In the last period of the first year—the last period in which the results were massed according to the days of the week on which they were obtained—the figures are as follows: On Monday and Tuesday, 62 series were learned; on Wednesday and Thursday, 93; on Friday and Saturday, 85. The average lengths were respectively 14.6, 14.4 and 14.5. The average numbers of repetitions necessary to a correct reconstruction were 1.4, 1.5, and 1.5. The numbers necessary to a perfect reconstruction were 1.7, 1.9, and 1.7. The percentages of sequences obtained at the first trial were 89.2, 82.3, and 82.6. The better results obtained early in the week cannot be explained wholly by the fact that the subject was in better physical trim, for in this period she learned about as well at *B* as at *A*, although the later work of the day was done in the afternoon amid the interruptions of the laboratory. The results as a whole give a slight but distinct indication that a short intermission in the work was in itself an advantage and that G.'s assertion in regard to the effect of the Sunday rest was not altogether fanciful.¹

The results of no other subject have been carefully examined to gauge the effect of fatigue. A cursory examination, however, tends to confirm the observation of ordinary life that with most

¹Cf. Müller and Schuman, *op. cit.* p. 328. As stated on p. 74 experiments were sometimes made on Sunday in the second year, of this group in order to test the effect of cutting out the day's rest. It proved impossible, however, to make the Sunday work regular.

persons learning progresses much better when the learner is well and fresh. The case of G. seems to have been unusual as regards the small difference made by fatigue and illness, but this small difference is probably 'a mere matter of practice.' According to several investigators, the overcoming of one's disinclination is no small part of practice in memorising, and it is certain that many of us allow the weakness of the flesh to impede us most in the tasks which are most odious to us. The series interested G.; therefore, they held her attention even toward the end of a long day's work.

e. The fifth difference between the three kinds of material which must here be discussed is the difference in heterogeneity. By heterogeneity the writer means any kind of variety, even the variety created by the very familiar among the unfamiliar. Very intense, pungent, unpleasant, or familiar odors, glaring colors, sharply contrasting or æsthetically pleasing pairs of colors, black or white set among the colors, word-like or liquid syllables or rhyming pairs of syllables,—in short, any unusual units or combinations of units,—are apt to form 'pillars' to which in the process of memorising other units are attached. The greater the number of such pillars and the more even their distribution throughout the series, the easier the series is to learn, at least within limits which are not likely to be transgressed in the case of series made up by chance. To be sure, the strikingness of certain links in a series is not an unmixed blessing to the subject. The uncouthness of certain syllables such as *thoth* may distract attention from the all-important matter of series-order. So also the 'shock' of a very unpleasant smell may distract attention from its position—especially if emotional disturbance supervenes. On the whole, however, the variegated series is learned with appreciably less effort.

In the matter of variety, the smell-series have the advantage over the colors and obviously both kinds of concrete material have a great advantage over the syllables. In the experience of G. variety is the chief advantage which the smell and color series have. This opinion of G. was submitted to a rough experimental test: If variety makes a series easier to memorise then it

might be argued that a series containing both colors and smells would be easier to memorise than a series formed of colors only or of smells only. We tested this supposition experimentally. To G. smells series made up of alternate colors and smells were given in turn with series made up wholly of smells and wholly of colors. The nature of the scents was concealed from the subject's eye in the manner described on page 16. When the mixed series contained an uneven number of members a color stood at the beginning and at the end. These experiments were made in the spring of 1906 and overlap the control-experiments described on pages 108-109.

The results with G. were as follows:

<i>Series.</i>	<i>Procedure.</i>	<i>ML.</i>	<i>N.</i>	<i>Nr.</i>	<i>Wr.</i>	<i>MV.</i>	<i>Np.</i>	<i>Wp.</i>	<i>MV.</i>
Smells....	With blindfolding	25.5	10	8	1.6	0.8	8	2.0	1.0
Smells....	Without blindfolding	25.5	10	10	1.5	0.5	10	1.9	0.5
Colors....	Normal	25.5	20	18	1.9	0.7	18	2.3	1.2
Mixed....		25.5	20	20	1.5	0.6	20	1.9	0.6

The series-lengths used were 12, 24, and 41. From the results it would seem that the mixed series were easier for G. to learn than the colors, but were neither easier nor harder to learn than the unmixed smell-series when the blindfolding handicap was removed. Their mixed character seemed to atone for the presence of the colors. The subject had had no previous practice in learning mixed series. The members were learned in single series, that is, the subject did not connect color to color or smell to smell, skipping the disparate link.

The following results were obtained from 19 beginners (not the beginners who served in the experiments of Groups II, III, and IV):

<i>Series.</i>	<i>L.</i>	<i>N.</i>	<i>RS%</i> <i>W = 1.</i>	<i>RP%</i> <i>W = 1.</i>	<i>L.</i>	<i>N.</i>	<i>RS%</i> <i>W = 1.</i>	<i>RP%</i> <i>W = 1.</i>
Mixed.....	17	304	38.8	54.9	25	303	22.0	36.8
Colors.....	17	19	41.4	46.7	25	19	25.7	28.5
Smells.....	17	16	27.3	36.7	25	15	17.5	21.1

Series as long as 17 and 25 were used in order that the numbers of repetitions necessary for reconstruction might not be so small for both lengths as to approximate to one another. As a matter of fact, however, so few of the series were ever reconstructed correctly or perfectly that the values of Wr and Wp are not worth giving. The unmixed series of smells and colors were given at the end of the experiments with the mixed series.¹ The results, just given, when eked out by an examination of the individual records, show that the variety of the mixed series was an advantage in memorising positions but that the presence of the smells was a disadvantage in memorising sequences. The mixed series were easier to learn than the series composed entirely of smells; the difficulty of memorising the sequences in the mixed series evidently depended not upon the fact of mixture but upon the difficulty of learning the smells. The subjects dealt with the mixed series in three different ways. Five subjects out of the nineteen were like G. in uniting the two kinds of members into a single series. Six subjects, on the other hand, learned the smells and colors in two separate series, connecting color with color and smell with smell. Eight subjects followed a middle course, learning the color-sequences or positions as a scaffolding and connecting each smell to the color preceding or following. It cannot be said that one of these methods proved more successful than another. The best memoriser in this group of beginners followed the second method which seems the most awkward. Position-notions did not seem to be relatively important in learning the mixed series; seventeen of the nineteen subjects claim to have noted both sequences and positions.

Our results, as far as they go, are evidently somewhat at variance with those of Münsterberg and Bigham.² It would be worth while to investigate the point at issue with more

¹ Two series-lengths were used for purposes of illustration and instruction. It is obvious that the experiments were badly planned as regards their bearing upon the point here under discussion.

² Münsterberg and Bigham, *op. cit.*, p. 37.

extended experiments. The data in hand are too scanty to make further discussion profitable.

Before closing the discussion of variety among series-members it should be noted incidentally that the rhymes, alliterations and assonances which occur in the hit-or-miss series of nonsense-syllables have their parallel in the case of the series of colors and smells. The presence of smells of the same subgroup—the presence, for example, of several spices or mints—makes the series easier rather than harder, provided that the smells are easily discriminated. The subject is aided by rapid comparisons of this sort: ‘cloves in such a place—cinnamon in such another; peppermint here—wintergreen there.’ It is only a certain great degree of similarity between the odors which creates difficulty. So, also, a color-series is made easier by the presence of similar yet easily discriminable colors such as the spectral green and blue-green. All three sorts of series are extremely liable to the favorable accident of similarity between some few of the links.

In the matter of variety, the order of advantage is the following; ‘smells, colors, syllables.’

f. A sixth point of difference between the kinds of materials lies in the fact that the difficulty of a smell-series depends upon the assortment and order of the qualities in a fashion which has no analogue in the case of the syllables and only a very rough one in the case of the colors. In the case of the smells one has to reckon with the phenomena of exhaustion. When smells which are very similar come next to one another in a series, then (1) the qualitative difference between them may be obscured, (2) the quality of the scent or scents which come later may be perverted and (3) the intensity of these later scents may be considerably diminished. An illustration of this last and most important alteration is that vanilla is almost odorless when smelled after a strong flower perfume. An instance of perversion from exhaustion is the shoe-polish odor taken on by the almond-like nitrobenzol. Qualitative alterations with the

small degree of exhaustion which here comes in question,¹ were very rarely noted by the principal subject except when several artificial perfumes occurred in succession. Theoretically, the degree to which exhaustion is likely to prove troublesome depends not only upon the juxtaposition of similar smells but also upon the quality of the odors in the series at large. Some smells, notably those of the fragrant group, are more exhausting than others. In the experience of the principal subject, however, the differences in the exhausting effect of different series were, apart from the length of the series, of no importance whatsoever. Nor did this subject actually observe that the intensity of an odor depended upon the number of other smells which had preceded it in the series. As a matter of fact, she noted the exhaustion-phenomena only (1) when a strong smell was followed by a weaker one of somewhat similar quality, (2) when several artificial perfumes came together, and (3) after general fatigue had begun to set in.

In the case of the color-series one finds a very rough parallel to the effect of exhaustion in that of adaptation.² This is important only as it occasionally makes itself felt in successive contrast-effects. The most extreme instance of this effect occurs when a neutral gray coming after a saturated color appears tinged with the complementary hue. The really neu-

¹ A distinction is drawn between this slight and local exhaustion and the kind of fatigue felt at the end of an experimental sitting. In this second sort of fatigue, both brain and sense-organ are involved. Exhaustion, in the technical sense and in the opinion of the present writer, is a fatigue phenomenon, but it is scarcely fatigue in the ordinary laboratory acceptance.

² In the opinion of the writer, the smell phenomenon which is ordinarily called exhaustion cannot be considered as analogous to visual adaptation because it has no contrast effects. There are no negative after-images of smells and there are no clear instances of successive contrast. Occasionally one smell in a series will catch the attention because it is preceded by others which are different in quality, but this fact does not imply any sense-organ contrast. Moreover, there is an actual blurring of difference between two similar smells occurring in close succession, whereas there is some contrast even between two very similar colors. (Orange, for example, takes on a more yellowish cast when seen after scarlet.) It is palpably true that smells do not 'throw each other off as far as possible' in the fashion of 'color-antagonism.' The only possible color parallel to the mutually levelling effect of two odors when smelled turn about is the extremely distant and doubtful one of the washing of colors over one another after simultaneous light induction has had time to set in.

tral gray may then be confused with a really colored gray in the same series. Since in each attempt at reconstruction the colors are presented to the subject in random order and not in the order of presentation, it is evident that perplexities must sometimes have arisen from an apparent alteration in the smells and colors themselves. To take again a most extreme instance, the same neutral gray which looked greenish in the series because it followed scarlet might look reddish when the links were presented for rearrangement because it followed blue-green. In the experience of G. such perversions were very frequent indeed. On the whole, however, the order of advantage in freedom from such alterations is 'syllables, colors, smells.'

g. The last point to be discussed in this chapter is the gross difference between visible and invisible material. Under this heading two questions arise: 1. The most important question is in regard to the degree of advantage which the colors and syllables received from being spread out before the subject's eyes in the process of reconstruction. To determine the degree of advantage a small number of control experiments have just been made (in the fall of 1908). In one of the two sets of experiments compared, series of colors were given according to the standard method, with the single unimportant variation of not handling the first link to the subject as a 'starter.' In the other set of experiments, the color-squares were covered one after another with squares of white (or buff) paper as soon as they were placed by the subject in the process of reconstruction. This covering of the colors makes the conditions comparable to the conditions in the smell-experiments in which the subject was not blindfolded; the subject has to remember the nature of the links already placed, but is not compelled to 'feel his way' in the act of placing. The series were all of the length 20. No importance was attached to certainty of correctness, but the placing-rules were the same as in the older experiments. The two procedures were used according to a compensating program. The subjects A. and Wil. were advanced students. The results were as follows:

<i>Subject.</i>	<i>Method.</i>	<i>N.</i>	<i>W_r.</i>	<i>MV.</i>	<i>W_p.</i>	<i>MV.</i>	<i>RS%</i> <i>W = 1.</i>
G.....	Normal	20	2.2	0.3	2.5	0.6	47.6
	Covered series	20	2.8	0.8	3.4	1.0	41.6
A.....	Normal	10	2.7	0.6	3.2	0.7	34.2
	Covered series	10	3.3	0.8	3.6	0.7	24.7
Wil.....	Normal	7	3.6	1.1	4.1	1.0	15.8
	Covered series	8	4.5	0.8	5.0	1.0	18.4

The figures demonstrated exactly that which one would conjecture without them, namely, that it is harder to reconstruct the color-series when one's work remains in part invisible. The difference is not overwhelming. According to the introspection of the subjects the difficulty in correctly rearranging the covered series lay mainly in the fact that very similar colors could not, as in the normal method, be compared in the process of reconstruction. (In the work with the covered series no two colors were ever seen simultaneously.) The subjects had no appreciable trouble in remembering the approximate nature of the links placed and covered. The length of the series was not great enough to make this task very arduous, but G. was surprised to find it as slight as it proved. No subject noted that the actual appearance of the links acted more definitely than the images of the covered links in the way of reproductive reinforcement. (See page 33.) On the whole, one may infer that the covering of the color-series would not have made them as difficult as the smells for the majority of the subjects but would have made them for G. even more difficult than they relatively were. It is practically certain that covering the syllables would not have introduced so much difficulty as covering the colors introduced, for the difficulty of discriminating the syllables from one another is negligible.

2. A less important question in regard to the difference between the visible and invisible material is in regard to the degree in which blindfolding handicapped the subjects in the normal procedure with the smell-material. The fact has already been emphasised that the subjects could not be allowed

in any case to tell the bottles apart by their appearance (page 38), but the expedient described on page 6—the use of the blue cotton—concealed the color of liquid scents and yet removed the peculiar difficulties of blindfold work.¹ These difficulties consist mainly in the facts that the groping hand-movements require considerable attention and that the subject is liable to disturbing accidents—likely to knock bottles off the table, for example. Control experiments to test the magnitude of the blindfolding-handicap were made only with G., were few in number, and are the less valuable because they were made after the subject had learned by long practice to surmount this handicap. The experiments in question form two of the sets described on pages 108-109. The results are given on page 142. The figures show that at this time the subject was placed only at the slightest possible disadvantage by the blindfolding. It seemed to her that she memorised better with the eye-bandage than without it, but this impression was no doubt due to disappointment at the smallness of the advantage in seeing the bottles. But however successfully G. finally surmounted the handicap of the eye-bandage and however easy it proved for her to remember the nature of the colors placed even when they were immediately covered from view, nevertheless, the fact remains that she was most conscious of strain in memorising the smell-series and that she attributed this fact to the double effort to remember both the order of the scents and the contents of the bottles placed. Since covering the color-series made them more difficult to reconstruct and since making the scent bottles visible made the smell-series easier to rearrange even for a subject long-inured to blindfold-work, we may certainly infer that the *syllables and colors have an advantage over the smell-series in the mere fact of being visible.*

The writer, as the subject G., draws the following conclusion from the various data presented in this chapter:

1. *In the case of long series, the order of the syllables was dis-*

¹ The only disadvantage in the procedure is that it cuts out the use of a number of very desirable solid scents.

tinctly easier to learn than the order of the smells and colors, certainly for herself and probably for the other subjects.

2. *When series are so short as to present few difficulties in discrimination, and so few that anticipatory and reciprocal inhibition do not come into play, then the concrete material has the advantage because it is more varied and more interesting.*

3. *The approximation of the numbers of necessary repetitions with the three sorts of series was due to the fact that memorising the order of all three sorts of units by the reconstruction method was so easy as to leave little margin for variation.*

4. *Neither the numerical results nor the introspective observations reveal any essential difference in the mental processes concerned in memorising the three kinds of series.*

CHAPTER VI.

AN EXPERIMENTAL STUDY OF THE RECONSTRUCTION METHOD AS USED IN THESE EXPERIMENTS

This chapter contains an account of certain groups of experiments which were made to test the various explanations proposed for the results reached on pages 87-89. It has been demonstrated that practice enables the subject to memorise by our reconstruction method the order of very long series of smells or of colors or of nonsense-syllables with a very small number of repetitions, and that this number does not vary greatly with the length of the series. Why, then, is it so easy to memorise serial order by this method and why does length make so little difference? The introspection of the subjects bears out to some extent the truth of all the solutions proposed in Chapter I for the first problem. The purpose of the experiments reported in this chapter was to put these five explanations to a more rigid test than the introspective.

It seems impossible to describe these experiments briefly in either a strictly logical or a strictly chronological order. In the first section of this chapter the different groups are treated in the order which best ensures conciseness. In the second section, an attempt is made to combine and explain the various results.

A. Account of the Experiments.

I. The first group of experiments with which one has here to deal, Group VI, bears upon the *explanation by long intervals* (see page 10). The experiments were made with smells by the normal reconstruction method except that the subjects were required in (about) half the experiments to do simple sums in addition while the units were in course of presentation. As each bottle was given a number was named which the subject was required to add to the sum of all the preceding num-

bers. The experimenter had ascertained the sum of the numbers beforehand, and recorded not merely the order in which the subject arranged the bottles but also whether the addition was right or wrong. The presentation of a series was repeated until the reconstruction was perfect and a correct 'answer' to the sum given in the corresponding presentation had been obtained. The subjects might add aloud but might not jot down the numbers. The experiments were originally designed as attention- and distraction-experiments. Hence, the numbers were given when the bottles were presented and not in the interval. However, as the subjects avowedly sandwiched the adding and the smelling, the arithmetic must have had the effect either of abbreviating the exposure-time, as far as attention was concerned, or else of filling, at least partially, the interval between exposures. The experimenters were charged not to allow any appreciable difference in the rate of presentation in the standard and in the distraction-series. Curiously enough, the additions were nearly always right, whether the series-order was learned or not. Cases in which the reconstruction was correct and the addition wrong form only about 6 per cent of the total number of reconstructions. The numbers to be added were in most sets of experiments under ten, but in a few sets they were two-place numbers between 10 and 20. One of the subjects remarked that it 'seemed a disgrace not to be able to add simple numbers.' Since the subjects took the greater pains with the adding, it seems probable that they did the adding first, during the actual presence of the odor and did the deliberate linking of the odor afterwards, in part during the relatively empty interval. The introspective records of these experiments are fragmentary.

The experiments extended through most of the year 1902-1903. The four subjects were all students in a second-year course and all worked as experimenters as well as subjects. One served six times a week as subject; the others three times.

The results of D.¹ will be given as showing the maximum effect of distraction:

¹ This was D.'s first year of smell-work. Cf. p. 72.

<i>Series.</i>	<i>Period.</i>	<i>L.</i>	<i>N.</i>	<i>RS%.</i>	<i>Nr.</i>	<i>Wr.</i>	<i>Np.</i>	<i>Wp.</i>
Standard.....	1	9	17	93.4	17	1.2	17	1.4
Single digits.....	1	9	19	79.6	18	1.4	17	1.8
Standard.....	1	12	21	77.5	20	1.7	18	2.3
Single digits.....	1	12	20	46.8	19	2.2	19	2.5
Standard.....	2	9	14	90.2	13	1.2	13	1.5
Two-place num- bers.....	2	9	14	68.8	14	1.8	13	2.1
Standard.....	2	12	17	93.6	17	1.2	16	1.3
Two place num- bers.....	2	12	14	66.4	14	1.9	14	2.4
Standard.....	3	18	13	88.7	13	1.5	13	1.8
Single digits.....	3	18	14	68.9	14	2.2	13	2.8
Standard.....	3	24	12	69.9	12	1.8	12	2.2
Single digits.....	3	24	14	62.1	14	2.1	13	2.5

Of course, the expressions 'single digits' and 'two-place numbers' refer to the numbers added in the distraction-series. Within each period, the different lengths and the standard and distraction-series were used in cyclic order.

The figures show that either the curtailing of the subject's time for memorising, or the shifting of attention between the two tasks imposed, or both, had an appreciable effect in raising the average number of repetitions necessary to learn the series and in lowering the number of sequences learned at the first trial, but that the values of *Wr* and *Wp* remain very small and are very little affected by difference in series-length. With none of the other subjects did distraction have nearly so much effect as with D. With two of them when the numbers to be added were single and the series short, the values of *Wr* and *Wp* were in a few cases actually smaller in the distraction than in the normal series.

In brief, one may conclude from these experiments that the memorising of serial order is so easy by the method under examination that the subject can bear a considerable abridgment of the leisure given for memorising, provided at least that the objective rate at which the units are given is not increased. Sometimes, evidently, the task, when easiest, is not quite hard enough without

distraction to raise attention to its highest level and to keep it taut. It may be noted that Coover and Angell consider the kind of attention most favorable for memorising to be a 'uniform state which is less than the maximum.'

II. The second group of experiments to be considered here, Group VII, bears upon *the explanation by presence of the links*. (See page 7.) The experiments were made with English normal series of nonsense-syllables of the length 12. Two methods were used *pari passu*, namely, our normal reconstruction method with one variation, and a modification of the method of retained numbers. The details of this second method were as follows: The series was presented in exactly the same fashion as when the normal reconstruction method was used. The same cards and tables were used as in the experiments of Group IV. When a series had been once presented, the subject was required to write down as many syllables as she remembered as nearly as possible in the right order. Her time was not limited. When she was done, the experimenter examined the slip of paper. If there were any mistakes whatsoever,—misspellings, omissions, interpolations, or transpositions,—the experimenter laid the first slip aside, presented the series again, and called for a new reproduction. This process was repeated until the reproduction was faultless. In these experiments, since the series were presented to the eye, no substitutions such as *c* for *k* or *se* for *ze* were allowed. The subject was told before the first presentation of a series whether she would be asked to reproduce or to reconstruct it. She was required to pronounce each syllable as it was presented and was corrected if she pronounced it improperly. The slips of paper were uniform and the subject was required to write the syllables in a column.

Two kinds of right cases were counted, namely, the syllables retained, whether they were reproduced in the right order or not, and the right sequences. In reckoning the number of members retained, 'half-right members' were taken into account. A syllable was reckoned as 'half' right when two of the three elements were exactly as given or when the initial and final elements were transposed. In the table given below two half-right

members are counted as one wholly right member. Sequences of half-right members or of a half-right and a wholly right member or the reverse were not counted. Therefore, one sequence implies two wholly right members.

In the reconstruction experiments the first syllable-card was not handed to the subject but was shuffled with the rest. Therefore, in each series of 12 members, 12 right positions but only 11 right sequences were possible. This was the single variation from the normal method.

The experiments were made in the spring of 1908. The subjects were 6 beginners, who are indicated in the table by numbers. The experimenters were 6 other beginners who worked under very close oversight. The same series were used with all the subjects in each of the two sets of experiments. The series were written on the cards and dealt out by the director. The program, not only from day to day but for each sitting, was so arranged as to make the effect of practice bear equally upon the two kinds of experiments. At first, three series were learned at a sitting; then four.

The results are contained in Table X.

Although these experiments were made upon beginners and were not very numerous, yet in the opinion of the writer the results throw the explanation by presence of the links once for all into a position of minor importance. (To these results should, of course, be added the results of the somewhat similar experiments made upon G. and reported on page 118.) *It is somewhat harder to reproduce a series perfectly from memory than to reconstruct it, but it is not very much harder.* In the case of only 2 subjects out of the 6 is Wp even $1\frac{1}{2}$ times as great in the reproduction as in the reconstruction experiments.

The relatively large number of members retained in isolation or in the wrong order appears to the writer to be not without significance. Inspection of the right-case columns seems so show that, in the reproduction series, the subjects were much more likely to retain syllables without any notion of their order than they were in the reconstruction series to remember the mere positions of syllables without any notion of their neighbors.

TABLE X.

To Show the Relative Difficulty of Memorising in the Case of Nonsense-Syllables, Series-Members and Series-Order.

<i>Subject.</i>	<i>Method.</i>	<i>N*.</i>	<i>Wr.</i>	<i>MV.</i>	<i>Wp.</i>	<i>MV.</i>	<i>RS% W = 1.</i>	<i>Retained members or right positions % W = 1.</i>
1	Reproduction	15	4.9	1.0	11.5	44.7
	Reconstruction	16	3.3	0.8	3.7	0.9	33.5	34.9
2	Reproduction	20	4.4	0.7	19.8	45.4
	Reconstruction	20	3.2	0.7	3.2	0.7	42.3	51.3
3	Reproduction	20	3.1	0.9	35.9	67.5
	Reconstruction	20	2.1	0.5	2.7	0.7	61.4	66.3
4	Reproduction	20	2.3	0.5	45.0	72.1
	Reconstruction	20	1.8	0.5	2.0	0.3	69.1	79.6
5	Reproduction	20	3.4	0.6	27.7	58.8
	Reconstruction	20	2.5	0.7	2.8	0.6	52.3	60.0
6	Reproduction	20	3.0	0.5	63.3
	Reconstruction	20	2.2	0.9	2.5	0.9	72.2	75.0

* The values of *Nr* and *Np* were in every case the same as those of the corresponding *N*.

Mastery of the units in a series seems not infrequently to antedate mastery of their order. To be sure, among the wrong cases, omissions greatly outnumber transpositions. The fact seems to be that when syllables are learned at all, they are generally, but by no means always, learned in order.

The observations of the subjects throw some light upon the difference between the two sorts of memorising. Subject 5 reported that reconstruction was easier than reproduction because she 'did not have to learn the whole word but only certain letters of it.' Although this remark shows that less was learned in the reconstruction than in the reproduction experiments, yet it implies exactly that which the subject G. had asserted of herself, namely, that *the subject learned the units (or parts of them) as a means of learning the order.*

Subjects 5 and 6 spoke of a special set of extrinsic associations which connected the syllables of the reconstruction series with their numbers, and which may, in some cases, have made the *exact* recall of a syllable unnecessary. An instance would be the connecting of a syllable which had the initial *d*, the fourth letter of the alphabet, with the number 4.

In these reproduction series, all the subjects except Nos. 2 and 5 wrote down the last few syllables first and then 'went back' to the beginning. Subject 3 says expressly that while writing the last five or six syllables, she 'forgot the first ones or else the order.' The writer knows from experience (which will be stated later) that it is inadvisable to seize last impressions first—as one often does greedily, with the feeling that 'a bird in the hand is worth two in the bush.'

Subjects 2, 3 and 5 reported that they visualised the syllables better in the reconstruction experiments, and Subjects 2 and 3 explain the relative ease of reconstruction by this fact. The sharper visual imagery might have been due to the fact that in the process of reconstruction they saw the syllables again in the same hand-writing in which they were presented, or to the fact that they were more impressed by visual spatial relations when they arranged the cards on the table than when they wrote them on a slip of paper, or to the fact that in 'learning by heart' many persons rely by habit upon auditory and tactile imagery. In any case, the better visual imagery seems to have been considered by the subjects as a boon.¹

All the subjects used auxiliary associations very freely. Many of those reported were of the 'story-type,' such as the connecting of *dave—gore—hoim* by '*Dave came home with bleeding hands.*'

After the first day, the effect of practice was not very marked and was, in the case of Subject 3, cut across by loss of interest. The first day's work of each subject was thrown out. Subject

¹ Subject 5 said that in the reproduction experiments her imagery was only auditory, whereas in the reconstruction experiments it was *both visual and auditory*—as if the advantage lay in the complexity of the imagery. Subject 2 said simply that 'learning was more oral in the reproduction series and more visual in the reconstruction series.'

1 (the poorest memoriser) said that practice consisted in 'learning to pay attention to the syllables' and finding that 'artificial connections were necessary' (*sic*). Subjects 2 and 6 spoke (spontaneously) of learning *not* to attend to all the syllables equally at each presentation ('economical distribution of attention'). Subject 2 also spoke of 'learning' to make artificial connections. Subject 3 said that practice consisted merely in 'getting used to the material and method.'

III. The third group of experiments to be considered, Group VIII, bears upon *the explanation by spatial localization*. (See page 9.) The design was to try the effect of eliminating such assistance as might be given to the subject by *reconstructing* the series in terms of spatial relations. Smells were used with one subject; nonsense-syllables with another. In one of the two sets of experiments to be compared, the normal reconstruction method was used. In the other set, the series was presented as usual, and the units were as usual handed to the subject in random order. Then, however, instead of actually arranging the units, the subject was required to state the numerical position of each in the series. The presentation and recitation of numbers was repeated until the latter was made without any mistakes—even mistakes at once corrected.

Subjects and experimenters were alike beginners. The supervision was close. The series of syllables were English normal series of the length 18. The smell work was done with the 'blue scents' (see page 16) and without the eye-bandage. The series were of the length 12. The two kinds of experiments to be compared were made in each case according to a cyclic and compensating program. Much of the smell-work had to be thrown out, so that the results are very scanty. The first day's work with the syllables was also thrown out. The results are given in Table XI.

The difference in the results for the two kinds of material is very easy to explain. Subject 1 (who memorises with rather extraordinary facility) says that memorising syllables by the reconstruction method was easier because she 'saw the series spread out' before her and this spreading out 'helped in mak-

ing artificial connections and in finding mistakes.' On the other hand, Subject 2 says that memorising smells by the 'number-method' was easier because 'one mistake in regard to a number did not involve a lot of others as one mistake in placing did.' It is evident that the difference in the situation lay in the fact that Subject 1 could *see* the series-cards which she had placed, whereas Subject 2 had to *remember* the contents of her bottles. Neither subject reports any help from spatial associations. Subject 1 is distinctly of the 'ingenius type.' Examples of her auxiliary associates are: *Rig*—13—*unlucky*—*upset in a rig*; *hive*—13—*angry swarm of bees*; *nab*—13—*having something*

TABLE XI

To Show the Relative Difficulty of Actually Reconstructing a Series from Memory and of Reciting the Numbers of the Units

Subject.	Material.	Method.	N.	Nr.	Wr.	MV.	Np.	Wp.	MV.	Right positions or numbers % $W=1$,
1	{ Syllables } L. 18	"Numbers" Reconstruction	16	16	2.3	0.5	16	2.3	0.5	70.5
			16	16	2.0	0.1	16	2.1	0.2	64.2
2	{ Smells } L. 12	"Nos." Rec.	7	7	2.3	0.8	7	2.3	0.8	63.1
			8	8	3.0	0.8	7	3.0	0.9	53.1

*The subjects of these experiments were not the same as those of the last group. With one exception, no beginner served in two groups of experiments.

stolen. She complained that practice was counteracted by the 'multitude of associations forced upon' her 'from foregoing series' (inhibition).

Even if the series had been more numerous *the results of this group of experiments would be inconclusive as regards the special point at issue*. The statement of Subject 1, however, illustrates the fact that when a series is spread out before the subject's eyes, reproductive reinforcement is one of the advantages of the reconstruction method. (See page 33.) It is, however, impossible that this advantage is the all-important one for which we are looking. It is impossible because the smell-series at large do not share in it, and yet are very easily learned.

IV. The experiments of Group IX had the double purpose of testing the explanations (1) *by spatial localisation* and (2) *by multiplied tests of mastery*. (See page 9.) They were made by the writer in Göttingen in the spring of 1907 (*not* in Professor Müller's laboratory but privately). The material consisted of German normal series of nonsense-syllables written upon cards of $2 \times 2\frac{1}{2}$ inches and of small playing cards of $1\frac{1}{2} \times 2\frac{1}{4}$ inches. The subjects Ki. and Ho., were German women twenty-four and twenty-one years of age. Ki. had passed through the 'Mittelschule;' Ho. only through the 'Vorschule.' Ho. reads German fluently and writes an excellent script, but is slow in learning English words and in apprehending anything new and uninteresting. Laboratory experiments by the method of right associates had failed with her utterly.

So many different methods were used tentatively in these experiments and so few series were given by each method that space cannot be taken for a complete account. The experiments fall into four small subgroups. In regard to the first three subgroups the following statements must suffice:

a. In about half the series, the syllable-cards or playing cards were laid in rows of six or eight upon a table covered with black and were exposed simultaneously by lifting a long piece of cardboard. The subject was required to read the syllables or to name the cards aloud from left to right as from the page of a book. In the other half of the series the cards were presented successively according to our normal reconstruction method. This difference was introduced to try the effect of increasing the opportunity for forming spatial associations. In the first case the subjects were instructed to arrange the series 'just as it was before,' and in the second also they were required to rearrange it in horizontal rows of six or eight. No distinction was drawn between a correct and a perfect reconstruction. The cards when shuffled were handed over to the subject in a pack—not one by one.

Ki., who worked only with syllable-cards, learned slightly better when the cards were exposed simultaneously. Nevertheless, she preferred the other method, saying that when the cards were exposed simultaneously she 'saw too much at once.'

Ho. learned decidedly better when the cards were exposed successively except only in the very last of the experiments under discussion. An example of what the writer means by learning decidedly better is this: *Wr* for one set of 8 series of the length 12 with simultaneous exposure is 4.6 and for the corresponding set with successive exposure of members is 3.3. In the last experiments, those in which Ho. did better with simultaneous exposure, the material consisted of playing cards.

The results are altogether inconclusive as regards the advantage of fostering spatial relations. The writer believes that the chief difference between the two subjects lay in the fact that Ho. 'gabbled' through the series, when the cards were exposed simultaneously, whereas Ki. read slowly and deliberately. The rapid rate was quite enough to counterbalance any advantage (as regards the *number of repetitions* necessary) which Ho. might have derived from spatial associations. (The writer had already failed in the laboratory to teach Ho. to read with the metronome.) That Ho. in the last experiments learned better with simultaneous exposure is doubtless explained by the combined facts that she had learned to read attentively and that the playing cards were represented, as the writer had hoped, by lively images in especially definite spatial relations.

b. In half the series of the first subgroup the subjects were required to rearrange the series after each presentation as in our normal reconstruction method. After the learning of each series was mastered in this fashion, another series was given with a number of preliminary presentations which was double the number required for learning the last series by the normal method. If, for example, the subject reconstructed Series A on the third trial, then Series B was presented six times before the subject attempted to reconstruct it. If she then failed, she was required to rearrange the series after each presentation. Cases of such failure were numerous enough with both subjects to show that *the interpolation of tests of mastery greatly decreases the number of repetitions necessary for memorising, in the case of persons whose attention is not under practiced control.* For example, Ho. learned with simultaneous exposure and a test after each reading, 8 series of 12 syllables with an average of 4.6

presentations, whereas she required 2.1 extra presentations to learn the corresponding series which had been presented with an average of 9.2 preliminary presentations without tests.

c. In half the experiments of the second subgroup and in half the successive-exposure series in the third subgroup, the subject was required to rearrange the series after each reading or presentation. In the other half every two readings or presentations were followed by a test. Between these pairs of presentations, an interval of only 5 or 6 seconds elapsed. In the third subgroup, in one-third of the simultaneous-exposure experiments, a test followed each reading, in another third a test followed each two readings and in the last third a test followed each four readings. The subject read the syllables or named the cards over twice in about the same time which the experimenter required to present the cards once in succession. Thus two and four readings with simultaneous exposure are equivalent in time respectively to one and two successive presentations. The time required for reconstruction was never less than sixty seconds. In these experiments, rough time records were kept with a stop-watch and proved most important to the correct interpretation of the results. The interpolation of tests reduced the number of repetitions necessary but the tests themselves consumed so much time that the total time which they saved is inconsiderable. Furthermore, the series with simultaneous exposure were learned faster than the series with successive exposure, either because the subject read faster¹ or because spatial associations were promoted.

d. The purpose of the fourth subgroup of experiments was primarily (1) to test the explanation by multiplied tests but incidentally (2) to test the relative ease of naming the units from memory and of reconstructing the series. The material consisted of series of 13 playing cards. Four methods were used. In all these methods, the series-members were exposed in succession in the normal fashion. (1) In the first and third, every presentation was followed by a test; in the second and fourth every two presentations. (2) In the first and second methods, the subject was required to *reconstruct* the series, and

¹ See p. 193.

when she had succeeded in doing so correctly, she was required to recite the series and was prompted when she broke off. The syllables given without prompting were considered as 'right cases.' In the third and fourth methods, the subject was required after every one or two presentations to *recite* the series and was prompted whenever she named a wrong syllable or said 'Ich weiss es nicht.' This process was repeated until the whole series was recited without mistakes. The subject was then required to reconstruct it and the right positions were counted as 'right cases.' Only Ho. served as subject. In this subgroup, the results seem worth schematizing:

<i>Method.</i>	<i>N.</i>	<i>Trials.</i>	<i>MV.</i>	<i>Presentations.</i>	<i>Right cases.</i>
R.M. 1 trial to one presentation:	10	2.6	0.8	2.6	12.7
R.M. 1 trial to two presentations:	10	1.2	0.3	2.4	12.8
P.M. 1 trial to one presentation:	10	2.4	0.5	2.4	13.0
P.M. 1 trial to two presentations:	10	2.1	0.4	4.2	12.9

P. M. stands for prompting method, although, of course, the method used was by no means the orthodox form of the method of Ebbinghaus and Ephrussi.

The experiments by the reconstruction method show that, as the subject's practice increases, multiplied tests may cease even to reduce the number of repetitions necessary for memorising. (For explanations of this fact see page 167 below). If the number of repetitions is not much reduced by the interpolation of tests then the total learning-time must be greatly increased. In the experiments by the prompting method, in which a new task, that of recitation, was imposed upon the subject, the multiplication of tests reduced the number of repetitions, but figures which are not contained in the table show that the time consumed in learning with a test after every presentation considerably exceeded the time consumed in learning with a test after every two presentations. When a test was given after each presentation, a correct recitation was attained with fewer repetitions than a correct reconstruction.

The experiments of this group as a whole are altogether

inconclusive in regard to the value of spatial associations. As regards the multiplication of tests of mastery they suggest that what the learner saves in the number of necessary presentations, he may lose in the total time expended in memorising.

In any case, the experiments were not 'pure.' For when every presentation was followed by a test, each pair of presentations was separated by a much wider time interval than were pairs of presentations between which no test was interpolated. Hence, the experiments with the greater number of tests have the advantage of greater 'distribution of repetitions.' In the next set of experiments this complication is eliminated.

V. The experiments of Group X were devoted simply to testing the explanation *by multiplied tests of mastery*. These experiments were made in the year 1907-1908. A modified form of the method of retained members was employed. The series were English normal series of nonsense-syllables of the length 12. Four different sets of series were used in the experiments. The series were read aloud ten times by the experimenter, in trochaic rhythm. The reading was regulated by the beats of a metronome. Intervals of one second elapsed between syllable and syllable in each presentation and intervals of 64 seconds between the end of one presentation and the beginning of the next. In one set of experiments, a test was interpolated after every reading; in the other set after every two readings. Several subjects were employed at once. They were required to write the series vertically upon slips of paper ($3\frac{1}{8}$ by $5\frac{3}{4}$ inches) and at the end of each test-period to drop the slips face down into the boxes before them. The time allowed for writing the series was 60 seconds. When no test was interpolated between the presentations, a rather successful attempt was made to distract the subject's attention from the series by reading aloud a novel. This reading was done by an assistant experimenter. Four different novels were used. Among them was *Miser Hoadley's Secret* by Arthur W. Marchmont, which, though execrable trash from a literary point of view, is singularly adapted to hold a subject's attention when read from the beginning in fragments. Three series were learned at each sitting. The

experiments of the two sets were made according to a compensating program.

Two kinds of right cases, 'retained members' and 'sequences' were counted (as in the experiments of Group VII). In these experiments, however, since the series were presented to the ear, the subjects were allowed to substitute for the several elements of the syllables letters which *normally* have the same phonetic value—to write, for example, *caj* for *kaj* and *rise* for *rize*. It seems sufficient to indicate in the table of results only the numbers of retained members. The trend of the sequences is precisely similar. Since all the series were of the same length, averages instead of percentages may be given.

The subjects were G., Ka., Mo., Wi., and eleven beginners. G. had already had a year's practice in learning syllable-series by ear. Ka., Mo and Wi. were members of a second-year class in psychology. Of the beginners, one was identical with Subject 2 in Group VIII. The work of the beginners who worked at the same sittings is massed. G. acted as reader except when she herself and Wi. were subjects. In this case, the syllables were sometimes pronounced incorrectly, and sometimes inconsistently from reading to reading, and moreover, they were often read with such breaks and changes in the rhythm as greatly to disturb G. whose grouping-habit was strong. The results are contained in Table XII.

From these figures, the following conclusions may be drawn:

1. The tests after the first presentation of a new series increased the amount learned on the second presentation.

2. In the method of many tests no extra trial except the first appreciably facilitated the learning of the series. For on comparing the results obtained by the two methods, one finds that the difference between the figures for the trials given after the fourth, sixth, eighth, and tenth presentations is never greater and is often less than the initial difference between the figures after the second presentation. The one set of figures never lags behind the other by more than this initial difference.

3. The numbers of members retained after the same number of presentations approximate in the two methods sufficiently to show that about the same number of repetitions would be

TABLE XII
To Show the Effect of Multiplying Tests in the Process of Memorising.

SUBJECTS.	NUMBER OF PRESENTATIONS FOR ONE TEST.	N	AVERAGE NUMBER OF RETAINED MEMBERS WHEN THE NUMBER OF PRESENTATIONS WAS									
			1	2	3	4	5	6	7	8	9	10
G.....	1	18	6.3	10.1	11.1	11.3	11.3	11.5	11.6	11.6	11.7	11.6
	2	18	9.2	11.0	11.5	11.8	11.7
Ka.....	1	16	4.0	6.2	7.7	8.5	9.5	9.9	10.3	10.5	10.8	10.9
	2	17	5.2	7.5	9.0	10.1	10.8
Mo.....	1	16	4.0	5.7	6.9	7.3	8.4	9.0	9.7	10.1	10.7	10.9
	2	17	4.7	6.9	8.3	9.6	10.1
Wi.	1	18	4.0	6.1	7.9	8.9	9.4	10.7	11.3	11.1	11.3	11.4
	2	18	5.7	8.5	9.9	10.4	11.0
4 beginners....	1	36	3.4	5.3	6.6	7.4	8.4	9.0	9.2	9.5	9.8	9.9
	2	35	4.3	6.5	8.1	9.2	9.9
7 beginners ...	1	62	3.8	5.3	6.1	7.2	8.0	8.4	8.9	9.1	9.4	9.6
	2	61	4.9	6.9	8.3	9.1	9.6

required for complete mastery with either method. Thus, it would seem that *when the time which elapses between presentations is the same, whether tests of mastery are interpolated or not, then the mere testing process in itself has little effect in reducing the number of repetitions required for memorising.*

From these last two groups of experiments (IX and X) taken together one may conclude:

First, that the 'explanation by multiplied tests' largely reduces itself to an explanation by one form of 'long intervals' (see page 10); but

Second, that at least the first test does itself raise the level (Niveau) of mastery, or does alter the learning process in the next presentation or does both, and that experiments by this modified form of the method of retained members are not, therefore, strictly comparable with experiments made by the standard form (see page 58); and

Third, that the interpolation of tests has most value to the poorest memorisers.

This third conclusion requires further discussion. A test involves an extra attentive repetition of that part of the series which the subject already knows. Therefore, two presentations with an interpolated test mean really two complete repetitions *plus* a partial repetition which has an off-set in the impressing of wrong syllables or mistakes of order. To the feeble memoriser the additional opportunity for clinching weak associations is doubtless one great advantage in the multiplication of tests. Thus, one subject in these experiments reported that she found it a help to *write* the syllables as often as possible. This subject memorised slowly, and had a habit peculiar to poor memorisers, that of relapsing in the process of learning a single series.¹ The good or practiced memoriser holds what he has once acquired at least until the whole series is learned, and has no need to keep impressing on himself syllables and sequences which he has once mastered. The feeble memoriser is like a person who is trying, unsystematically and with weak hands, to take up and carry many small packages at one load.

¹ The observation that relapses are peculiar to poor memorisers is based on the examination of many experimental records.

He drops some syllables as he picks up others. A test acts like a careful packing of bundles into pockets during a pause in which no others are taken up. On the other hand, the practiced memoriser grasps the articles in his load firmly at the first and packs them with system as he takes them up, so that pauses for adjustment are unnecessary. For G., the most practiced learner, the help afforded by the tests seemed mainly a matter of saving and profitably distributing her attention. When her knowledge of a series was tested, she located the gaps in it, and on the next presentation could save her energy for filling them in. Thus her attention was certain not to sag in the wrong place. But even this advantage is, like the extra opportunity for clinching associations, unimportant to the practiced subject who learns to test himself even in the course of closely successive presentations.¹ G. distinctly preferred learning by the method with the 'rests.' The first tests involved some exertion, the last few out of ten were simply a bore. As the figures show, toward the last of the series of repetitions, G. could reproduce perfectly rather more series-members with the 'rest-method' than with the other. If the series had been read properly she would have required for perfect mastery by either method far fewer presentations than ten (see the work of G. contained in Tables XI—XIII).

Most of the other subjects agreed with G. in preferring the method with one test for two presentations. The reasons for this preference were not always clearly stated. Thus, two of the beginners reported that they preferred the 'method with the reading' *because* there were two presentations to one test. One beginner, who memorised with marked facility, said that she preferred this method because she was 'not so frightened.' This subject resembled G. in finally accomplishing rather more with the 'rest-method.' Another beginner stated that she preferred this method because in the first of a pair of presentations she could correct mistakes and in the second could learn new syllables. At least two of the subjects preferred the method

¹ With G. this process is partially visual and is *somewhat* like comparing an imperfect copy of a series of colored spots with an original, and filling in the blank spaces.

with the many tests. Mo. reported that during the novel-reading, in the other method, her attention wandered and was with difficulty brought back to the series at the next presentation. These statements of preference were made without knowledge of the results obtained by the two methods.

To recapitulate again, in brief, multiplied tests involve three advantages for the learner:

1. They interpolate considerable intervals of time between successive presentations. Whatever else these intervals accomplish (see page 202 below), they at least relieve the breaking-strain upon attention.

2. They give the learner a special opportunity—if he needs it—to hammer down the pile-like foundations upon which his knowledge of the series is gradually built up.

3. They give the learner a special opportunity to find the gaps and weak places in his knowledge.

Nevertheless, these advantages do not prove that multiplied tests save *time* in learning the series.

An incidental observation in connection with this group of experiments is the following: Nearly all the subjects, including G., were inclined, at least until a series was pretty well mastered to write down the last members first. The syllables were not in general, written in order from last to first, but the last few were written first and in their proper order. With G., at least, this reproduction depended on the auditory-memory after-image. The procedure was for G. distinctly disadvantageous although it is the line of action which subjects in the method of retained members take on impulse. By the time the subject G. was ready to write the first members of the series she had very often forgotten them, either owing to the interval which had elapsed since they were presented or owing to retroactive inhibition. The first members of a series tend strongly to recall the later but the later members have much less power over the earlier and it would even seem that the later may actually inhibit the earlier.¹ It is, therefore, prudent to take no risks

¹ Even granting the importance of the 'initiale Reproduktionstendenz.' See Müller and Pilzecker, *op. cit.* pp. 199–204.

in regard to the initial members of a series which is rapidly presented and which must be reproduced and not merely reconstructed. (See page 207.)

VI. The experiments of the eleventh group are few in number but are nevertheless important. It appeared highly desirable to probe the explanation by multiplied tests with experiments on G. by the reconstruction method, and to do so in such a way as to isolate the effect of the tests as such from the effect of the intervals which they of necessity interpolate between presentations. The effect of multiplying tests in the reconstruction method must be the greater because the process of reconstruction must foster the 'criss-cross' (supplementary intrinsic) associations whose importance is peculiar to this method. The experiments of this group have been made in the month before the Christmas holidays of 1908 and are the most recent experiments covered by this monograph. Colors were chosen as material because they are hard for G. to discriminate and therefore make especially important such opportunities for review and comparison as are offered in the tests. The series consisted of 32 members which were arranged in four horizontal rows of eight each. The placing rules were the same as in the older experiments but no importance was attached to certainty of correctness. The first member of the series was not handed to the subject as the first so that the series were *bona fide* series of 32 members. Three sets of experiments were made according to a compensating program. In the first set, the procedure was practically normal except that eight minutes were always made to intervene between the end of one presentation and the beginning of the next, whether the whole of this period was required for the reconstruction and recording of the series or not. In the second set of experiments, two presentations were made in immediate succession whenever one presentation would be made by the normal method, and eight minutes were made to intervene between the end of one pair of presentations and the beginning of the next. The procedure in the third set of experiments differed from that in the second, only in the interpolation of eight minutes, unoccupied by any attempt at reconstruction,

between the members of each pair of presentations. Symbolising *presentation* by *P.*, *trial at reconstruction* by *T.*, *recording* by *R.*, and *vacant interval* by *V.*, and representing minutes in round numbers by numerals in parenthesis one may schematise the three procedures as follows:

1. P (2) T (5) R (3) P (2) T (4) R (2) V (2) P (2) T (4).....
2. P (2) P (2) T (5) R (3) P (2) P (2) T (4) R (2) V (2).....
3. P (2) V (8) P (2) T (5) R (3) P (2) V (8) P (2) T (4).....

The third procedure was called for brevity the method of 'double presentations with rests' and the second the method of 'double presentations without rests.' The subject spent the vacant intervals in chatting with the experimenter. The times occupied by the various phases of the experiment were kept with a stop-watch; the color-squares were presented rather more quickly than at the rate of one per four seconds; two minutes is an ample representation of the time occupied in presentation. Eight minutes was approximately the time required for the first reconstruction and recording of the series; therefore, in order that the intervals might be uniform, eight minutes were allowed to elapse whenever an interval was introduced between presentations. We may safely say that the reconstruction of the series took at least twice as long as the presentation. In reporting experiments of this character, only round numbers seem required for the time-estimates.

The results are as follows:

Method.	N.	CORRECT RECONSTRUCTION.			PERFECT RECONSTRUCTION.			RS%.	
		Trials MV.		Pres'ns	Trials MV.		Pres'ns	W = 1.	W = 2
Normal.....	10	3.3	0.8	3.3	4.0	0.8	4.0	19.0	71.0
Double pres'ns without rests	10	2.3	0.4	4.6	3.1	0.8	6.2	56.5
Double pres'ns with rests....	10	2.3	0.4	4.6	2.3	0.4	4.6	58.4

From the figures for the first and third methods, it would seem that multiplying tests saves presentations, but it would seem that presentations are not saved to the extent that trials

are lavished. In any method in which the presentations go in pairs, the possibility always exists that the last presentation of any series was unnecessary, and even as the figures stand the numbers of presentations approximate more closely than the numbers of trials. In these two procedures the intervals between presentations was the same whether a trial had been given or not. If the figures for the second procedure are not influenced by the accidental difficulty of the series,¹ then the lapse between presentations of the time ordinarily occupied by reconstruction reduces the number of presentations necessary to the perfect reconstruction of the series whether a trial has been given or not. Thus, this small group of experiments leads one to the same conclusions as those suggested by the preceding group. The advantage of multiplied tests is in part explained by the time-interval interpolated between presentations. Nevertheless, the learner does make some actual advance through the testing process. But on the other hand, trials have no magical effect in reducing the number of presentations required. The figures obtained by the several methods are not very different, and in these last experiments with G., the number of presentations necessary for learning is much more uniform than the number of trials.

G. greatly preferred the method of double presentations without rests to either of the others used in the last group. This method was quick on the one hand, and on the other eliminated the irksome guessing of a reconstruction after only one presentation. The average time occupied for learning a series by these three methods may be estimated, on the basis of the round numbers of minutes given in the scheme, as 29, 23.6 and 42 minutes respectively for a correct reconstruction, and as 36, 33.2 and 42 minutes respectively for a perfect reconstruction. These times include all the intervals essential to the different procedures, and also the time occupied by the last reconstruc-

¹ The facts must again be noted that when series of 30 or 40 members are given the number of series given must be relatively small, and that long series must be given by this method if, with a practiced subject, the numbers of presentations are not all to approximate to unity. The learning of 30 series of 32 members represented a time-expenditure of about 18½ full hours.

tion, but not the last recording-time. It is noticeable that the method which makes the worst showing as regards the number of presentations, makes the best showing both as regards the total time required and as regards the preference of the subject.¹

VII. The experiments of Group XII are the most important of the experiments which were made to find an explanation of the peculiar results obtained by the reconstruction method. They are the most important because they were the most extensive, and because they go farthest toward furnishing the explanation desired.

In this group of experiments four questions are at issue. The first of these questions belongs only to the first subgroup of experiments but it may be enumerated among the four because it determined the method of the first subgroup, and the method of the first subgroup determined the method of the other subgroups.

This first question was as follows: What could G., who could memorise such extraordinarily long series by the reconstruction method, accomplish with the method of Ebbinghaus and of Müller and Schumann? The experiments of this first subgroup followed, logically and chronologically, upon the work of the fourth year of the reconstruction experiments. This fourth year had been devoted to the experiments with parallel use of smells, colors, and nonsense-syllables, and to the control-experiments whose object was to determine how far G. in memorising series-order memorised series-members as well. It was proved that G. could master very long series of nonsense-syllables even rather better than very long series of smells and of colors. It was shown, moreover, that when she had mastered the order of a smell or syllable-series, she could also name nearly all of the smells and syllables in order. The question then arose as to whether her achievements in learning long series were conditioned by the reconstruction method itself.

¹ The eight-minute periods of rest are included in the learning-time because they both postponed the completion of the learning and had an effect on the number of presentations necessary.

In the subgroup of experiments with which we are now dealing, the first subgroup of the twelfth group, four different series-lengths including the length 81, were used. The syllables were presented to the ear instead of to the eye in order that the conditions might be as different as possible from those of the experiments with syllables by the reconstruction method. For the sake of uniformity, this auditory mode of presentation was retained throughout the whole of the twelfth group of experiments.

The method of complete memorising was chosen for the experiments of this twelfth group, not merely for the sake of bringing G.'s work into comparison with that of Ebbinghaus and of the Müller and Schumann subjects, but also for the sake of studying time intervals. In our own reconstruction method the time required for rearrangement cannot well be controlled, although with the use of suitable apparatus it is perfectly possible to vary accurately the intervals at which the series-members are presented. It seemed to us that the effect of time-intervals could, for our special purposes, be studied satisfactorily only with a method in which oral reproduction at a given rate is required of the subject. Therefore, since we wished results in terms of repetitions necessary for complete memorising, we were confined by the conditions of our problem to the Ebbinghaus method with only minor variations. The exact nature of the interval-problem with which we were concerned in this group of experiments can best be stated after two other problems have been formulated.

The second question at issue is as to whether or not the degree to which recitation enters into the several repetitions of a series makes much difference in the number of repetitions which are necessary for learning it. The meaning of repetition in the method of complete memorising must be emphasised and should be kept clearly in mind. A repetition is a traversing or a going-over of the series and may consist partly of presentation and partly of recitation. Only the very first and last repetitions must be all the one or the other: the first repetition (really not a repetition in the strictest sense) *must* be all presentation; the last repetition must be all recitation. Experimenters by this method

assume that presentation and recitation have the same value for learning; that is to say, they assign the same value to a repetition which consists three-quarters of recitation and one-quarter of presentation and to a repetition which consists one-quarter of recitation and three-quarters of presentation. The question is whether or not the same value ought to be assigned to the presentation and recitation. One of the criticisms made by Münsterberg upon the method described by Ebbinghaus in *Ueber das Gedächtnis* was to the effect that the repeated attempts to recite a series which was incompletely learned introduced into the procedure an element difficult to control, and that recitation-repetitions and presentation-repetitions ought not be brought into simple coördination.¹ Müller and Schumann made special experiments to determine whether one of their subjects (Dr. Pilzecker) did or did not, by dint of his partial recitations of the series from the beginning, make more lasting associations between the syllables in the first half than between those in the second half of series. The result was negative, but has, of course, only an individual value.² Furthermore, it does not follow from the case of this subject, even if it be typical, that attempts at recitation do not diminish the number of repetitions necessary to memorise. For that strengthening of associations which is involved in the first perfect recitation of a series *may* not be the same kind of strengthening which insures the tenacity of associations even for twenty-four hours.

To answer the second question, that is, to test the value of recitation in learning we introduced prompting into some of our experiments with the method of complete memorising. In some sets of experiments, the orthodox method of complete memorising was used without any variations except in the matters of auditory presentation and varied time-intervals and in the fact that the subject was *required* to try to recite the series as soon as it had been presented once. As soon as the subject made a mistake or failed to name a syllable in a given length of time after the naming of the next preceding, the experimenter named the proper syllable and then presented the rest

¹ *Beiträge zur experimentellen Psychologie*, 4, p. 124.

² Müller and Schumann, *op. cit.*, pp. 187-190.

of the series without giving the subject another chance to recite until she had read to the end. The subject was then required to try again, and the rest of the procedure was repeated until the whole series was recited without mistakes and without hesitation beyond the limits of a certain time interval. This method will be called our *standard* method of complete memorising. In the other sets of experiments, the method was exactly the same except that the subject was prompted whenever she hesitated or made a mistake and was then allowed to recite until she again broke down. This method will be called the *prompting* method of complete memorising. The purpose of the prompting variation is indicated in the following assumptions: first, that if partial recitation in itself, that is, apart from the fact that it involves a partial repetition or traversing of the series-sequences, reduces the number of repetitions necessary for memorising, then the subject should learn more quickly by the prompting method of complete memorising than by the standard method; and second, that if recitation, as such, facilitates learning by the method of complete memorising then the multiplied tests of our reconstruction method must have been a factor in reducing the number of repetitions necessary for memorising series-order.

The third question of the group of experiments with which we are dealing is concerned with the emotional disturbance which the subject may feel in the method of complete memorising as the result of having to recite the series at a rapid rate or even of having to repeat it at any fixed rate at all. Can the number of necessary repetitions be decreased by allowing the subject a longer time or unlimited time in which to recite the series? This question bears upon the validity of the explanation of the easiness of the reconstruction method by leisurely recall, that is, by the fact that the subjects were allowed all the time they wanted for the rearrangement of the series. The question cannot be solved without altering time intervals in recitation and yet it is not concerned at all with lapse of time *per se*.

The fourth question, on the other hand, has to do only with lapse of time as such. It deals with the effect of altering the

time which elapses between each series-member and the next (between *a* and *b*, *b* and *c*, *c* and *d*, and so on) in traversing the series in either fashion, presentation or recitation. It waives any possible difference between presentation and recitation. For the sake of clearness in discussion, it should be noted that the experiments of this present group are not concerned, like those of the last group, with the interval which elapses between the end of one complete repetition and the beginning of the next (the interval between *z* and *a*). This interval in the first two subgroups of the experiments of the present group was not varied at all, and in the last two subgroups it was varied very slightly.

In brief, the four-fold object of the twelfth group of experiments was to gauge the degree to which G.'s achievements in learning very long series were conditioned by the peculiarities of the reconstruction method (the first question), and to test the validity of three theories in regard to the small number of repetitions necessary for learning series-order by our normal reconstruction method (the second, third, and fourth of the questions outlined above). These theories are the explanations *by multiplied tests*, *by leisurely recall*, and *by slow rate of presentation*. (See pages 9 and 10.)

The tables of results must be preceded and will be followed by sundry descriptive and explanatory details. The experiments, as already indicated, fall into four subgroups. The experiments of the first two subgroups were made in the academic year 1905-1906; those of the last two subgroups in the spring of 1908 (after the experiments of Group X). The subject in the first and third subgroups was G.; the subjects in the second and fourth subgroups were beginners. In the first two subgroups, series of the hit-or-miss type and of different lengths were used; in the last two subgroups, English normal series of the lengths 12 and 18. In the first two subgroups, the intervals between the syllables in presentation and recitation were timed roughly with a watch; in the last two subgroups, both presentation and recitation were timed with a metronome. In the first two subgroups the series were read without rhythm, and the subjects were allowed to recite the series as fast as they

pleased even in cases where they were also allowed to name the syllables at five-second intervals. The experimenter, however, named the next syllable at once if the subject hesitated beyond the limit of the interval allowed. In the last two subgroups, those series which were presented at the rate of one syllable to a second, were read (not always perfectly) in trochaic rhythm. The series which were presented more slowly were not read rhythmically. In all cases, however, with one exception which will be stated later, the subjects were required to name the syllables upon certain metronome-beats—on successive beats, on every second beat, or on every fourth beat as the case might be. The metronome was so set as to beat once to a second. Verbal counting of a small even number of beats is not necessary, after the subject has once put rhythm into the beats. "Tick-tack, tick-tack" says the clock in the nursery-rhyme and both subject and experimenter might watch as well as listen to the metronome if they chose. If a subject failed to name the proper syllable on the proper beat, the experimenter named it, not on the first, but on the second beat afterwards. That is to say, if the syllable was due on a 'tick-beat' it was named on a tick-beat; if it was due on a 'tack-beat' it was named on a tack-beat. Thus, the rhythm was not broken by the prompting. Rhythmical recitation, however, was not *required* of the subjects. In the first two subgroups the time between presentations was not intentionally varied although it was not kept rigidly uniform, and it sometimes amounted to a minute, or even on rare occasions, with long series, to two or three minutes; but any serious interruption of the subject's attention caused the series to be discarded. In the third and fourth subgroups the time between repetitions was carefully regulated and (except by trifling accident) never exceeded nine seconds. Details will be given below. In the first two groups of experiments the first syllable was named by the experimenter at the beginning of every recitation as a 'starter'; in the last two groups it was not.

In the experiments of the third and fourth subgroups, two experimenters were employed. The first experimenter gave

her whole attention to reading the series and preserving the proper intervals; the second experimenter recorded the points at which the subject failed and the number of repetitions required. Tables XIII–XV should here be examined.

In explaining Tables XIII to XV it remains merely to outline in words the different sets of experiments and to indicate the method by which the figures for the 'learning-time' were obtained. As in the earlier experiments, *L* stands for series-length; *N* for the number of series in the set; *W* for the average number of repetitions required for memorising and *MV* for mean variation.

The experiments of the first subgroup are covered by the first part of Table XIII. The subject was G. The subgroup includes twenty-four sets of experiments. The experiments of these sets were made according to a compensating program. Series of four different lengths were used in six different ways. That is to say, there were four different arrangements of intervals with the standard method and two different arrangements with the prompting method,¹ making six different procedures in all.

The second subgroup of experiments is covered by the latter part of Table XIII. Eight beginners served as subjects. Other beginners served as experimenters. The results are massed according to the principles stated on page 49.² Eight different sets of experiments were made according to an alternating program. Two different series-lengths were used in four different ways, that is, with two different methods and with two different arrangements of intervals under each method.

The experiments of the third subgroup fall into four different divisions which were made one after another. In each division two sets of experiments were made according to a compensating program. The subject was G. The results of the first divi-

¹ We may perhaps be permitted in this section to call this latter method for short the prompting method, although it is not the prompting method proper. (See pp. 55.)

² In these experiments, the results were massed not only in the ordinary way, but also by averaging the averages of individuals. The difference between the two sets of results is surprisingly small, and does not alter in the slightest degree the conclusions to be drawn.

TABLE XIII.
To Show the Effect of Prompting and of Alteration of Time Intervals between Series-Members in the Method of Complete Memorising
 Experiment-Group XII, Subgroups 1 and 2

Subject.	Method.	INTERVALS.		L	N	W	MV	L	N	W	MV	L	N	W	MV
		Pres.: Secs.	Rep.: Secs.												
G.....	Standard	5	5	11	3	1.7	0.4	21	4	3.8	1.8	41	4	4.0	0
		1	1	11	4	5.8	0.4	21	4	10.3	2.3	41	4	14.0	2.0
		5	1	11	4	3.5	0.8	21	4	4.5	0.8	41	4	10.5	6.0
		1	5	11	4	3.3	0.8	21	4	5.3	1.9	41	4	7.5	2.5
Eight beginners	Prompting	5	5	11	3	2.3	1.1	21	4	3.5	0.5	41	4	4.3	0.4
		1	1	11	3	5.0	0.7	21	4	5.8	1.3	41	4	10.3	1.8
		5	5	11	75	4.1	1.3	21	75	6.9	1.9
		1	1	11	73	6.8	2.3	21	73	12.5	4.3
	Prompting	5	5	11	75	4.2	1.2	21	75	7.4	2.1
		1	1	11	75	5.7	1.6	21	75	9.8	3.1

TABLE XIV

To Show the Effects of Prompting in the Method of Complete Memorising.

Experiment-Group XII, Subgroup 3 and 4

Division 1. Intervals in Presentation and Reconstruction: 4 Seconds

<i>Subject.</i>	<i>Method.</i>	<i>L</i>	<i>N</i>	<i>W</i>	<i>MV</i>	Mean learning time: secs.	Mean time per repetition: secs.
G.....	Standard	18	20	4.9	1.6	341	70
	Prompting	18	20	3.8	0.8	278	74
B.1.....	Standard	12	20	5.4	1.5	246	46
	Prompting	12	20	4.9	1.1	244	50
B.2.....	Standard	12	20	8.6	3.8	394	46
	Prompting	12	20	7.6	2.7	391	51
B.3.....	Standard	12	15	8.9	2.2	406	46
	Prompting	12	15	10.0	3.6	535	54
B.4.....	Standard	12	20	6.8	2.0	311	46
	Prompting	12	20	6.5	1.9	332	51

sion stand at the beginning of Table XIV. The two sets of experiments differ only in the methods used, the standard and the prompting methods. The series were all of the same length and the same (four-second) intervals were used. The results of the second, third, and fourth divisions occupy the first part of Table XV. The sets of experiments differ only in the matter of intervals. The series were all of the same length, and the standard method was used throughout. In the second division, all the series were recited slowly, that is, at four-second intervals, but half were presented slowly and half rapidly, that is, at one-second intervals. In the third division, all the series were presented rapidly, but half were recited rapidly and half slowly. In the fourth division half the series were presented and recited slowly and half were presented and recited rapidly.

The experiments of the fourth subgroup fall into three divisions. The subjects were the beginners who are symbolised by B. with numbers from 1 to 10. Other beginners served as experimenters. The experimental programs were of the compensating type. The experiments of the first division are exactly analogous to the first division of the third subgroup. The results are included in Table XIV. The experiments of the

TABLE XV.

To Show the Effect of altering the Time-Intervals between Series-Members in the Standard Method of Complete Memorising

Exp.-Group XII, Subgroup 3, Divs. 2-4 and Subgroup 4, Divs. 2 and 3.

Subject.	INTERVALS.		L	N	W	MV	Mean learning time: secs.	Mean time per repetition: secs.
	Presentation: secs.	Reproduction: secs.						
G.....	4 1	4 4	18 18	18 18	3.7 6.6	0.9 1.5	259 211	69 32
	1 1	4 1	18 18	18 18	6.5 10.8	1.0 3.0	199 203	31 19
	4 1	4 1	18 18	18 18	4.3 10.4	0.9 1.2	299 196	70 19
B. 5.....	4 1	4 4	12 12	20 20	6.2 9.9	0.8 2.8	281 251	46 25
	4 1	4 4	12 12	20 20	5.9 8.4	1.7 1.6	269 221	46 26
B. 6.....	4 1	4 4	12 12	20 20	5.3 9.4	1.4 2.4	242 224	46 24
	4 1	4 4	12 12	20 20	5.3 9.4	1.4 2.4	242 224	46 24
B. 7.....	2 2	2	12 12	20 20	9.1 8.7	1.8 2.1	24 23.4
	2 2	2	12 12	20 20	12.0 9.0	4.0 1.7	24 28.2
B. 8.....	2 2	2	12 12	20 20	9.0 6.1	5.0 1.4	24 28.2
	2 2	2	12 12	20 20	9.0 6.1	5.0 1.4	24 28.2

second division are exactly analogous to those of the second division of the third subgroup. The results are found in Table XV and are there labelled as the work of beginners 5 to 7. The experiments of the third division differ from all the other experiments. The results stand at the end of Table XV and are marked as the work of beginners 8-10. In both the sets of experiments made upon each subject, the syllables were pre-

sented at intervals of two seconds. In one set, however, the subjects were required to recite the syllables at this same rate whereas in the other set they were not in any way limited in the time of recitation except that they were required to pronounce the syllables on metronome-beats, and therefore could not recite them at a faster rate than one per second. The time required for reproduction was measured with a stop-watch.

In the first two subgroups (Table XIII) the time between repetitions was not intentionally varied although it was not kept rigidly uniform. The interval often amounted to a minute or even sometimes to two or three minutes in the case of the very long series. In this case the subject G. occasionally suffered from the same nervous disturbance which was involved in the reconstruction of the long smell series; and she was accustomed to delay the beginning of a recitation until violent heart-beat subsided. In the third and fourth subgroups (Tables XIV-XV) the time between repetitions was carefully regulated. When presentation and recitation were both slow, that is, when the syllables were named on every fourth metronome-beat, the experimenter gave the signal 'Now' on the fourth beat after the one on which the series ended, and the subject was required to name the first syllable on the fourth beat after the signal. In this case, there was no rhythm to be preserved. When both presentation and recitation were fast, that is at the rate of one syllable to a beat, the experimenter said 'Now' on the first beat following the last syllable and the recitation began on the second beat after the signal. Thus, in the case of the 'slow-slow' series the interval between presentations was eight seconds and in the case of the 'fast-fast' series was three seconds. The last number of seconds was uneven because the series ended on a 'tack-beat,' and yet the rhythm required that the signal 'Now' should be given and that recitation should be begun on a 'tick-beat.' In the case of the fast-slow series, the interval was seven or nine seconds, (seven seconds by a rule, not always carried out). When the syllables were named on every second beat, the signal was given two seconds after the last syllable-beat. If the recitation-time was limited the subject was required to begin on the second beat after the signal; if the time was not

limited she might begin either on this beat or after it as she chose. Thus the total interval was at least four seconds.

The difference in these intervals is very important as regards the subject's emotional condition, almost more important than the intervals within the series. The subject who is liable to stage-fright finds it all-important to get a recitation once under way. But aside from its emotional effect the writer believes that the difference in these intervals may be almost disregarded, since the intervals themselves were largely occupied in the counting of the beats (not necessarily a verbal counting).

Except in the case of the experiments in which the recitation-time was unlimited, the learning-time for each series was calculated on the basis of the length of the intervals, of the number of repetitions necessary, of the number of times the subject was prompted, and also, for the series in which the presentation-intervals differed from the recitation-intervals, on the basis of the points at which the subject failed in the various repetitions. The intervals between the repetitions were left out of account. One imperfect repetition of a series of 18 members, with four second intervals by the standard method, occupies 70 seconds since there are 17 four-second intervals and an extra two-second interval at the point at which the subject fails. If five repetitions are required the learning time is 348 seconds, that is, 4×70 seconds *plus* the 68 seconds consumed in the first presentation. In these experiments the final repetition, which consists entirely of recitation, was not counted in the number of repetitions necessary for memorising; therefore to make the results comparable with those of Müller and Schumann 1 must be added to all the numbers given under *W* in the tables. A single repetition of a series of the same length at the same intervals, if the subject were prompted five times, would consume 78 seconds since 5 extra two-second intervals must be added to the 17 four-second intervals. Further details seem unnecessary.

From the results of the twelfth group of experiments as a whole, the following conclusions may be drawn:

1. The subject G. *did succeed* in learning series of 41 and of 81 nonsense-syllables by the method of complete memorising

even when the syllables were presented and recited approximately at one-second intervals. To learn a series of 81 members by this method and at this rate required about three quarters of an hour. To learn a series of 81 members by the same method when the syllables were presented and recited at five-second intervals required about an hour. To learn a series of 81 syllables by the reconstruction method required, exclusive of the last rearrangement, about an hour and a quarter. We may, therefore, conclude without hesitation that *after the practice obtained with the reconstruction method* (three years and a half in all and one year with nonsense-syllables) *G. could master series of syllables in an even shorter time by the method of complete memorising, whether the members were presented rather more slowly than the syllables in the reconstruction method or about three and a half times as fast.*

Unfortunately, it can now never be determined whether or not the technique by which the subject performed these feats could have been acquired by any other method than our own reconstruction method with its slow presentations and its premium on spatial associations. The subject herself believes that she would *in time* have acquired a very similar technique with the method of complete memorising, since she had always (so far as she can remember) been especially prone to making and to holding fast by spatial associations.¹ In any case, the subject's procedure in learning by the method of complete memorising was certainly closely similar to her method in memorising by the reconstruction method. She saw the color-images (see pages 106 and 110) of the syllables in one long line running from left to right sometimes at reading distance, sometimes at about the height of the table on which she had been wont to arrange the syllable-cards and colors. The line in the case of longer series

¹ For example, the rules and paradigms of the Greek grammar always appear in definite page-positions, although examination of the grammar first and most diligently conned shows that they have often shifted out of their original positions. The rule for 'conditions contrary to fact' occupies in the grammar the middle of the left-hand page; the subject sees the rule at the bottom of the right-hand page. Here among dusky lines within a white margin the colors of $\epsilon\iota$ and the $\acute{\alpha}\nu$ glimmer in definite positions. The two breathing-marks and the accent to $\acute{\alpha}\nu$ are black and are the only characters visualised.

ran further to the left and to the right than in the case of the shorter series. The subject knew at starting the length of the series and located the first syllable according to the series-length. In the case of the long series, the line was apt to tilt somewhat, lying outward at the extreme left and then running inward so that the right end sometimes seemed to be a little behind the subject.¹ The line was not all seen at once but the several syllables had places in it. The syllables were massed into unit-groups of four except that the last two in every ten syllables, after twenty, paired off by themselves. The unit-groups were themselves grouped in the longer series; the first larger group included twelve, the next eight, and all the rest contained ten. The subject read the syllables off from the colored spots with the aid of auditory and tactile images. The points of greatest danger were at the junctures of the groups. While reciting each group of four the subject looked on for the initial of the next. If she could not see it, disastrous perturbation nearly always set in. Special attention was, therefore, given to 'locking together' the terminal member of one group with the initial of the next. The chief difficulty which the subject experienced when the series were presented rapidly was in getting the syllables quickly and correctly into their groups. The transcending of this difficulty was the chief effect of practice.

The experiments of the third subgroup were made after an

¹ The character of the line may have been determined by the subject's number-form. All the lower numbers lie on a line from left to right—the pinkish-brown of the eight, the green of the nine, and so on, flashing out in turn—but numbers over a million are thought of with reference to one another as lying on a line running forward at right angles to the first. The word *eternity* is gray. The subject thinks of eternity as a gray band running forward and backward like the higher numbers and always on her right. Past time is in front, future time is behind. (She has her back toward the future!) The present is in line with her right shoulder. Both ends of the line are lost in masses of gray cloud. Childish as these images are, they show how ingrained with this subject are pseudochromæsthesia, symbolic visual representation, and spatial projection, and how the three tendencies intermingle. The *Ding an sich* is a pink and white ball alone in a gray mist. *Sich* is pink. The *Anstoss* of Fichte is a brownish wall against which the vaguely ball-like ego bounces. *Anstöss* is brown, on the whole. Images undoubtedly become more stereotyped when they have once been described in words, but none of those just mentioned were deliberately formed.

interval of twenty months during which the subject G. had had very little practice. Her self-observation showed that she was somewhat out of practice. The position of the colored spots was less distinct and less fixed, the grouping was less easy and less marked, and the subject grasped more at auxiliary (mnemonic) associations than ever before in memorising any kind of material. The numerical results also show a slight fall in the practice-level. (The results for the series-length 21 in Table XIII should be compared with the results for the series-length 18 in Tables XIV and XV. Of course, the comparison should be made only between sets of series learned by the same method with parallel arrangement of intervals.) These experiments of the third subgroup are the last experiments made upon G. except the short sets of control-experiments described on page 146 and the experiments of Group XI. The latter show but slight loss of practice as compared with the earlier color-work.

The second, third, and fourth conclusions all relate to explanation offered for the peculiar results of the reconstruction method. (See pages 173 and 176.)

2. Series which were repeated at the rate of one syllable *per second* were learned with a smaller number of repetitions (and in a shorter total time) by the prompting than by the standard method of complete memorising. In the case of series which were repeated at the rate of one syllable every four or five seconds the prompting method has no constant advantage. Even in the case of rapid repetition, its advantage in the series of eleven members is very slight. We may, therefore, infer that *with the series-lengths ordinarily used in the method of complete memorising the degree to which recitation enters into the repetitions is unimportant, but not quite immaterial when the question at issue is delicate.* But we may also conclude that *the results of these experiments are purely negative as regards the value of the multiplication of tests in the reconstruction method in which presentation is so very leisurely.*

The details of the case in the experiments which we are now considering are as follows: The beginners in the second subgroup of experiments (see Table XIII) learned the slowly repeated series with almost as small a number of repetitions by

one method as by the other. Nevertheless, a uniform, though slight advantage lies with the standard method. In Subgroup 4 (see Table XIV), three of the four beginners learned the series, all slowly repeated, with a slightly smaller number of presentations by the prompting method; with Beginner No. 3 the case was reversed. This subject complained that the prompting 'worried' her. Some of the beginners also in Subgroup 2 complained of such disturbance. Distraction produced by the prompting may explain the advantage which in Subgroup 2 rests with the standard method. G. learned the slowly repeated series of length 18 (see Table XIV) and of 21 (see Table XIII) with a rather smaller number of repetitions by the prompting method, but learned the slowly repeated series of lengths 11, 41 and 81 (see Table XIII) with a smaller number of repetitions by the standard method. (By the expression 'slowly repeated series' is meant series which were both presented and recited at four or five-second intervals.) G. was not distracted by the prompting. The reason she learned the longer series, when slowly presented, with a smaller number of repetitions by the standard than by the prompting method is of a very different kind. This subject was apt to fail from 'nervous excitement' toward the beginning of long series even when, or perhaps specially when, she had mastered the latter part. She particularly disliked to hear the experimenter read, one by one, at long intervals an interminable array of syllables which she had already memorised. Therefore, in reciting the 'long and slow' series by the standard method, she exercised supernormal attention, an agonised self-command.

The advantage of the prompting method in the case of the rapidly repeated series seems to be in the attention and swift apprehension demanded under conditions in which it is difficult to apprehend at all and consequently difficult to attend. In the standard method when the subject has once failed, he may 'sit back' (often literally) and let the rest of a rapidly repeated series pass over his helpless head.

3. *The allowing of unlimited time for recitation and even the lengthening of the intervals at which the subject was required to recite the syllables greatly reduced the number of repetitions*

necessary for learning the series. This fact can, of course, be most clearly seen by comparing sets of series presented at a uniform rate. (See Table XV, the work of G., represented in the third and fourth lines and the work of Beginners 8 to 10.) The reduction in the number of repetitions required for memorising can scarcely be laid entirely to the fact that the subjects traversed the syllables more slowly when leisurely recall was allowed. For the average time spent upon each repetition shows that most of the syllables were actually passed over at the rate prescribed for presentation, either because the subject actually recited only a few of the syllables at each unsuccessful attempt to reproduce the series, or because she preferred to recite at the rate prescribed for presentation. When with G., for example, the recitation-intervals were of four seconds and the presentation-intervals were of one second then 31 seconds on an average were spent to a repetition. This number of seconds, spent upon a single repetition, would imply 3 four-second intervals, 1 six-second interval, and 13 one-second intervals; as an average, it implies that only the first four or five syllables were recited and that the rest were presented. As a matter of fact, the lengthening of the time spent in recitation actually shortened the total time consumed in learning the series. This saving of time could not have been secured unless the decrease in the number of repetitions had been great in proportion to the increase in the time occupied by each repetition. We may, therefore, conclude that *timing, and especially the requiring of rapid recitation, raise the number of repetitions necessary in the method of complete memorising. From this conclusion we may infer that the unlimited time allowed by our method in rearrangement is an important factor in enabling the subject perfectly to reconstruct series by our method after a very small number of trials.*

4. *Decreasing the rate (increasing the intervals) at which the syllables were presented greatly decreased the number of repetitions necessary for learning the series.* This fact is brought out unequivocally by the whole trend of Tables XIII to XV. We may, therefore, conclude without hesitation that *the slow rate of presentation is a potent factor in reducing the number of repetitions necessary for memorising by our reconstruction method.*

Nevertheless, the 'explanation by slow rate of presentation' is not altogether sufficient to interpret the difference between our results with the reconstruction method and the Ebbinghaus and Müller-Schumann results with the method of complete memorising. Two facts must be put clearly side by side—namely, (1) that it is considerably 'easier'—as measured by the number of necessary repetitions—to learn serial order by our reconstruction method than it is to learn 'slow' series by our method of complete memorising, and (2) that it is easier to learn 'fast' series by our method of complete memorising than it is by that of Ebbinghaus and Müller. These two statements must now be justified. In the first place, even with a somewhat slower rate of presentation, with about the same number of members in the series, and at approximately the same practice-level, the number of presentations necessary for learning series by our method of complete memorising somewhat exceeds the number necessary for learning series-order by the reconstruction method. The beginners whose work is represented in Table XIII learned 75 series of 11 nonsense-syllables repeated at the rate of one syllable per five seconds with an average of 4.1 repetitions and learned 75 series of 21 syllables repeated at the same rate in 6.9 repetitions. But the beginners whose work is represented in Table VV perfectly rearranged 52 series of 12 syllables (series of exactly the same type) presented at the rate of one syllable every three and a half seconds with an average of 2.4 repetitions and rearranged 58 series of 18 members with an average of 3.5 repetitions. The relatively small number of repetitions required by our reconstruction method as compared with our own standard method of complete memorising could readily be shown by still other figures. In the second place, even our unpracticed subjects required for the learning of series of 11 and even of 21 syllables, repeated by our standard method of complete memorising at the approximate rate of one syllable a second, a distinctly smaller number of repetitions than Ebbinghaus and the Müller and Schumann subjects required for learning series of 12 syllables repeated by their method at the rates respectively of one syllable to every two-fifths and to every four-fifths of a second. Ebbinghaus required from '14 to 16 repeti-

tions,'—including the last, successful recitation; the Müller and Schumann subjects required about the same number as Ebbinghaus or required a slightly higher number of repetitions for learning wholly new series. Our own beginners, who work appears in Table XIII, learned 73 'fast' series of 11 members with 7.8 repetitions and 73 'fast' series of 21 members with 13.5 repetitions, including the fully successful recitation. Our intervals in the 'fast series' were $5/2$ as long as the Ebbinghaus intervals and $5/4$ as long as the Müller-Schumann intervals were in general, yet at least two considerations prevent us from explaining the difference in the results by the difference in the rate. One consideration is that the Müller and Schumann subjects required about the same number of repetitions as did Ebbinghaus although the rate of repetition was only about half as fast. It may be added that Ebert and Meumann obtained closely similar results with the standard procedure when their subjects were still unpracticed ($W = 15.41$ for series of 12 members) although their rate of repetition was one syllable to every $5/6$ of a second—almost as slow as ours.¹ A second consideration is that meaningless syllables cannot be intelligibly presented *to the ear* at a rate much faster than one per second. The writer believes that the rate of one syllable per second taxes the subject's auditory apprehension as much as a rate $5/4$ as fast taxes his visual apprehension. This opinion needs an exact experimental verification for which we have never had the proper apparatus, but it is more than a conjecture. Further discussion of the reasons for the difference between our results and those of Ebbinghaus and Müller will be postponed to the last section of this chapter. On the basis of the facts now before us we may conclude, first, that slow rate of presentation is an important factor in explaining the results characteristic of our reconstruction method; second, that slow presentation is not, taken alone, the all-explaining and crucial peculiarity of this method, and third, that our experiments with the method of complete memorising, like our experiments by the reconstruction method, lack some circumstance which raises

¹ See Ebert and Meumann, *op. cit.*, p. 43 and p. 75.

the number of necessary repetitions in the Ebbinghaus-Müller method.

Before passing to two more conclusions which may be drawn from the tables, one may remark that the figures do not help one to decide whether the slow presentation of the series-members or the allowing of slow recitation is the more important factor in reducing the number of repetitions. From the work of G., represented in Table XIII, work done at the subject's highest practice-level, it would appear that this subject could learn the series which were presented rapidly but might be recited slowly with a rather smaller number of repetitions than the series which were presented slowly but had to be recited rapidly. In passing from the reconstruction method to the method of complete memorising, the subject seemed to have more difficulty in adjusting herself to the necessity of rapid recall than to the necessity of rapid apprehension and grouping. However, the number of series in each set is too few and the existing results too inconsistent to warrant any positive conclusion. The trend of Table XV is in the opposite direction to that of Table XIII. This table represents the work of beginners, and the work of G. after some lapse of practice. From this table, it would seem that the number of repetitions necessary for learning is rather more affected by difference in the rate of presentation than by difference in the rate of recitation. This showing is not surprising for most of the repetitions contained much more presentation than recitation. A characteristic of the method of complete memorising, at least in our own experiments, is that the subject, who has failed repeatedly upon some one of the earlier sequences of a series, should often rather suddenly recite the series from beginning to end. The degree to which the repetitions were made up of presentation of course appears clearly in the original records but it is also implied by the figures in the last column of the table. The mean time in every case in which a *fixed* rate of recitation differed from the rate of presentation is obviously made up mainly of presentation-intervals.¹ On the whole, as regards the relative value

¹ How the effect of one set of intervals may dominate over the other may be seen from the following illustration: Suppose that under the conditions of these

of slow presentation and leisurely recall in reducing the number of repetitions necessary for memorising, the data represented in Table XV are too complicated to warrant a very definite conclusion. One can see only that the advantage of allowing leisurely recall is not great enough to outweigh the advantage of allowing leisurely apprehension *plus* the advantage of traversing the major portion of the series at a slow rate. But as regards economy of time—a saving which is here only an incidental consideration—it is clear that the allowing of leisurely recall is a positive advantage whereas slow presentation is not. Table XV shows that increasing the intervals at which the syllables must be or might be recited reduced the total time required for learning the series, whereas increasing the intervals at which the syllables were presented increased this total time.¹²⁰

The fifth and sixth conclusions to be drawn from the results of the twelfth group of experiments are incidental—that is to say, they do not bear directly upon any of the questions formulated on pages 173-176.

5. *Reducing the rate at which the members of a series are repeated is no royal road to quick memorising in spite of the saving in the number of repetitions and presumably in energy.* For, as we have just seen, *reducing the rate at which the syllables of a series are for the most part repeated, does not reduce the number of repetitions necessary for learning in anything like so great a proportion as it increases the time spent upon each repetition.* If one examines in Tables XIII and XV, the cases in which the rates of presentation and recitation were the same, one sees that quintupling or quadrupling the repetition-intervals does not by any manner of means reduce the number of necessary repeti-

experiments a series of 18 syllables is presented at the rate of one syllable to a second and recited at the rate of one syllable to every four seconds. Then if the first four syllables were recited and the last 14 presented, the time spent in the repetition would be $(3 \times 4 + 6 + 13 \times 1) = 31$ seconds. But if the first 14 syllables were recited and the last four presented the time would be $(13 \times 4 + 6 + 3 \times 1) = 61$ seconds.

¹²⁰ Since the presentation-intervals greatly predominate over the recitation-intervals, the lengthening of the presentation-intervals must have reduced the number of necessary repetitions *very much* more than the lengthening of the recitation-intervals reduced it if the total learning-time had been shortened.

tions to a fifth or a fourth of the number necessary at the more rapid rate. Moreover, if one examines the sixth and the ninth columns of Table XV, one sees that, except in the cases where the recitation-intervals were greater than the presentation-intervals, the average number of repetitions required at the slower rate of repetition is always a larger fraction of the average number required at the faster rate than the mean time spent upon the more rapid repetition is of the mean time spent upon the slower repetition. The fact that rapid repetition saves time in learning is not a new discovery¹ but it is nicely illustrated by these experiments. These particular experiments do not touch the question of the relative tenacity of associations formed by slow and by rapid repetition.

6. In general *doubling the series-length* in the experiment of the first two subgroups (Table XIII) *did not double the number of repetitions necessary for memorising*. With G., however, in all the six procedures save one, the number of repetitions necessary for memorising series of 81 members is at least approximately double the number necessary for learning series of 41 members. In four out of the six cases it is more than double. In the one procedure in which it is less than double the syllables were presented slowly but had to be recited rapidly. With this procedure, the subject certainly paid more than normal attention to the learning of the series of 81 members. Other anomalies exist in G.'s work, but they are probably due to the small number of series in each set. In the case of the beginners the increase of difficulty with series-length is really rather less in learning nonsense-syllables by either our standard or our prompting method of complete memorising than it is in learning syllables or colors by the reconstruction method. In the relatively small increase of difficulty with series-length we have another marked difference between our results with the method of complete memorising and those of Ebbinghaus. (See page 88.)

¹ See Ebbinghaus, *Grundzüge*, pp. 671-673; and Ephrussi, *op. cit.*, p. 183, sq. Upon the relative advantage of slow and rapid repetition, cf. also Bergström, *op. cit.*

B. Combination and Interpretation of Results.

In his *Grundzüge* Professor Ebbinghaus gives a concise and masterly exposition of the different methods used in memory investigation. At the end of this exposition stands a passage which may be freely translated as follows: "One has no right at all to presuppose that the investigation of the same problem by means of the different methods will furnish exactly the same results. To be sure, the law which operates under any given set of conditions is always objectively the same, but the conditions which are involved, *ipso facto*, in the application of the different methods are never the same. Even if all the external circumstances are entirely parallel, yet the aim of the subjects and with it their whole mental attitude will be different. It will vary, for example, according to whether they may have so to impress something on themselves as merely to recognize it afterwards or as they may have so to impress it as to be able to reproduce it actively. It will vary according to whether they must memorise the given matter as a whole or only the members which pair off with certain others; and it will vary according to whether they must retain what they have learned for a considerable time or only for the moment."¹

As stated in the passage just quoted, different methods create different situations. The studies reported in this chapter are attempts to analyse the situation created by the reconstruction method, especially in comparison with the situation created by the method of complete memorising. The reconstruction method is left out of account by Ebbinghaus. Yet grossly rough as this method is, it deserves study because the memorising of series of concrete impressions as such cannot possibly be studied by any other.

The preceding pages have many times insisted on the peculiarity of the results obtained by the reconstruction method. Here the briefest summary will suffice. Unpractised subjects can memorise series of 12 and of 18 members with a number of repetitions which is surprisingly small in comparison with the number required by the method of complete memorising and

¹ *Grundzüge*, pp. 648-649.

especially in comparison with the number required by the special procedure of Ebbinghaus and of Müller. With our method, moreover, practiced subjects can learn series of 31 and 41 and even, when time permits, of 61 and of 81 members with a number of repetitions which approximates toward the number required for the smaller series. The number of sequences which can invariably be learned with a single presentation is not remarkable, but the number which a practiced subject can reproduce after a second presentation is astonishing. How, then, is the peculiar situation created by the reconstruction method related to these results? The problem is twofold: Why under the conditions of the reconstruction method is the number of repetitions required for memorising so small, and why with practice does series-length make so little difference? The first phase of the problem will be discussed first.

I. If the writer were required to sum up the peculiarities of the reconstruction method in one adjective the word chosen would be *easy-going*. *In the first place the required kind of recall seems easy*; therefore, the subject approaches his task in a relatively calm frame of mind,—an objective frame of mind in which he does not ask himself feverishly ‘How soon will I be able to do this?’ but asks himself coolly “What, then, is the exact nature of these impressions of which I am asked to learn the order?” The theorist who approaches them atter for the first time usually says, off-hand, that “the reconstruction method is easy because the subject does not have to learn the series-members but only their order.” This statement is literally true but not chiefly in the sense given to it by the tyro in memorising. The reconstruction method is easier because the subject does not *have to* learn the series-members—that is, because he is not *asked* to learn them—but not because he *does not* learn them. Suppose that one is asked for the first time to learn the order of the presidents of the United States. Will one proceed in a different way than if one were asked to learn the names of the presidents? Will not the natural procedure in both cases be to learn the names in order? The first requirement, however, seems easier than the second; one is much less

afraid, and with reason, of being panic-stricken—‘phased’—at the moment when recall is demanded. Proper names which have a certain arbitrariness and cannot be recalled by any process of logical memory, have sometimes a maddening fashion of slipping just beyond one’s reach when most wanted, behavior which is probably due to the reciprocal inhibition of associations which run out from the ideas which ought to bring them up. If one is asked, ‘What did David say in his haste?’ or ‘What did Portia say about mercy?’ one answers with mechanical glibness ‘All men are liars’ or “The quality of mercy is not strained; it falleth like the gentle dew from Heaven;” but if one is asked ‘*Who* said in his haste, All men are liars?’ or ‘*Who* said, The quality of mercy is not strained?’ though one may answer promptly, ‘David’ or Portia,’ yet the reproduction-reaction time will be a little longer, and one may be conscious of the swift mediation of other images—perhaps of an open Psalter or of a mediæval court-room. One may even detect and suppress a conflicting association with the mercy-quotation; a New Testament scene or one or more of the Beatitudes may flash through one’s mind, even when one seems to others scarcely to hesitate in giving the right answer. Now when proper names or syllables, which are still more senseless than proper names, are learned in order, no other association is likely to conflict with that which runs from the series-member to the idea of its number or place.¹ On the other hand, there are apt to be a number of conflicting associations with the association which runs from the number or place-on-the-list idea to the name or syllable, and even with the association with runs from one name or syllable to the next. For example, when one wishes to recall the name of the president who succeeded Andrew Jackson one’s ‘mind’ may run feverishly about among the details of that gentleman’s personal history or the actual consequences of his dictum ‘To the

¹ Of course, this will not be true if the names and syllables figure in different series which one has occasion to memorise. Witness the confusion of the average American visitor to Berlin in regard to the departed members of that royal line in which an elector of Brandenburg became a king of Prussia, and a king of Prussia an emperor of Germany, so that two new sets of numbers have been introduced.

victors belong the spoils' and may refuse—at least for the moment—to proceed along the straight line of the presidents.¹

In brief, the point which the writer wishes to make is this: One does one's work in about the same way whether one is asked to learn the order of series-members or to learn series-members in order. Even if one set out to learn simply the order, one could not altogether escape learning the content of the series-members, any more than in learning series-members, always presented in the same order, one could altogether escape learning that order. The experimental results reported on pages 118 and 155 and the corresponding introspective observations have made amply clear the fact that in learning by the reconstruction method the subjects learned the content of the series-members either in order to learn the order or in learning the order. Nevertheless, *accidents are much more likely to occur in the recall of meaningless series-members than in the recall of their order.* The subject has had disagreeable experiences in reciting names, paradigms, and other matter which must be memorised mechanically, and is thrown into a state of trepidation by the demand that he should 'learn nonsense-syllables,' whereas learning the 'mere order' of colors, smells, and syllables seems to him relatively easy. Now when one is frightened in learning or in reciting, one learns or recites poorly because one's attention keeps jerking back from the material to be learned or reproduced to the idea of one self as succeeding or failing and to one's own organic sensations. The emotional disturbance, taken together with some genuine danger of a 'block' in the recall of certain syllables, explains the fact that it is really, though slightly, easier to reconstruct a series of nonsense-syllables than to reproduce it, a fact which has been

¹ The writer believes that divergence and conflict of associations is the chief explanation of the difficulty of remembering a foreign equivalent for an English word as compared with the difficulty of remembering the English equivalent for a foreign word. If one is asked 'What does *Schnur* mean' one answers promptly 'string!' but if one is asked 'What is the German word for *string*' one is likely to think the English words *cord*, *thread*, and so on, or to see parti-colored visions of strings, before one gets to *Schnur*. Some uncalled-for verbal or concrete image associate may even lead one off on a wrong track. Thus one may find oneself thinking *string—draw-string*, and replying 'Draht!'

pretty satisfactorily demonstrated by the experiments reported on page 155. Nevertheless, the fact that the series-members are given again and do not have to be recalled bodily in the reconstruction method is not sufficient to explain the difference between this method and the method of complete memorising. In the very group of experiments last mentioned, which show that reconstruction is a trifle easier than reproduction, the subjects learned to reproduce the series (which were presented in exactly the same way as in the reconstruction method) with a far smaller number of repetitions than are required in the method of complete memorising.

In this connection should be noted a point which is important though rather difficult to evaluate: The criss-cross associations between members of the series which do not stand next to each other are fostered much more by attention to the order of series-members than by attention to the members themselves.

The second respect in which the reconstruction method is easy-going lies in the fact that complete presentations of the series alternate with opportunities to test one's knowledge of the series-order as a whole—the opportunities which are called, for brevity, tests. The interpolation of tests has four consequences: (1) The subject impresses upon himself both the sequences and positions which he has learned and also certain mistaken notions of order. (2) The strain upon attention involved in the presentations is relieved; periods of relatively slack attention can occur in the process of learning without doing any harm. (3) The subject need not seek to test himself and to impress the series-sequences on himself at the same time; he can give his whole attention alternately to one process and to the other. (4) The presentations are distributed over a wider time interval. Thus, series learned by the reconstruction method have to a certain extent the advantage of 'distributed repetitions.' The learning process comes under Jost's law that a new repetition of a series has more effect on a long standing association than on a relatively fresh association. In the belief of the writer this law has a physiological basis. This matter will be taken up again. Here it should be noted that the experiments reported on pages 162, 165 and 170 have shown that the advantages accruing

from the mere multiplication of tests,—either through the testing-process or through the interpolation of intervals between presentations,—is relatively of minor importance.

The third respect in which the reconstruction method is easy-going lies in the fact that not merely long time but unlimited time is allowed for recall. The last group of experiments reported (see pages 175 and 188) has shown that this circumstance is of very great importance indeed. 'Leisurely recall' contributes greatly to the emotional equilibrium of the subject. Whether or not it contributes more than 'leisurely apprehension,' this study fails to tell us.

The fourth respect in which the reconstruction method is easy-going lies in the fact that the members are slowly presented. The slow rate of presentation has the following consequences: (1) The series-members are satisfactorily apprehended so that the subject is not confused as to their exact nature, except where real difficulties in sensible discrimination come into play. (2) The subject is not agitated by the effort to apprehend quickly. (3) The attention of the subject is not so taken up in the effort to apprehend the separate members rapidly that no attention is left over for noting order. (4) During the exposure of a series-member and the interval following it, the subject has time to lay stress upon the sequence which this member completes. He thus (3 and 4) may grasp the series-members in pairs instead of in isolation. A series becomes to him $a-b$, $b-c$, $c-d$, $d-e$, and so on instead of remaining a , b , c , d , e . (5) If the subject can shift his attention rapidly he is able to clinch more sequences than the last during the interval between exposures. (6) He has time to couple each series-member with the notion of its number in the series. He may not and usually does not consciously clinch each series-member both with the preceding member and with the absolute number in the series. Nevertheless, he has full time for the kind of counting, often non-verbal, which, with or without the formation of firm criss-cross associations, is involved in grouping. (7) The subject has time to couple the series-members with images of places on the table before him so that the place ideas become distinctly spatial. (8) He has time deliberately to make auxiliary associa-

tions. (9) Auxiliary associations not deliberately made have time to come into play. (10) A relatively great length of time supervenes between the earlier and later sequences of the series and this lessens the inhibitive action of one part upon the other. This very important point will be taken up again in a moment. (11) The time consumed in each presentation of the whole series is lengthened so that the interval between the moment when any one sequence—say, *c—d*—is presented in any repetition, and the moment when it comes around in the next repetition is also lengthened. Thus, slowly presented series have, after a fashion, the advantage of distributed repetitions, whatever this advantage may be.

Against these consequences of slow presentation, which are all advantageous in the sense that they tend to decrease the number of repetitions necessary, must be added two disadvantageous consequences: (12) In the case of the smells and colors, the long intervals which elapse between the series-members increase the difficulty of sensible discrimination. (13) The demands upon the subject are not strenuous enough to keep attention steady. This fact was shown by experiments described on pages 150 and 153.

All the consequences which arise from presenting nonsense-syllables slowly by the reconstruction method arise from presenting them slowly in the method of complete memorising excepting only the seventh of those named above. Slow presentation greatly reduces the number of repetitions necessary in the method of complete memorising (see pages 188-191). We may, therefore, infer that slow presentation is an important factor in making the number required by the reconstruction method absolutely small. Nevertheless, slow presentation in the case of the reconstruction method does not in itself suffice to explain the difference between the results of the two methods. For when syllables are repeated in the method of complete memorising at exactly the same rate as they are presented in the reconstruction method, yet still the number of repetitions required for learning remains rather greater than the number required for learning syllable-series by the reconstruction method proper. Moreover, it remains rather greater than the number required

for learning series which are presented in the same way and at the same rate as in the reconstruction method, but which are reproduced in writing after each presentation.

In brief, the first of the two problems at issue in this chapter—namely, the reason for the fewness of the repetitions required by the reconstruction method—may, in the opinion of the writer, be solved somewhat as follows: *The leasurly recall and the slow rate of presentation in the reconstruction method largely explains the difference between the number of repetitions required with this method and the number required with the procedure of Ebbinghaus and of Muller and Schumann. Leasurly recall involves a certain amount of composure on the part of the subject. Slow presentation involves complete apprehension of the series-members, a certain amount of emotional composure, and the slow traversing of the sequences. This slow traversing of the sequences probably involves an advantage of purely physiological origin and certainly fosters the development of a certain learning-technique on the part of the subjects.* (These last two points will be discussed in a moment.) Either the explanation by leasurly recall or the explanation by slow presentation leaves unexplained a residuum of difference between our reconstruction method results and the results of the Ebbinghaus and Muller procedures. For in our own procedure with the method of complete memorising, neither when the series were presented as slowly as in the reconstruction-method nor when unlimited time was allowed for recall, was the number of repetitions required as small as in the reconstruction method. It may be that the two explanations taken together very nearly account for the difference under discussion.¹ It seems probable, however, that the difference is explained in some slight degree by the multiplication of tests in the case of the reconstruction method and by the fact that the subject is required to reproduce only the order of the series-members. *The chief, not the exclusive, advantage in interpolating a test after each presentation of the series lies in the fact that high-level and undivided attention*

¹ In none of our experiments with the method of complete memorising was it the case both that series were presented quite as slowly as in the reconstruction method and that unlimited time was allowed for reproduction.

is secured during the tests themselves. The chief (not the only) advantage in 'the presence of the links' lies in the fact that the subject is not perturbed by the demands upon him.

Closely connected with the first problem of the chapter, though not a part of it, is the question why a smaller number of repetitions is required in our complete-memorising procedure, even when series are presented and recited rapidly, than the number which is required in the Ebbinghaus and the Müller-Schumann procedures. The difference cannot be due to difference in rate. (See p. 190.) Neither is it due to the fact that English nonsense-syllables are easier to memorise than German. It is true that among our own English normal series more words occur than are found among the German series of Müller and Schumann, but this aid to the learner is fully offset by the fact that English syllables which are spelled alike are not always pronounced alike and *vice versa*. It cannot be that the number of repetitions is smaller in our procedure because auditory, *viva voce*, presentation is more impressive than visual.¹ To the writer, it seems probable that the number is smaller on account of the technique acquired by the subjects in learning those series which were slowly presented *pari passu* with the series which were rapidly presented.

II. Slow presentation is, in the belief of the writer, the key to the second problem at issue in this chapter: the question why series-length makes relatively little difference in the number of repetitions required for memorising by the reconstruction method. *Slow presentation has two great advantages. The first lies in the fact that when a series is slowly presented the mutual interference of the different association is lessened.* This advantage is a plain fact of introspection. To explain it the writer has worked out a theory made up, crazy-quilt fashion, out of shreds and patches of other people's theories and observations.² This speculation involves the following suppositions:

1. In order that an association may be even relatively permanent, some process of consolidation or 'setting' must take

¹ Cf. Pohlmann, *op. cit.*, p. 168.

² The theory is so eclectic that references cannot be given without space-consuming discussion.

place.¹ This process consists in some modification of the nerve-fibers connecting two cortical areas.

2. The consolidation process requires time and cortical energy and will not take place if the persistence of the processes which directly conditioned the occurrence of the associated ideas in consciousness is too quickly interrupted. This statement needs to be amplified by a discussion of three points, namely, time as a factor in the consolidation-process, cortical energy, and persistence and its interruption.

First, the consolidation-process, as a gradual one, differs from the nerve-path process which forms the association when two cortical areas function together or in close succession; one might say, metaphorically, that the former differs from the latter as the 'setting' of a jelly differs from its moulding. The more consolidation has taken place, the more an association

¹ The writer has meant (and means with unblushing determination) to use the word *association* to stand interchangeably for the 'connection' of ideas in virtue of which one 'tends to suggest' another and for the cortical modification in virtue of which an excitation tends to run over from one cortical area to another. The writer eschews the use of the term *association* to stand for the process of reproduction or for the actual arousing or 'touching off' of excitation in one cortical area by excitation in another. It seems, however, mere word-wasting pedantry to insist on distinguishing at every turn by separate expressions the so-called association of ideas and the cortical modification which conditions it. We know of the cortical modification only through the touching-off process which it conditions and this we know only through its psychical index, the 'suggestion' of one idea 'by' another. Nevertheless, the cortical modification or 'trace' is the only tie which persists. Ideas in consciousness are not connected with ideas out of consciousness. Ideas cannot exist out of consciousness. The more or less orderly sequence of ideas is experienced and introspectively observed; their connection—unless connection means merely the frequent sequence of particular ideas—is not experienced. Association 'between' ideas cannot inhibit one another; *between* is meaningless with reference to something and nothing, that is, to the idea in consciousness and the idea not recalled and perhaps never to be recalled. *The association of ideas* is a mere short-hand expression for a neural tendency based on a cortical modification. Therefore, it seems absurd, instead of saying 'The association *knife* Messer inhibits the association *knife* couteau' to say 'The tendency of the cortical excitation which conditions the verbal image *knife* to arouse the cortical excitation which conditions the verbal image *Messer* inhibits the tendency of the former to arouse the cortical excitation which conditions the verbal image *couteau*.' (Cf. Binet, *L'Associations des Idées*, pp. 6-7.) In this matter, there is no danger of confusing one's physiology and psychology unduly because there is nothing on the psychical side to answer to association in the sense of actual connection.

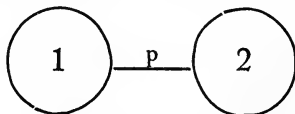
is strengthened by the repetition of the process which forms it. Hence, Jost's first law, and the advantage of distributed repetitions.¹ By the strengthening of an association is meant the insuring of its functioning, and, other things being equal, the shortening of its functioning time. An association is strengthened first by consolidation; second, by every repetition of the process which forms it, e. g., by every *presentation* of a syllable-sequence; and third, by every actual functioning,—e. g., by every *recitation* of this syllable-sequence.

Second, the notion of cerebral energy here employed is almost as vague as the notion of modification in the association-fibers. It may mean reinforcement by the 'association centers' or ample blood supply or both. Whatever energy means, it is here assumed that the draughting of it to one piece of work tends to take it away from another.

Third, by persistence the writer means the continuance, after an idea has vanished from consciousness, of the cortical excitation which at sufficient intensity brings about the occurrence of this idea. During the period of continuance, the excitation, if not set up afresh by transmission from some other nerve-center, is rapidly subsiding, but this subsidence or ringing off (*Abklingen*) is characterised by little rises or swells which may bring about the recurrence of the idea. Persistence and consolidation are two different processes. The former may be thought of as a cortical-center process; the latter as a nerve-path process. The former may directly bring about the recurrence of a recent idea; the latter paves the way for the recurrence of an idea perhaps no longer recent. Nevertheless, although the two processes are not the same, a certain amount of persistence of excitation in the cortical areas concerned, is a prerequisite to consolidation.² Excitations in only a limited

¹ See Jost, *op. cit.*, p. 472. See also Ebbinghaus, *Grundzüge*, pp. 657-660.

²



This figure may serve to represent two cortical sensory areas with an inter-connecting path. Excitations in 1 and 2 condition percepts or images. (The writer assumes that the excitation which conditions a percept and that which

number of areas can persist at any one time for persistence requires cortical energy, and the amount of energy available at any one time is limited, although it seems to vary in different individuals. The formation of fresh associations also requires cortical energy.

3. There is not in any short space of time—say, in the time consumed in rapidly repeating a series of forty syllables—enough cerebral energy both to allow a large number of excitations to persist and thus to furnish the initial prerequisite for

conditions the corresponding image take place in the same cortical area.) The continuance of the excitations is *persistence*. The modification somehow produced in the nerve-path p when the areas 1 and 2 function at about the same time is an *association*. The modifying process is the *forming* of the association; the 'setting' of the modification is the *consolidation* of the association; the transmission of excitation from 1 to 2 along the path p is the *functioning* of the association. Every repetition (within certain limits) of the condition which led to the first forming of the association and every repetition of its actual functioning *strengthen* the association—that is to say, they increase its readiness to function.

The writer does not wish to postulate in this speculation any particular theory of the interconnection between the 'knots of neurones' in the cortex or of the functional relation between the nerve-cell-body and its processes. That there are areas in the cortex which represent, very roughly indeed, particular contents of consciousness and that excitation is transmitted from one area to another over some material path or paths is scarcely to be called a mere theory.

The writer has attempted to divest the *Perserserationstendenz* of Müller and Pilzecker of a Herbartian guise which it does not seem necessary for it to wear. These writers ascribe to the idea (*Vorstellung*) a quickly diminishing tendency to come back into consciousness of its own accord and say that an idea attentively, repeatedly, and recently experienced will often possess such a strong persistence-tendency that it will push its way into consciousness at any moment when consciousness is not besieged by other 'factors.' Is not this merely the figurative language of brevity? If one speaks, for the sake of brevity, of ideas as persisting or as inhibiting one another, does it necessarily mean that one soberly hypostatizes the idea? Ebbinghaus's two objections to the persistence-doctrine are first, that the physiological conditions assumed have no parallel, and second, that all ideas do not persist. What then of the conditions of retinal after-images? The retina and the cerebral cortex are surely near akin in origin and in structure. And what of retinal rivalry? What also of the differences in individual liability to after-images? There is, surely some introspective and experimental evidence for persistence. It may be possible to interpret all such data by chains of subliminal associations, but this explanation often seems to go further afield than the supposition of persistence. Cf. Müller and Pilzecker, *op. cit.*, pp. 58-78; Aschaffenburg, *Experimentelle Studien über Assoziationen*; Kräplin's *Psychologische Arbeiten*, i, pp. 55, 61 and 66; Wreschner, *Die Reproduction und Assoziation von Vorstellungen*, *Zeitschrift f. Psych. und Phys. der Sinnesorgane*, *Ergänzungsband* 3, pp. 237-258; and Ebbinghaus *Grundzüge*, pp. 691-692.

the consolidation of a large number of associations, and over and above this to form a large number of new associations. Therefore, when a long series is rapidly presented for the first time, the persistence of the earlier members blocks the formation of associations between the later members. Furthermore, the actual appearance and the persistence of the later members tend to prevent the consolidation of the associations already formed. The association $\widehat{p\ q}$ when formed and 'setting' dissolves $\widehat{c\ d}$, and $\widehat{c\ d}$, when reformed and setting, dissolves $\widehat{p\ q}$. As Ebbinghaus says: "Many persons can never in any reasonable length of time achieve a perfectly correct recitation of the longer series of nonsense-syllables. Some parts are continually thrown into confusion by others, The readings which follow the first readings of a series often bring about only a slight increase in the subject's power to reproduce it. The mutual disturbance of the series-members by one another, which is distinctly manifest to immediate consciousness, brings the process of learning to a standstill, and only when the whole series has become somewhat more familiar through several readings does a further and marked increase in the number of retained elements take place."

4. When on the other hand a series is slowly presented—even when the subject is scarcely conscious of having free time but seems to busy himself with each sequence as it comes—the mutual interference of the incipient associations is much diminished because energy is not draughted so rapidly in different directions. Hence when series are slowly enough presented their length makes relatively little difference. This is the point which of all others the writer desires to make. However vague and rash the physiological speculation of the last few paragraphs may be, yet no very skilled introspection is required to discover that associations dislodge one another when a long series is rapidly presented, and may, as it were, be held peaceably side by side when a like series is slowly presented.

5. Series, if short, are learned in a shorter time when rapidly presented than when slowly presented, but this fact does not disprove the theory just advanced for, in this case, the great *persistence* of all the series-members may compensate for the

defective consolidation of the associations. This supposition requires amplification. Every attentive repetition of a series, of course, strengthens the associations involved. But all repetitions of a series do not strengthen the associations to the same extent. It is obvious that a slow repetition accomplishes more *absolutely* than a rapid repetition, since fewer slow repetitions are required to bring all the associations to that particular readiness to function which we call the mastery of the series. This fact has just been explained by the supposition of a consolidation-process which requires time. Nevertheless, a rapid repetition accomplishes more in proportion to the time expended on it than a slow repetition, for halving the time allowed for each repetition of the series does not double the number of repetitions necessary. Rapid repetition must, therefore, involve some advantage which offsets the interruption of the consolidation-process. If it did not, halving the time would *at least* double the number of necessary repetitions; and indeed if the consolidation-processes are cumulative, halving the time might more than double the number of repetitions. Now rapid repetition may be supposed greatly to increase the persistence of the series-members because the excitations which condition their appearance in consciousness are renewed before they have had time to subside so that the prolonging effect is cumulative. Furthermore, a weak association may be supposed to be sufficient to raise a process which is still subsiding to such a level (*Niveau*) that the corresponding idea emerges in consciousness. Therefore, after a number of rapid repetitions of a rather short series, one finds the series suddenly resounding in one's ears or rolling off before one's eyes from start to finish.

6. The length of the series which can be learned by rapid repetition is sharply limited except in certain cases in which a subject—such as G.—has had long practice in memorising and has acquired a deft technique. In ordinary cases, the subject cannot learn a long series if rapidly repeated, with any reasonable number of repetitions, because its members cannot all persist at once, and the incipient associations between them dissolve one another. On the other hand, an association once

fairly consolidated draughts off no energy until it functions. Hence, if there were no limits to the amount of time which can be spent in learning series there would be no limit to the length of the series which the subject could learn with slow repetition. It should not be forgotten that the lengthening of the series in itself gives to the individual sequences the advantage of distributed repetitions. Of course, the rate of repetition need not be made slower and slower as the length of the series is increased. Each interval (for example, that between c and d) need be long enough only to allow the taking place of a certain degree of consolidation in the association just formed (\widehat{bc}); if the interval is made too long, no association will be formed between the series-members which bound it (c and d).

Before this discussion of consolidation *versus* persistence is closed, two more points should be expressly noted. In the first place, the phenomena of memorising with quick repetition seem intermediate between the phenomena of memorising with slow repetition and the phenomena of the memory-span. The width of the memory-span depends on attention-range and persistence; facility in memorising with rapid repetition probably depends largely on persistence; facility in memorising by slow repetition cannot depend on persistence at all, but must depend on the readiness of the cortex (association-fibers) to receive and to consolidate the modifications which constitute associations.

In the second place it should be noted that further experiment may prove that series learned with rapid repetition are as well remembered for hours as series learned by repetition as slow as the presentation of our reconstruction method. If this should prove to be the case, then the foregoing speculation falls wholly to the ground.

It will be remembered that *two important advantages were attributed* (on page 202) *to slow repetition*. The first—namely, the lessening of the conflict between incipient associations—has just been discussed. *The second is the development of a technique which can scarcely be gained under either the sensory or the emotional conditions of rapid repetition*, although when developed it may be used under those conditions. Slow presentation is

the secret of the astounding results of practice in the reconstruction method. The nature of the technique acquired has already been so fully described that it need scarcely be sketched here. It does *not* consist of mnemonic devices. In the case of all sorts of material, it consists in part of keenness of apprehension, that is, of precision both in apprehending the individual series-members and in apprehending sequences. In the case of meaningless material it consists largely in the practice of grouping. In the case of non-verbal material it consists in the differentiation and stereotyping of the imagery, so that each unit in the material has some one mental counter all to itself. In G.'s technique a highly important factor was the system of spatial projection fostered by the peculiarities of the reconstruction method. Our experiments to test with other subjects the value of spatial associations have had a negative outcome, but these experiments were not extensive nor exact.

It may be urged that the same technique could in time be acquired with rapid repetition. Perhaps, if the time were long enough, but it must indeed be long. Many of us have had an experience which brings home the fact that exercises in apprehension are ineffective if too difficult. In our first few weeks in France or in Germany, we may have learned absolutely nothing from the rapid table-talk of the natives. We might as well not have sat at the table; as far as language-learning went, a solitary half-hour with our grammar would have been more profitable. Yet we learned much from the slow speech of benevolent foreigners, and having learned through it to recognize the foreign words, we finally learned much from the table-talk which at first passed utterly over our heads. In any case—whatever may be acquired through rapid repetition—the writer is convinced of two things, first that it was the technique acquired in learning with slower repetition which enabled her to learn series of 81 nonsense-syllables when presented as rapidly as one per second, and second, that it was mainly the technique acquired with the series which were slowly presented which enabled our unpracticed subjects to learn the rapidly presented series with a number of repetitions which was small as compared with the number required by Ebbinghaus and the Müller-Schumann subjects.

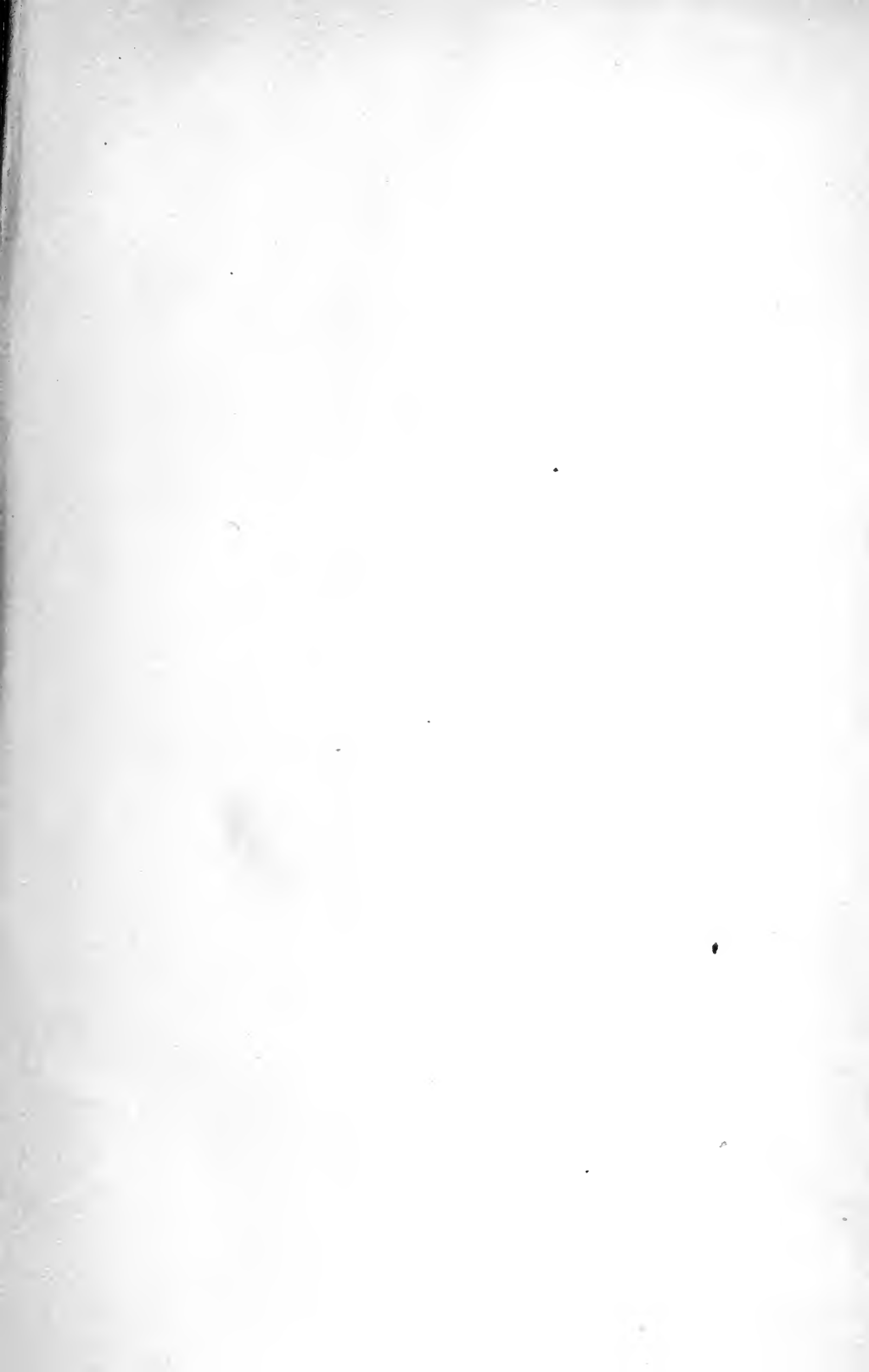
From a practical point of view, the conclusion of the whole matter is this: To learn a single short series of any kind in a short time, one should repeat it as rapidly as one can with complete apprehension, but to learn long series and *to learn to learn anything* one should repeat at a rate much slower than that which just allows the perfect apprehension of the material to be learned—perhaps, at a rate four or five times as slow as this maximum. If those who essay memory-training would enter the strait and thorny path by the proper wicket-gate, and would start with less extravagant expectations, they might not land in such numbers in the Slough of Despond. It is probable that practice is transferable only within very narrow limits. It is probable also that one's 'brute retentiveness' cannot be improved by training. Nevertheless, *it is certain that a very great difference can be made by training in what one can do with one's brute retentiveness along specific lines.*

APPENDIX.

PUBLISHED REEPORTS OF THE WORK OF THE WELLESLEY COLLEGE PSYCHOLOGY LABORATORY.

- 1893 *Statistical Study of Pseudochromesthesia and of Mental Forms.* By M. W. Calkins. American Journal of Psychology.
- 1895 *Minor Studies from the Psychological Laboratory of Wellesley College.* American Journal of Psychology.
The Continued Story. By M. W. Learoyd and M. L. Taylor.
Synesthesia. By M. W. Calkins.
- 1896 *Minor Studies from the Psychological Laboratory of Wellesley College.* American Journal of Psychology
The Dream Consciousness. By S. C. Weed, F. H. Hallam and others.
- 1898 *Short Studies in Memory and in Association from the Wellesley College Laboratory.* By M. W. Calkins. Psychological Review.
- 1900 *Wellesley College Psychological Studies: An Attempted Experiment in Psychological Aesthetics.* By M. W. Calkins with the assistance of H. Buttrick and M. W. Young. Psychological Review.
Minor Studies from the Psychological Laboratory of Wellesley College. American Journal of Psychology.
Studies of the Dream Consciousness. By G. A. Andrews.
- 1902 *The Perception of Sound Direction as a Conscious Process.* By E. A. McC. Gamble. Psychological Review.
- 1903 *Die reproducierte Vorstellung beim Wiedererkennen und Vergleichen.* By E. A. McGamble and M. W. Calkins. Zeitschrift für Psychologie und Physiologie der Sinnesorgane.
- 1905 *Attention and Thoracic Breathing.* By E. A. McG. Gamble. American Journal of Psychology.
- 1906 *The Effect of Music upon Thoracic Breathing.* By Eugenia Foster and E. A. McC. Gamble. American Journal of Psychology.
- 1908 *The Effect of Suggestion upon the Reproduction of Triangles and of Point Distances.* By J. C. Bell with the assistance of G. E. Hatch and L. T. Orr. American Journal of Psychology.







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