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THE
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No. 1

PRACTICE EFFECTS IN FREE ASSOCIATION

By FREDERIC LYMAN WELLS, PH. D., Psychiatric Institute, Ward's Island, N. Y., formerly Assistant in Pathological Psychology in the McLean Hospital, Waverley, Mass.

From the Psychological Laboratory of the McLean Hospital.

While it is difficult to believe that any extended series of association experiments could be made without the appearance of some trace of practice effect, there has yet been no occasion, within the writer's knowledge, for the presentation of any systematized study of these effects. To make such a study with any degree of satisfaction requires a much larger series of stimulus words than is usually requisite for the immediate purpose of association experiments. For the present study, a list was constructed to consist of one thousand different stimulus words, which should be so far as possible unambiguous, and familiar to the class of subjects dealt with. These words were written on as many separate slips of paper, which were then placed in a large box and thoroughly shaken together for 15 minutes. The slips were then drawn from the box at random, one at a time, and made up into twenty series of 50 words each. A revised list will be an improvement over this one, which has, however, shown entirely sufficient adaptability to the present experiments.

One series of 50 words was given to each of six subjects each day, six days in the week, until the entire twenty series had been given. On the next two days the first two series of 50 words were repeated. The present results are based essentially upon these experiments, totalling 6,600 observations; especially on the two series that are repeated. Two other subjects reacted to 500 words each. About a third as many

more observations were made with each of the first six subjects, with reference to special points in the experiment, but these results have only a limited application for the present purpose since the experimental material was here varied somewhat for the different subjects.

While regretting that there is no more accurate method of timing than the stopwatch, which permits other essential conditions of the experiment to be satisfactorily preserved, it must be acknowledged the most useful method available for timing the individual responses of the experiment. It was employed in all the observations here recorded. I have elsewhere spoken very distrustfully of this method, and it cannot be used for the interpretation of single measures on a minute scale. This is owing partly to the inherent coarseness of the measure, partly also to the inaccuracies of operation. In the present discussion the changes are sufficiently great as to be reliably reflected in this method of timing, which modifies the external conditions for the subject less than any other.

Of the six subjects with whom we are principally concerned, one is a highly educated physician in middle life, the remainder are women nurses, with one exception under 30 years of age. In Jung's classification of association types, four of the subjects belong to the *Sachlicher Typus* tending in different degrees towards the more subjective types; one is a fairly distinct *Prädikattypus*, and one rather a *Konstellationstypus*. The two other subjects, VII and VIII, are also women nurses under thirty, one being a *Sachlicher Typus*, the other a less marked *Konstellationstypus*.

The progressive changes in the time of the response, and the qualitative changes shown by the responses in the repeated series, form the basis of the present discussion. The most noticeable practice effect is that in reaction-time; the changes in the content of the responses are perhaps of an equally interesting nature, but hardly so well defined.

The accompanying cut illustrates the practice curve of the association time for each subject during the twenty consecutive daily series. The time unit, here as elsewhere, is $\frac{1}{5}$ of one second.

The range of individual differences at the beginning of practice is here about 2:1, a range that is seen in many mental measurements, but in other observations with a larger number of subjects, this range is seen to be much nearer 3:1. The fastest subject here is about as fast as the writer has ever observed, but in other subjects from this group the median of 100 association times may run as high as 20 fifths, as in several of the series with subject VIII. The reaction times in the present experiments also run somewhat longer than those

2a

Fishes of
a second

Day 0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20

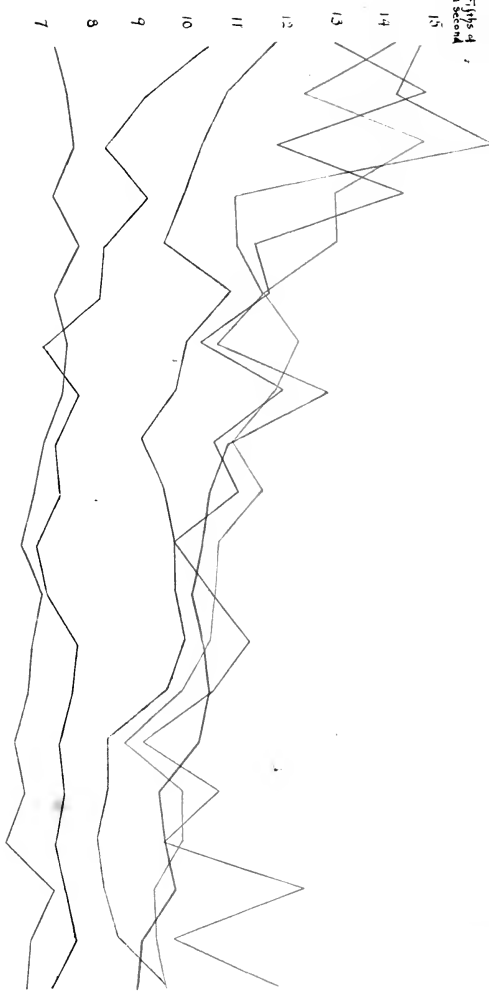


PLATE I.



reported by Jung, which, however, is amply accounted for by the greater difficulty of the individual series of words. A random selection of fifty from a thousand available words is naturally more difficult than a list of one hundred or two hundred words selected immediately.

The effect of practice seems to be towards a diminution of the individual differences in association time, although such a diminution is far from being what is found in all mental functions. Between three of the subjects (RED, ORANGE and GREEN) there is practically no such individual difference at any stage of practice, but in general the effect of practice seems to be to bring all the subjects near together at a certain psychological limit of quickness, which limit shows comparatively little individual variation. There are, therefore, very great individual differences in the closeness with which the different subjects approximate to this limit at the beginning of their practice. Thus subject BLUE starts well below any level which most of the subjects ever attain, and remains at this level throughout, the practice effect being almost absent so far as the association time is concerned. Other individuals have been observed, who, during the first observations with this test approximated to the short times here given by BLUE, and might be expected in further experiments to show as little practice effect. It seems fair to infer that the limit of quickness which any subject can attain by practice in this experiment, is equalled, if not surpassed, by other subjects at the beginning of practice, and these subjects then show little change during special practice. Some are born with the capacity for the promptest reaction, others achieve it only with special practice, and in very different degrees; still others perhaps not at all. And the general mental characters which give to one individual an inherently short association time are probably of a much more fundamental nature, and more important factors in the individual's make-up than those special characters which, developing during special practice, may give to another subject a somewhat greater facility in reacting.

Of the two subjects reacting to 500 words only, Subject VII shows nothing out of the ordinary, but the case of Subject VIII who shows an actual reverse of practice effect, is naturally of special interest. She seems to have a fairly good idea of the cause of the difficulty, which she describes as essentially a difficulty of choice between the large numbers of responses that would present themselves. Of this she is able to give quite detailed introspection in individual cases. She spoke of a difficulty in sufficiently focussing the "attention" *verbo ipso*. She said that the novel experimental conditions had



PLATE II.

“rattled” her a little only on the first two days, when as a matter of fact she was quickest. She denied absolutely that any conscious suppression of disagreeable associations had influenced the results and was able to definitely assign such a process in but one specific instance. It may be added that the subject is of an entirely normal make-up, and in education, mental balance, and professional efficiency, will bear comparison with any other member of the group.

One of the most unfortunate limitations of the median as a measure of central tendency is the fact that, unlike the average, it has no convenient index of the variability of the distribution about it. As a rough and ready index of the variability of the association times in each of the different series of fifty words, has been taken the smallest number of steps required to include 50% of the cases. Like the median itself, however, this measure takes no account for the extra long times but gives only an indication of how closely the central reaction-times are crowded together. In magnitude, this figure varies between 1.5 and 9 fifths of a second. As the time decreases with practice, the variability naturally drops also, although the record of Subject BLACK, in whom the

variability remains practically constant throughout, shows that it does not necessarily do so. The fluctuations in variability are more marked, and the individual differences less distinct, than in the curves of the reaction-times themselves. The variability of ORANGE is somewhat, that of BLACK, very disproportionately low. These are also the two most intellectual subjects in the group, although widely differing in their association types.

The distribution of the individual reaction-times in the single series of fifty words show a rather interesting fact that is contrary to what is ordinarily expected in such distributions. At no time during the practice is a distribution of marked skewness the rule. The mode is frequently as many as five steps away from the shortest reactions, and even during the final stages of practice there occur distributions exhibiting considerable skew towards the *long* end. Beyond the above decrease in variability, it is difficult to state any specific effect of practice on the form of the distribution. The natural interpretation of this is that an indefinite amount of practice would still leave us a considerable distance from the physiological limit of association time (which is more nearly approached in the controlled associations), and the limit of free association time, dependent as it is upon the most delicate interplay of the higher mental processes, is of too fluctuating a nature to leave any characteristic impress on the form of the distributions.

Contemporaneous with the free association experiment, the first five women subjects underwent practice in two other psychological tests. These were the Kræpelin addition test and a special form of the *A*-test, both of which belong to the general group of controlled association experiments. It is interesting to compare susceptibility to practice in these tests, which involve the continual repetition of the same or a few different associations, with the susceptibility to practice in the free association test, where the experimental task is a novel one in each individual observation. For present purposes it will suffice to compare the mean performance of the first two records with that of the last two for the period of twenty days. In the number-checking test it is more desirable to take the last two of the entire thirty days for which the function was practiced. This comparison gives us the following table:

FIGURES EXPRESSING THE PER CENT. OF PRACTICE IMPROVEMENT IN THE
FREE ASSOCIATION, THE ADDITION, AND THE NUMBER CHECKING
TESTS, INDICATING THEIR COMPARATIVE SUSCEPTIBILITY
TO PRACTICE

(The Lower the Figure, the greater the Practice)

Subject	Free Association	Addition	Number checking
BROWN	80	61	52
RED	74	52	81
ORANGE	70	55	52
GREEN	61	61	55
BLUE	89	49	41

In the case of the physician (BLACK) the practice in free association was 75%. In these figures it appears that while the practice improvement is practically always least in the free association test, it is nevertheless of the same order of magnitude save only, perhaps, in Subject BLUE. This is a striking result in view of the essential differences in the experimental tasks. In the individuality of the successive situations it presents, the free association test is unique among psychological experiments. It affords small opportunity for making any given association path more open through frequent use. Such a conception of practice fails when applied to the results indicated in the present experiments. It is therefore a question how far the practice in the number-checking test and in the addition test is of the same type, and the product of the same causes, as that in the free association test. The essential features in the free association practice can probably be cleared up only through the most accurate introspection, although it is a very natural interpretation to conceive of it almost wholly in terms of a removal of inhibitions. These decreasing inhibitions can for the present be only loosely figured as a greater accustomedness to the experimental conditions, a lessened emotional reaction to them, feeling freer and more at ease, less liability to distraction, and the like. They are essentially conscious inhibitions, although it is not easy to describe them accurately unless one has had some practice in introspection. We can probably reduce all these factors to the general term of the *elimination of the inessential*. The part which is necessarily played by this *elimination of the inessential* in the reduction of free association time throws a not uninteresting side-light upon its possible importance in other sorts of practice, where, owing to the fact that there is more repeated traversal of the same association paths, we may tend to place the burden of explanation rather upon decreased resistance in the path itself.

So much for the practice effect on association time. With regard to any possible effect on the nature of the responses, may be first noted the number of times the same response is repeated during the same hundred words at the beginning and end of the practice. This per cent. of the repetition is calculated for the two series that were repeated, thus for the same stimulus words, and forms a basis for some striking comparisons. The per cent. of repetition before practice and after practice is as follows for the different subjects:

Subject . . .	Black	Brown	Red	Orange	Green	Blue
Before Practice	8	11	14	16	17	13
After Practice	8	7	7	4	22	8

The regular tendency is to reduce the number of repetitions. In the case of GREEN the increase is essentially a matter of the subject's developing a "set" towards reacting with such responses as *large*, *small*, and *grand*, whenever they were available. With the exception of the physician, the remainder tend to particularize their responses more, as their practice in reacting gives them greater *Sprachfertigkeit*. It seems likely that BLACK, with his much greater initial *Sprachfertigkeit*, had already developed the quality that makes for decrease in repetitions beyond the point where practice would bring out any special change. The general trend towards greater particularization of the responses by practice, of which the decrease in repetitions is an aspect, will be discussed below in greater detail.

Further individual difference appears in the frequency with which a given stimulus word elicits the same response in both the initial and the repeated series, the figures on this point being as follows:

NUMBER OF CASES IN WHICH THE SAME RESPONSE WAS GIVEN BOTH BEFORE AND AFTER PRACTICE¹

	Black	Brown	Red	Orange	Green	Blue
Average number of identical responses in the two repeated series	16	22.5	20.5	9.5	21.5	22.5
M. V. of this average . . .	2	2.5	4.5	.5	1.5	1.5

Save for the probably negligible incidence of the memory factor, the number of times in which a different response is given is in the nature of an indication of the adaptability of the individual's thought processes; that is, of the capacity for differential response in relatively similar external situations. This is somewhat complicated by the factor of special education, because an educated subject possess an artificial

¹ There are a few cases in which the stimulus word was not understood the same in the repeated as in the original series, but these are negligible for the results. While it is not strictly a practise phenomenon, it is worth quoting for comparison with the above. (Fuhrmann.)

capacity to differentiate his responses more than an uneducated one. Thus, as above, the physician again gives a relatively small number of repetitions of the responses under these conditions. But of the women subjects who have approximately equal education, ORANGE differentiates her responses a great deal more than any of the others, more even than the physician; and she is the same subject who made the greatest gain in differentiation in the previous table. It may or may not be a coincidence that this subject has been placed in more responsible positions, also credited with more than ordinary *resourcefulness* in her professional work. And however much one might naturally incline to stress the merely educational explanation of this greater differentiation of responses, one must never forget that superior education to a certain extent implies superior ability to acquire it, in the fundamental dynamic correlation between superior innate endowments, and superior opportunities for developing the powers which they confer.

For these differentiated responses, there then presents itself the question of whether they are differentiated along any particular lines; *i. e.*, of whether the response, besides changing in form and in content, tends also to change in association type.

Before attempting to consider any possible effect of practice on the form of association, it must be thoroughly understood that the ordinary means of classification are at best very subjective. Such categories as the *Sachliches Urteil* and *Werturteil* are wholly continuous, even though they may cover a considerable range, as from a commonplace predicate like *handkerchief—white*, to such a highly particularized reactions as *journey—distasteful*. These last are what Jung calls the "egocentric predicates." The same continuity exists, of course, between the *Eingeübte sprachliche Verbindungen* and the *Sprichwörter und Zitate*; thus one might with equal justice assign *citizen—Roman* to either group. The most unfortunate confusion, however, is that likely to arise between these language-motor responses and those from the upper associative categories. Every experimental series is replete with reactions where this vital distinction is itself largely a matter of "*persönliches Werturteil*." *Only—chance* and *never—settled* may be harmless *Geläufige Phrasen* or highly egocentric predicates. *Betray—criminal* may be a *Subjectverhältniss*, an *Objectverhältniss* or an *Urteil*; *spread—feast* an identity or a predicate; *lady—gentleman* an opposite or a co-existence. The stimulus *cart* may elicit the response *horse* through the medium of word-compounding, familiar phrase, or co-existence. Shall we call *itch—scabies* an identity,

a co-ordination, or a Kausalabhängigkeit? *Fun—loving* a Wortergänzung or an Urteil?

Such examples could be multiplied indefinitely but are, perhaps, sufficient to show the character of the difficulties encountered in attempting an impartial classification of associative responses along the conventional lines. If available, reliable introspective data would go far towards removing them, but the very nature of the experiment usually renders this aid impracticable. With the more recondite responses, such classification is nearly meaningless. The interpretation, without reliable introspective data, of such reactions as *pole—legs*, *satisfy—savage*, *almost—conditional*, *enough—period*, *expect—to-morrow*, *justice—execution*, and the like, is little more than guesswork.

Subject to these reservations, then, the forms of association here justifying separate consideration may be enumerated as follows:

<i>Description</i>	<i>Approximately corresponding to Jung's</i>	<i>Example</i>
1. Failure of response	Ausbleiben des Reaktionsworts
2. Egocentric	Egozentrische Reaktion Direkte Ichbeziehung	succeed—I must
3. Egocentric predicate	Egozentrisches Prädikat	lonesome—never
4. Judgment of quality	Werturteil	rose—beautiful
5. Simple predicate	Sachliches Urteil	spinach—green
6. Subject relation	Substantiv-Verbum Subjektverhältniss	dog—bite
7. Object relation	Substantiv-Verbum Objektverhältniss	deer—shoot
8. Causality	Kausalabhängigkeit	joke—laughter
9. Co-ordination	Beiordnung	cow—horse
10. Subordination	Unterordnung	food—bread
11. Supraordination	Ueberordnung	rat—animal
12. Contrast	Kontrast	sunlight—shadow
13. Co-existence	Koexistenz	engine—cars
14. Identity	Identität	expensive—costly
15. Language—motor	Eingeübte sprachliche Verbindungen, etc.	town—state
16. Word-compounding or completing	Wortzusammensetzung, Wortergänzung	side—board
17. Pure sound associations	Reime	pack—tack
18. Syntactic change	Syntaktische Veränderung	deep—depth

The categories are divided into three groups. In the first group are those usually implying a special emotive element in the association; the second contains the more intellectual associations, while the very superficial associations are summed up in the third group. The associations from the present

material not falling into any of the above categories, it is impracticable to classify with any pretence of objective validity. They are also negligible in number, and indeed, several of the specified categories are very meagerly represented.

The tables indicate that in BLACK the type of association has undergone no particular change, except for trebling the number of contrasts, and halving that of the subordinates. The number of supraordinates, however, is remarkably small, which is significant in connection with this subject's superior education, and the tendency elsewhere for the supraordinates to decrease with practice. In BROWN, the responses show a slight tendency downward in the scale, and a considerable decrease in the supraordinates. In RED, this downward tendency of the associations is more marked, there being also a loss of half the supraordinates, which become mainly co-existences, these trebling in number. There are six of the third group of associations in the repeated series, to none at all in the original ones. And in respect to form of association, ORANGE again shows the most marked change of all. The predicates, many of which are quite egocentric in character, are decreased by about one-half, being relegated mainly to the language-motor, the word-compounds and the co-existences. There are also less than a third of the original number of supraordinates, these again becoming mainly co-existences and language-motors. Consequently these lower forms of reaction are greatly increased in number, there being 47 of them in the repeated series compared with 16 in the original. Notable is the total absence of contrast associations, also in Subject GREEN. The reactions of GREEN show no special change in type, except for the same decrease in the supraordinates, which here change to such responses as *large*, *small*, *grand* (as noted previously), technically predicates, but it is doubtful whether they are actually much more than language-motors. BLUE, however, shows a peculiar tendency to change the associations originally supraordinates to predicates of a higher order; e. g., *donkey-animal*, *donkey-bray*. The lower categories are also better represented than at the start, so that the general result is to make the association type more variable than before. The remarkable shortness of the times in this subject will be remembered, and the responses themselves show rather greater superficiality than can be indicated in the classification.

In the totals, two main trends are apparent, which, however, cross, and to some extent mask, each other. First, the tendency, especially mentioned for some of the individual subjects, for the whole body of responses to move down in the scale of associations, and secondly, the tendency to greater

According to this system, it seemed that the associations in the two repeated series were most reasonably to be classified as follows:

CHANGE IN THE TYPE OF ASSOCIATION TO THE SAME STIMULUS WORD AFTER PRACTICE ASSOCIATION TYPE.

Subject	Black		Brown		Red		Orange		Green		Blue		Total Practice Before	Total Practice After
	before	after	before	after	before	after	before	after	before	after	before	after		
1. Failure of Response	3				1				1				5	1
2. Egocentric (Direct)	1	1	1	1				11	7				14	8
3. Egocentric Predicate	4	3	1	2	5	3		14	5	19	20	2	45	39
4. Judgment of Quality												6		
Total													65	48
5. Simple Predicate	3	5	4	6	2	3	8	5	13	13	1	4	31	36
6. Subst. Vb. Subj.	2	2	1	1	2	2	5	2	7	1	1	2	4	6
7. Subst. Vb. Obj.	4	6	3	2	2	3	5	2	7	2	7	3	19	25
8. Causality	4	4	1	1	3	4	1	1	4	1	4	5	14	18
9. Co-ordination	22	26	22	21	19	21	9	13	5	8	15	17	92	106
10. Subordination	17	9	3	4	2	3	2	10	3	2	2	5	29	33
11. Supraordination	4	5	21	14	34	17	34	10	25	15	27	11	145	72
12. Contrast	3	9	3	4	6	6	9	17	2	3	8	6	20	25
13. Co-existence	14	10	10	13	5	16	9	17	2	3	15	10	55	69
14. Identity	12	12	27	25	19	18	5	5	24	22	18	19	105	101
Total													514	491
15. Language-motor	4	3	1	5	3	3	2	16	2	3	3	5	12	35
16. Word compounding or completing	4	3	1	1	1	1	9	1	1	1	3	3	5	18
17. Pure Sound Reactions	1	1											1	1
18. Syntactic Change	1	1			2		1				1	3	2	6
Total													20	60

Grand Total, 599 responses in each column. One stimulus word was unfamiliar to Subject GREEN.

particularization of the response as indicated mainly in the decrease of over 50 per cent. in the supraordinates. The latter is closely related to the decrease in repetitions described on page 7, for these repetitions consist largely of such supraordinate responses as *animal, food*, and the like, which have many subordinates among the stimulus words. For obvious reasons, these classifications do not lend themselves readily to further illustration of this tendency. It is plain what great individual differences there are in the amount of change of association type, but it is not easy to say just what ultimately constitutes these differences; they are not closely related to education. One is practically reduced to the tautology that as practice tends to lower the association type and to decrease the supraordinates, those individuals are most liable to practice effects who show the upper levels of association type, or a marked tendency to generalization in their responses. The *Sachlicher Typus* certainly shows the less change in association form, and probably also in reaction time.

The precise nature of these changes will perhaps be made clearer by the following illustrations. The comparative association times before and after practice are given, as usual, in 5ths of a second.

Stimulus word	Response before Practice	Response after Practice
<i>Greater Particularization</i>		
ancient	man 11	and Honorable Artillery 10
bank	building 13	England 9
contrast	judgment 46	black and white 8
dog	animal 22	Airedale 16
engine	machine 10	Morris Heights 21
herald	king 8	Globe 7
little	child 16	statue 12
parlor	room 8	sitting-room 7
swift	runner 12	Mercury 17
wheat	vegetable 24	cream of wheat 15
<i>Greater Superficiality</i>		
axle	hub 11	grease 6
axle	wheel 9	tree 6
bank	money 7	banker 5
discretion(twice)	wise 22, 42	valor 11, 6
lady	refined 21	man 19
pancake	tough 12	flour 9
shadow	shade 16	wall 6
spread	distance 17	bed 7
suffer	weak 11	pain 4
weak	frail 10	strong 13

In spite of the tendency to greater particularization, it is not unnatural, in view of these latter instances, that such actual "Komplexmerkmale" as are given in the reaction time, and in the form of the association as well as in the content of the response, should also be somewhat reduced by practice. The subjoined instances will serve to show what is meant, though some of the most striking examples of this tendency are not included.

Stimulus word	Response before Practice	Response after Practice
breast	face 22	milk 10
common	loose 14	Boston 8
flirt	disgusting 26	bird 9
heaven	peace 18	hell 6
person	woman 40	body 9
rat	ugly 27	large 10
sister	(Anna) 25	brother 7
virtue	good 18	reward 7
want	cherish 38	wish 9
whiskey	dangerous 13	Bourbon 7

The "complexual" character of the responses is apparently much diminished. This phenomenon should be attributable mainly to decreased emotive value in the stimulus-words, and only very sparingly to any greater expertness in dodging. In the above instances, the time is rather short for dodging, even though the occasion had presented itself. In this connection, it is well to bear in mind that special emotional reaction to a stimulus word may be a *product* of long association time as well as a cause of it, since the greater the tendency to hesitation, the greater the opportunity for emotive associations to be introduced. With greater facility of response, whether inherent or gained through practice, the importance of such a process is much reduced.

In brief, then, these experiments indicate the usual effects of practice on free association to be:

1. To decrease the association time to a limit approximating 6 fifths of a second for the median of 50 associations. At the beginning of practice, the subject may be any distance from this limit up to 15 fifths or more.

2. To further differentiate and particularize the responses, by increasing the readiness with which the subject's entire vocabulary becomes available for the purpose of such response.

3. To "flatten," or make more superficial, the form of association which the responses take.

4. To decrease the emotive value of the experiment, and consequently its applicability for all purposes involving its emotive value.

A PRELIMINARY EXPERIMENTAL STUDY OF THE CONSCIOUS CONCOMITANTS OF UNDERSTANDING

By HIKOZO KAKISE

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INTRODUCTION

When a word or a phrase is presented to an observer for a certain interval of time, it will awaken a succession of various events in his mind, such as inner reading of the word, "sense of meaning," suggested images of objects or of other words, and so forth, attended by various feelings and emotions. In the succession of these experiences, some will precede, others succeed, and still others occur simultaneously with understanding. The purpose of the study about to be reported is to examine, by a special method of experimentation, first the relations of the events preceding and succeeding to understanding, and then to investigate the nature of the simultaneous events, *i. e.*, the consciousnesses of meaning.

The method of experimentation was the same as that used by Marbe,¹ Messer,² Bühler³ and others in their studies of thought, the so-called "*Ausfrage* experiment" by which

¹Marbe, K.: Experimentell-psychologische Untersuchungen über das Urteil, 1901.

²Messer, A.: Experimentell-psychologische Untersuchungen über das Denken, *Archiv für die gesamte Psychologie*, 1906, Vol. VIII.

³Bühler, K.: Ueber Gedanken, *Archiv für Ges. Psychol.*, Vol. IX.

the total introspection of observers in reaction to words or phrases is taken down in the form of protocols upon which the conclusions are based.

Remarks on the Method: In spite of certain objections against the method from the side of certain psychologists, as Wundt, who calls it a "pseudo experiment" (*Scheinexperiment*) because it satisfies none of the four requirements of a psychological experiment, viz.: concentration of attention, repetition of the experiment, methodical change of the conditions, and the observer's own determination of the phenomena to be observed,¹ yet the present writer had recourse to this method partly as a kind of trial, and partly because there are no other methods which seem better suited to the present purpose. The reasons for this belief are: (1) That this method leads to division of labor, in that the experimenter and the observer are different persons, and thus permits the observer to observe the mental phenomena more freely than in ordinary introspection in which the observer and the experimenter are one and the same person and so attention must be divided between two different tasks, one active and one passive, which fact itself is of great detriment to the efficacy of introspection, especially when it has to do with marginal conscious experiences, as is the case with the present study. Galton² in his early experiments on association was surprised to find so many associations connected with a single word, when he separated the active and the passive attitudes by a special device. (2) As a comparative study, it is freed from any individual peculiarities or prejudices of the investigator. And lastly, (3) as the totality of the introspection is set down, it permits the investigator to take a fairer view of the position and significance of any particular element in the totality of the mental reaction than would be possible in any other way. This last characteristic brings this study into connection at some points with the three important studies of the present time, viz.: the study of mental types, the study of thought-processes, and the study of associations. The study of individual types deals principally with the individual differences of the means to understanding. The study of thought-processes deals with the direct conscious concomitants of understanding. And the study of association deals with the suggested conscious experiences after the understanding. Each of these studies considers only one section of the conscious concomitants independent of the others, all of which can be found in the preceding, simultaneous and succeeding concomitants of understanding.

THE TECHNIQUE OF THE EXPERIMENTS

Three series of experiments were made in succession for the same purpose and mainly under the same conditions, except for differences in observers and apparatus and in some added elements in the third experiment.

Stimuli. The stimuli were words and phrases familiar and unfamiliar (or easy and difficult), concrete and abstract, mostly English or foreign. For abstract phrases proverbs were mostly used. For example: (familiar concrete words) snake, hand, mountain, etc.; (familiar abstract words) philosophy, psychology, fatigue, etc.; (unfamiliar concrete words) timbrel,

¹*Psychologische Studien*, Vol. III, No. 4, Sept., 1907.

²F. Galton: *Inquiry into Human Faculty and its Development*. London, 1883, chapter on "Psychometry."

nostrum, nabob, etc.; (unfamiliar abstract words) pistology, oneirology, noumenon, etc.; (familiar concrete phrases) The sea is calm; The milk smells sour, etc.; (familiar abstract phrases) Union makes strength; Duty before pleasure, etc.; (unfamiliar concrete phrases) Long tongue, short hand; One man is no man, etc.; (unfamiliar abstract phrases) A sin concealed is half pardoned; Time enough is little enough, etc. A new kind of stimulus (meaningless visual stimulus), in the form of Chinese characters, was added to the verbal stimuli in the third experiment. The account and description of these will be given later (Part III, § 3).

Forms of Reaction. Two forms of reaction, an active (or short) and a passive (or prolonged) were used. In the active reaction the observer was asked to react by saying "yes" in the first experiment, and by pressing an electric key in the second and third experiments, as soon as he understood the word or phrase, and immediately afterward to report in the order of its occurrence the whole process (or as much as he could recall), which took place in the interval between the sensory perception of the stimulus and the reaction. In case he did not understand the stimulus the observer was asked to give in the same way his introspection as to what occurred in the interval between the perception of the stimulus and a signal which was given by the experimenter after the lapse of five seconds from the presentation of the stimulus. In the passive reaction, the observer was asked to remain passive without reacting, but to let the processes go as they would until a signal for ceasing (which was given by the experimenter this time at the end of three seconds) and then to give his total introspection for the interval as before.

Presentation of the Stimulus. Two ways of presentation were used: an auditory, in which it was spoken by the experimenter, and a visual, in which it was exposed.

Apparatus. The apparatus for the exposure of the stimulus and the measurement of the time in the first experiment was simply a set of cards with typewritten words and phrases and a stopwatch reading to one-fifth of a second. In the second and third experiments this simple apparatus was replaced by a more elaborate one constructed for the purpose. It consisted of three principal parts, *i. e.*, (1), an exposer; (2), a registering apparatus, and (3), a control pendulum. The exposer consists of a large board with an opening about in the centre, behind which stimulus-cards were exposed in turn by means of the rotation of a large wheel attached behind the board. Between the stimulus-card and the opening there was a sort of fan which closed the latter until it (the fan) was raised.

The fan was attached to one end of an electro-magnetic lever and was raised and lowered under the following conditions: (1), in case of the passive reaction, it was moved automatically by means of the control pendulum which made and broke the circuit to the lever, the circuit being kept closed, and at the same time exposed the card for an interval of three seconds; (2), in case of the active reaction, the experimenter moved

the fan by a make-key; and (3), in case the observer did not react, owing to the difficulty of understanding, the experimenter lowered the fan at the end of five seconds. Two mercury contacts at the free end of the lever made, and the reaction key of the observer broke, another circuit passing through an electro-magnetic marker which traced the reaction curves on the smoked surface of the drum of a Zimmermann kymograph. The marker of a Jacquet chronograph cut time units on the curve which could be read by one-tenth of a second. Between the fan and the opening there were two shutters or slides meeting at the middle of the opening when both were closed, and exposing one-half of the whole surface, when one was opened, which was done for words. With phrases both slides were opened. Special care was taken for the prevention of all distracting noises. In its rotation the apparatus made but very slight noise which was almost totally shut off from the observer by the large board of the exposer. The most comfortable position of the observer was obtained by the adjustable inclinations of the board and the height of the chair.

Observers. Of the 14 observers, 3 were Japanese students reading and speaking English fairly well, all others were English speaking people. The distribution of the observers and their qualifications were as follows:

Experiment I.

An.	Outsider.
Cff.	Student of psychology.
Gl.	Student of psychology.
Hi. (Jap.)	Outsider.
Kk. (Jap.)	Student of psychology.
Kn. (Jap.)	Student of psychology.

Experiment II.

Ac.	Student of psychology.
Ky.	Student of psychology.
Sm.	Student of psychology.
St.	Student of psychology.

Experiment III.

Ac.	Student of psychology.
Ch.	Student of psychology.
E. M.	Student of psychology.
L. M.	Student of psychology.
Sn.	Professor of psychology.

Remark: Several auxiliary experiments and minor tests with some of these observers and several others were made for special points. The description of these experiments has been omitted owing to the limits of space, and references only will be made to them.

Samples of the Protocols. The following are a few samples of protocols from among nearly five hundred thus obtained. They represent about the average length or amount of the reports, some being shorter and simpler, while others are longer and more minute. They are the reactions of three

observers, one from each experiment, to familiar abstract words, (A) by the active form of reaction, and (B), by the passive form. The stimulus-words for observer Cff., in the first experiment, were spoken, while those for others were exposed. Even by reading these few, which are typical in many respects, the reader may find traces of the general influence of these conditions upon certain of the concomitants.

A. ACTIVE REACTIONS

Fault. (I exp. word spoken; Obs. Cff. No. w. 20, time, 1.5".) 'While you were saying the first half of the word I wondered what you were saying. Then the sound lingered. And I got the consonant at the end, and the meaning at the same time. No image, but just feeling of meaning. There was a feeling of satisfaction and at the same time dissatisfaction. It is hard to analyse.'

Peace. (II exp. word exposed; Obs. Ac. No. iii-5, time, 0.5".) 'Expectant attention keyed up owing to the delay of the arrival of the stimulus word. I read it in inner speech and grasped the word and relaxed. I grasped it at once in a very vague way, but at the same time with a feeling of assurance that I was right although I had very little of any imagery.'

Psychology. (III exp. word exposed; Obs. Ch. No. iii-15, time, 0.5".) 'First feeling (of recognition of the form of the word) was followed instantly by the feeling of familiarity. There was no imagery. In this particular case it appears that the feeling of recognition and the feeling of familiarity are the same thing. There was n't any imagery, any attempt to define the word. I recognized the word. Absolute certainty.'

B. PASSIVE REACTIONS

Philosophy. (I exp. word spoken; Obs. Coff. no. w. 11.) 'I got the apperception of the word at once. It came in connection with a recent little discussion with a fellow student on some question on philosophy. It occurred just this morning. I had just the sound of his name, very vague, and hardly any visual image of the place where we had discussed. Then I began to pronounce the name of 'Weber'. Also the word 'examination' was present. But no definite visual image of anything.'

(Remark: The date of the experiment was shortly before the observer's doctor's examination and he was then reading Weber's History of Philosophy.)

Apperception. (II exp. word exposed; Obs. Ac. No. w. 36.) 'Read it in the same way as before. I at once thought of Dr. Sanford in the lecture room giving the definition of the word. I had a vague visual image of him and the room.'

Apperception. (III exp. No. iii-1, 16, word exposed; Obs. Ch.) 'Pronounced the word. There was no feeling of effort, but just feeling of familiarity which came at once. I did n't put the meaning in words, but had simply a feeling of what it meant. Then I got a peculiar visual image of myself taking notes of the lectures in the classroom. The name 'Wundt' came into my mind in auditory form. The feeling was rather neutral.'

On the Working up of the Protocols. These protocols were read with the following two points in view: First, the influence of the conditions upon the frequency of the concomitants. The conditions were divided into, *a*, material (*i. e.*, concreteness-abstractness of stimuli, etc.) *b*, experimental (whether the stimuli were exposed or spoken, etc.); *c*, individual (wheth-

er the observer belonged to the visual or auditory type, etc.). The primary or vital influences were taken chiefly into consideration though sometimes secondary influences were also considered. The second point was to examine the relative positions of the concomitants with reference to the understanding and to each other in the temporal sequence of their occurrence.

PART I

CONDITIONS OF THE PRECEDING CONCOMITANTS

These concomitants were reproductions of the sensory components of the stimulus word or phrase, *i. e.*, audito-motor reading and visual imagery of the stimulus. In ordinary introspection these events are often overlooked by many, as we are accustomed to attend only to the meaning and not to the means by which we reach it. According to their kind and frequency they have been sometimes used as criteria for individual differences. Sometimes they have been identified with meaning itself. In our experiments they were first to occur, following directly after the sense perception of the stimulus, excepting some cases with the visual reproductions which frequently followed, or occurred simultaneously with, the understanding. With easy or familiar stimuli the understanding followed immediately without any intermediary imagery. With unfamiliar or difficult stimuli there occurred often intermediary imagery such as the appearance of other verbal images in the form of synonymes, translations, definitions, etc., or suggested object images. As these were in their nature exactly identical with suggested images or those occurring after the understanding with easy stimuli we shall consider them later in Part II.

§ I. AUDITO-MOTOR REPRODUCTION OF THE STIMULUS

Influence upon the frequency of audito-motor reproduction of the stimulus (i. e., the reading of the word in inner speech) when (a) the stimulus was exposed and (b) when it was spoken.

The influence of these conditions was so marked and definite that the results of the three successive experiments showed the same tendency and the influence of other conditions was almost negligible.

The total number of cases of this form of imagery in ten observers was 291 out of 311¹ or 93% when the stimuli were

¹ From this total, the first one or two (tentative) reactions by most observers are excluded. Also the reactions of four observers *An. Kk. Sm. St.* are excluded, for the reason that in case of the first two, the stimulus was spoken only. In the case of *Sm.* the tendency was not clear in the earlier reactions, with *St.* it was not clear in any of the reactions; about these see the succeeding accounts.

exposed; while it occurred in only 17 out of 176¹ cases, or 9%, when the stimuli were spoken. And these 17 cases occurred mostly when the stimuli were rather difficult or the pronunciation by the experimenter were indistinct.

We may conclude, therefore, that the inner reading of the word generally occurs when it is exposed, and occurs seldom when the stimulus is easy and spoken. In other words, the appearance and non-appearance of the reading of the stimulus in inner speech is primarily conditioned by the way of presenting the stimulus.

For the determination of the secondary influences affecting the frequency of this imagery, *i. e.*, influences arising from individual differences and from qualities of the material, some minor tests were made. The first thing to be mentioned is the fact that at the start the majority of our observers did not know about this tendency, but became aware of it after a few reactions, owing, perhaps, to the summation of the faint impressions occurring repeatedly at the same place, *i. e.*, at the beginning, in each reaction. One observer (*Hi*) belonging to a very pronounced motor speech type was not aware of this tendency at first, but a simple test of reading with the mouth open, etc., brought his attention, to his great surprise, to what he was actually doing. A lady (extra observer) who when asked about this tendency denied it, or was at least doubtful about it, was also surprised to find it after a simple test, and confessed that in her whole life she was never aware of it before. With another extra observer (*Ms*), to whom the presence of this tendency was doubtful, a test with simultaneous counting or speaking of a word showed that he could not read understandingly while they were continued. With two observers (*Sm*, *St*) who said they read by eye, the results of a test in the instantaneous grasping of a list of unconnected words or phrases showed no differences from other observers, either in the amounts of reproduction or in the ways of reading, *i. e.*, instead of grasping the whole at once at a glance they also read the words one by one in the same way as others. One of them (*Sm*), who denied the presence of this tendency in the reading of some phrases, found it later almost always with words. There was thus only one observer remaining who did not yet find in himself this tendency. No further tests with him have yet been made, so I cannot absolutely decide at present whether this process is really lacking in him or he is only

¹ This smaller number of spoken stimuli is due to the fact that in the majority of the second and the whole of third experiment the stimulus was only exposed.

not yet aware of what he actually does, but from the results of the test above stated and from the reports of many about the difficulty of the introspection of this tendency, the most probable supposition is that he is subject to it in spite of himself.

The distinction between the auditory and motor elements in inner reading our observers found it hard to make. But if we call the inner reading with the consciousness of the innervation or movements of the organs of speech motor, and the inner reading without these auditory, then there was at least one observer (*Hi*) who belonged to a pronounced motor type, like that of Stricker. With difficult words or phrases most of our observers went over to the motor speech form, and inner reading became pronounced. With familiar words of many syllables or with familiar phrases, mutilated reading was not uncommon. In the repeated reading, in inner speech, of unfamiliar and difficult phrases (mostly proverbs), reading with emphasis upon the principal words, or first a quick reading of the whole followed by a return to the principal words, was also frequent.

The conclusions suggested by these studies are: (1) that motor reading in the sense above defined is not universal as was believed by Stricker¹ and assumed by Max Müller,² but is limited to some individuals only, and with average individuals, to the reading of difficult words. (2) Auditory reading in the above sense, on the contrary, seems universal and necessary for the understanding of exposed words or phrases, which favors the view that the connection between the auditory and "concept centres" is immediate, while that for the other senses, *i. e.*, motor and visual, is indirect.³ (3) Expressed in terms of conditions, the occurrence of auditory reading is mainly conditioned by the method of experimentation (*i. e.*, exposure), while that of motor reading is influenced by (1), the method,⁴ (2) the material, and (3), individual differences.

¹ Stricker: Ueber die Sprachvorstellungen. Wien, 1880. One of his tests is: "Keeping the mouth open and the tongue firm try to think the words papa, morning, stammer, etc.; If you do not succeed it means that a motor image is necessary to your inner speech."

² Max Müller: Lectures on the Science of Thought. 1887 (1st ed. 1883), Chicago. In his letter to Galton he unconsciously betrays his type in these words: "Yet if we watch ourselves, it is very curious that we can often feel the vocal chords and the muscles of the mouth moving as if we were speaking." *Ibid.*, Appendix, p. 8.

³ Cf. Dodge: Die motorische Wortvorstellungen. Wien, 1896, p. 62.

⁴ The writer thinks it very likely that even persons of pronounced motor type would not experience innervation or movements of speech-organs in the case of *hearing* easy or familiar words.

§2. VISUAL REPRODUCTION OF THE STIMULUS

Influence upon the frequency of visual reproduction of the stimulus when (a) it was exposed and when (b) it was spoken.

There occurred 30 cases of visual imagery of the stimulus out of 81 reactions with 3 observers (*Gl, Hi, Kn*) when the stimuli were spoken, and not a single case occurred in 61 reactions, with the same observers, when the stimuli were exposed. With two other observers (*Ac, Cf*) no case occurred—neither when the stimuli were exposed (71 reactions) nor when they were spoken (65 reactions). With the rest of the observers none occurred in 179 reactions to the stimuli which were exposed.

Of the three observers with whom this form occurred, two were Japanese. This naturally led to the suspicion that the frequency of this imagery for them might perhaps be the result of unfamiliarity with the English language, and not to individual peculiarity. This suspicion was, however, soon dispelled by an extra test with a number of Japanese words as stimuli which showed the same results; the only difference being that with Japanese words orally presented, the visual image of the stimulus was directly followed by understanding or suggested images, while with English words, the visual image of the stimulus was sometimes succeeded by the image of the corresponding Japanese word before the arrival of understanding or suggested images.

For instance: '*Traveller*'—(Obs. *Hi*). "First I heard distinctly the sound 'travel'. There was a moment of hesitation and doubt. Then I saw the printed image of the word 'traveller' accompanied by the images of the Chinese characters, and at the same time full realization of the meaning. Then I saw mentally a traveller walking on Main Street."

The form and localization of the image were almost constant in the same individual. All three localized the image, usually at a distance of one or two feet in front. Two observers saw the image in handwritten form, the other in printed form.

About the frequency and conditions of suggested (associated) verbal imagery we shall see later.

These results lead to the conclusion: (1) that the visual image of the stimulus word occurs only occasionally when the stimulus is spoken, and scarcely occurs at all when it is exposed. (2) There are great individual differences in this experience as in the case of motor speech; in other words, this imagery is especially conditioned by the method of experimentation and individual differences.

The results of the above study in so far as they bear upon the validity of the customary method for the determination of individual types are, in certain respects, negative,

viz.: (1) We cannot wholly rely upon the questionnaire method in the study of types, because there are many people who are not aware of what they are actually doing. (2) Because the frequency of these speech forms primarily depends upon the manner of the presentations of the words, we cannot at once label an individual as, for instance, of the auditory or the motor type simply because he says that he pronounces when he reads or when he writes, etc.¹

PART II

CONDITIONS OF THE SUCCEEDING CONCOMITANTS

When the stimuli were easy and familiar, the images of other words than the stimuli and the images of objects or events suggested by the stimuli, or, in short, what I might call "suggested images"² usually followed the understanding. When the stimuli were difficult or unfamiliar, they frequently preceded the understanding, but followed the images of the stimulus words or phrases. These cases of preceding suggested images I will call "intermediary images." All these images, the preceding, intermediary and succeeding suggested images, were of the same kinds differing only in their temporal relations to understanding.

All the suggested images which we need consider may be divided into two main classes, according to the nature of their constituents: (1) object images, which are the representations of, or references to, concrete objects or events; and (2) verbal images, which are the visual or audito-motor representations of suggested or associated words.

I shall begin with the succeeding suggested images or images following the understanding of familiar stimuli, and first with one of the object images, namely with memory images.

¹These considerations seem to be neglected by the following authors Patini, E.: *Contributo allo studio sperimentale della formula endophasia*. Napoli, 1907. Cf. p. 26, observation xlv, and others. Ribot, T. *L'évolution des idées générales*. Paris, 1897. Cf. Eng. tr. 1899, p. 114 ff. Max Müller. *op. cit.* Appendix p. 26. There in his answer to Romanes he wants to prove the universality, among all individuals as well as under all circumstances, of motor speech in thinking, by referring to special cases, and says: "How could I hold pronunciation necessary for thought when I am silent while I am reading, while I am writing?" When one listens, it is not necessary for understanding to pronounce each word.

²The term "image" is used here in the broadest sense including faint and indefinite experiences if they refer to concrete objects or events which can be in other cases distinctly represented. For instance, such experiences as, "I thought of that typewriter," or, "I thought of my buying a typewriter at a store about a year ago," etc., were also included as images though it is by no means clear in these cases what kinds of sensory imagery, (*i. e.*, visual, auditory, kinesthetic, etc.), were actually present. For further analysis of these experiences see Part III.

Experimental conditions. The numerical results given in the following discussion are from the records of reactions to familiar stimulus words (not phrases)¹ by the ten observers who assisted in the first and second experiments. Results from the third experiment, which confirmed those of the preceding ones, are sometimes referred to. The frequency ratios of various sorts of imagery are regarded merely as representing general tendencies or main proportions and not at all as expressing exact or even approximate relations. The total number of reactions to easy words² in the first and second experiments under the different conditions, whose influences we are about to examine, was 285.

§1. MEMORY IMAGES

By memory images here are understood those which refer to particular personal experiences with the objects or events indicated by the stimulus. They can always be localized in space (where) and time (when). They are sometimes called reminiscent associations.

Results of the Experiments. 1. This imagery not only followed the understanding, but also made the terminus of the reactions in which it was found, *i. e.*, other suggested images when they occurred generally preceded, and seldom succeeded, the memory imagery in the allotted intervals of the time, owing, perhaps, to the richness of contents or the vividness of the latter.

For instance: *Wheel* (passive, spoken stimulus, Obs. *Gl.* No. w. 22). "First thought of all kinds of wheels, blurred and indistinct images of multitude of wheels. Then the one I saw recently emerged very clearly in mental picture."

Vacation. (Passive, word exposed, Obs. *Ac.* No. w. 5.) "I read and spelt the word twice mentally. I thought of week-after-next and the work that I planned to do then. I think I thought of that because I have been recently thinking about the work to be done. And then I recalled the conversation I had in the Bloomingdale hospital a few minutes ago. Dr. C. proposed to meet his class next week. Some one said we had vacation then and Dr. C. had trouble to understand on account of his ear trouble."

¹ The results with phrases were not calculated for the following reasons: (1), The number of words used as stimuli, was greater than that of phrases; (2), there were no new kinds of suggested images found in phrase-reactions, those in the word-reactions and phrase-reactions being practically the same (Ribot found the same results; *Cf. op. 114 ff.*) the only definite difference being that with phrases the suggestions were more definite a matter which we shall consider later; (3), furthermore with phrases, not only the suggested images were less in number and more limited in variety but very frequently there occurred none at all, perhaps because the reactions took more time and energy than those with words.

² Ease and difficulty are only relative distinctions, some of the easy stimuli turned out to be rather difficult or unfamiliar ones to some observers. *Cf.* the remark under § 6 (Difficult stimuli).

The scene of the classroom was pretty vivid, and it was rather interesting, amusing."

2. This imagery in the case of passive reactions occurred in more than two-fifths of all the reactions of all the observers, the figures being 102 cases out of 242 passive reactions to easy words for ten observers with no considerable individual variations among them. The active reactions with easy words (not phrases) in the first experiment were in all but 37, made by three observers (*An*, *Gl*, *Kn*). In these 37 there were four cases of the sort of imagery now under consideration, all in the 15 reactions of observer *An* alone. The results of the third experiment, in which the majority was of active reactions, showed also very definitely that a memory image seldom occurs in active reactions. In all the cases in which it occurred, the observers reported it as occurring after the reactions or at any rate after the understanding.

3. Abstractness in the stimulus words had little or no influence on the frequency of this imagery in passive reactions to easy stimuli, or, if any, tended more toward inducing recent memory images than did the concrete words. Of 50 cases of recent memory images¹ out of 151 reactions in the case of five observers (*Ac*, *Gl*, *Hr*, *Kn*, *Ky*) 30 cases occurred with 74 abstract words and 20 with 77 concrete words with no considerable individual variations. The same general tendency is shown in the results with the rest of the observers. Likewise, whether the stimuli were spoken or exposed made no noticeable difference with the frequency of imagery of this sort.

4. Beside the passivity of reaction, the most important factor for the occurrence of this imagery was the recency of association. (a) Out of 106 memory associations in 285 total reactions 82 were recent associations or reminiscence of events falling within a period of not more than one or two years previous, 21 of remote associations, and only 3 of boyhood associations.² (b) Quite insignificant and accidental associations, alone, without any emotional excitation or logical connection or repetition, by sheer power of recency, occurred frequently, pushing other images aside.

¹ These were the most frequent of all memory images; for details, see the sections to follow.

² These results rather contradict those found by Galton. The numbers which he found were: boyhood associations 48, manhood 57, while "quite recent events" had only 19, in his four times repeated experiment with 75 words. These are, of course, the combined results of the pure revivability and the fixity or tenacity of associations as they were repeated. But even in his series of the first reactions the frequency of recent associations is quite low. Cf. *ibid.*, p. 195.

For instance: *Ink* (Obs. *Ky*), "Pronounced the word. Idea of the blackness was the first thing. Then I thought of those ink-blotters I got this morning at a down-town store. There was a vague image of blotters. I had also clear image of my fingers being dabbled with ink which occurred a couple of days ago."

Vacation. (Obs. *Sn*.) "I think I saw the middle of the word and noticed the syllable 'cat'. Then I read the whole word to myself. I think I had an incipient pronunciation of it in inner speech. Then I remembered that this word was the word which Dr. Bolton read when we came here yesterday. I didn't get any definite image except that association. I just thought of it, an idea of direction rather than a visualization. That idea of direction is very frequent with me as the first thing to come in case of such an association as that."

(c) With such words as *vacation*, *memory*, *fatigue*, *pedagogy*, *philosophy*, *apperception*, etc., which were purposely selected and used in the third experiment (mixed among other words), the observers had in nearly all reactions, in spite of the abstractness of the words, a concrete image in the form of a recent memory association which the experimenter could often predict from his own share in the same recent and repeated experiences, e. g., the images of certain professors, the classrooms, certain authors, etc.

For instance, *Apperception* (Obs. *E. M.*). "First slight tension and action on the motor side with the pronunciation of the word. Then the meaning came, but there was an effort to get the psychological meaning. I had a rather clear visual image of pages in Wundt's 'Outline of Psychology' in which the thing is treated. And then the visual image of Dr. S. in the lecture room and also some auditory image of his voice."

Pedagogy. (Obs. *Sn*.) "I was not quite ready. I read the word in inner speech but not very loud, and was not quite sure whether I read it correctly. So I read it again and I had a faint feeling that I knew the word. Then I thought of the direction of Dr. B's room and probably had also a very vague suggestion of Dr. B. himself. The consciousness of direction was very clear. I had the word 'teaching,' probably in inner speech."

Remark. Ribot's "thinking by analogy" by which he means such reactions as "I thought of Hume's theory of causality", for the stimulus "cause"; or the recalling of "Littré's definition" for the word "Justice", and so forth (*ib.*, 114 ff.), is merely our "memory imagery," and cannot therefore be regarded as a special mark of individual differences. The whole matter rests upon the duration (slowness or quickness) of reactions. Besides, with these familiar words, and especially in scholars, the understanding of what the words mean precedes any suggested images, so that such memory images are not means to understanding but the results of natural and spontaneous associations.

§ 2. INDICATIVE IMAGES.

For the lack of a better name, I have called "indicative images" those which referred to particular objects found in the

room at the time of the experiment. For instance: *Typewriter*, I thought of that typewriter on the table. *Experiment*,—I thought of this experiment, etc. In their psychological nature, a strict line of demarcation between this sort of imagery and recent memory imagery is hard to draw as the one gradually passes into the other; yet I found a separation, by the conventional definition above given, necessary in the treatment of the results for, in the first place, the frequency of indicative imagery was markedly more pronounced under certain conditions than that of memory imagery; and in the second place, it seemed to be influenced by a new factor soon to be mentioned. So that, when, for example, the word *Entrance* suggested to some observers the aperture of the experimenting apparatus, and to others the door of the experimenting room, while to a third the gateway of the university or that of the library, and to a fourth the entrance to the court house of this city, and to a fifth the entrance to the capitol in Washington and finally to a sixth some front steps leading up to a building, I put the first two into the category of the indicative imagery, the third and the fourth into the recent, the fifth into the remote memory imagery and the sixth into the general visual object imagery described later on.

Sometimes one and the same response, therefore, may become the one or the other of these types of imagery according to circumstances. For instance, with the word *Seminary*, if one thinks of Dr. H—'s seminary, or Dr. S—'s or Dr. B—'s, it will be a case of indicative imagery when the word was given there, while it will be recent memory imagery when the experiment was performed at some other place, and remote memory imagery when it was made years after the personal experiences of the observers.

But a very small number of appropriate stimulus words for the arousal of imagery of this sort, such as *room*, *window*, *entrance*, *watch*, *hand*, *typewriter*, *experiment*, etc., happened to be found in our list of stimulus words. Nevertheless the following tendencies were rather definitely brought out. 1. The occurrence of imagery of this sort is primarily conditioned by a special kind of stimulus words which I may call "indicative words," such as those just mentioned. 2. With these words this imagery occurred in far greater number of cases when the words were spoken than when they were exposed, the frequency ratios being respectively 89% and 19%. Further inferences with reference to this sort of imagery are impossible from the data at hand, but the following one is also suggested by the results, namely that this sort of imagery is determined by a new factor which I might call "implicit context"; in other words, the spoken stimulus word becomes

virtually in its effect a phrase especially adapted to induce imagery of this kind. In daily life we are accustomed to react to a single word under such circumstance when the object indicated by the word is near at hand and the speaker wants something to be done with the object. A single uttered word then is, in fact, an imperative sentence, meaning, for instance, 'Please give me that thing', or 'Look at it', etc., and the person addressed turns his attention instinctively to the object mentioned. Now in the experiment when an "indicative word" is uttered the observer falls unconsciously into this attitude, because of the similarity of situation; while, when it is exposed, this link of habitual associations becomes broken, whence the less frequency of this sort of imagery.

In the temporal order of occurrence, this imagery was, in general, the promptest of all suggested images, occurring immediately after, or sometimes simultaneously with, the understanding.

Remark. In this kind of reactions, which were, as a rule, rather reflex, the full realization of the meaning, such as richness of concept, came often later than the arrival of this imagery, though the understanding of the word in the sense of recognition obviously preceded it. To these varieties of meaning we shall return in Part III.

§ 3. ORGANIC IMAGES

Under this term I understand a reference to, or becoming aware of, the organic sensations or feelings either produced directly or revived, which are habitually associated with the words. The term organic sensations is used here in its broadest sense, comprising kinesthesia or sensations of muscular movements or innervation, as well as sensations attending the conditions of internal organs.

For instance: *Excitation.* (Ac) "Read it in the same way. I tried to state it in the sense given by Wundt. Then I tried to think about the psychological evidences of excitation, and simulated to myself its bodily state unconsciously." *Rain.* (Ky) "First pronounced the word inwardly. Next there was a visual image of raining just outside of this window (of the experimenting room). There was also an idea of wetness just in the form of bodily sensation in which no visual or auditory elements were discernible."

Results. 1. This imagery occurred in a very small number of cases. 2. It occurred only, (a) with a special class of stimulus words suggestive of this imagery or what I might call "Organic words", with which the organic associations (or components) more or less predominate, such as, *respiration, suffocation, fatigue, uneasiness*, etc., and (b) with passive (or prolonged) reactions. 3. It seldom occurred alone but

usually accompanied by other suggested images, such as verbal and visual imagery. In cases of its concurrence with these images it usually succeeded the latter, *i. e.*, it was less prompt in its occurrence than visual and verbal suggested images.

§4. GENERAL VISUAL-OBJECT IMAGES

These images, instead of referring to any particular object or event of past experiences like memory images, represent, predominantly in visual terms, merely types or concrete examples of objects designated by the stimulus words. For instance, *Box*—I had a visual image of a wooden box. This is a case of simple object imagery in which only a single object is represented. It occurred often that many objects belonging to the same class were visualized simultaneously or in quick successions, producing what I may call "complex object imagery." In such cases the visualizations were, as a rule, faint and incomplete. For instance, *Animal (An)*—I thought of all sorts of animals moving alive, etc. *Vacation (L. M.)*—(I had a very rapid visual impression of landscapes. The ideas or faint visual images of a whole summer.

Sometimes these general visual images, which as a rule preceded memory images, turned out to be the first stage of the latter, in the same way as free associations are sometimes traced to particular incidents. For instance, *Cross, (Ac)*—When I heard the word, there came at once the picture of a crucifix. It seemed to be traced to those pictures of crucifixes which Dr. H— showed us in his lecture on Christ. It impressed me at that time.

Results. 1. There were great individual differences in the frequency of this imagery, ranging from zero to 53%, in the percentages for the ten*observers. 2. In the case of this imagery, reversing the case with memory imagery, the concreteness and abstractness of the stimulus word influenced the frequency of the imagery in a marked degree, the frequency with concrete words being nearly three times as great as that with abstract words. 3. The frequency of imagery in all the reactions was 51 cases in a total of 286, falling thus far below that of the memory imagery, but rising far above that of the organic imagery. 4. In cases of concurrence this imagery always preceded memory imagery.

§5. SUGGESTED VERBAL IMAGES

By a "suggested verbal image" is meant here a visual or audito-motor reproduction of a word associated with the stimulus-word. It must be distinguished, therefore, from the verbal imagery of the stimulus word itself the conditions of which were treated at the beginning of this paper. Suggested

verbal imagery may occur either with or without corresponding object imagery, *i. e.*, dependently or independently. All independent imagery which appeared in our records was in the nature of either sensory or conceptual associations (*i. e.*, those having a sensory or a conceptual relationship with the stimulus words). In sensory associations we found only "klang associations," or associations by the similarity of sound. In conceptual associations there were roughly three kinds: 1, synonyms, 2, contrasts, and 3, co-ordinations, subordination and superordination. For instance (co-ordination) dog—cat; (subordination) city—New York; (superordination) cat—animal.

Results: 1. There were very marked individual differences in the frequency of this sort of imagery, as in the case of general visual imagery, the ratio ranging from zero to 100%; for instance Obs. *Ac.* had no cases of this imagery in all his reactions, while Obs. *Sm* always had it. Some observers had a few cases, others many. 2. The frequency of this imagery like that of general visual imagery was also markedly influenced by the nature of the stimulus word (abstractness or concreteness) and in this case in inverse relation. It occurred three times as frequently with abstract words as with concrete words. 3. As to "klang associations" there were only three of them in all the reactions, and only in the case of one observer (*Sm*), so that this form must be regarded as rather exceptional, at least with easy stimulus words. (Of the frequency of this form with unfamiliar words we shall speak later.) We had now and then phrase reactions from four observers most of which appeared in the form of definitions of abstract scientific terms. Conceptual associations in the form of synonyms, etc., however, made the majority of the cases. 4. As to the time of occurrence: With independent imagery, it was one of the quickest to occur; in case of concurrence with other images, it generally preceded the general visual image and the memory image. With dependent verbal imagery, the time depended on that of object imagery which the verbal imagery accompanied.

On the Tracing of Verbal Imagery

With independent verbal images the observer in the majority of cases could not give introspectively any account of their origin owing to the lack of conscious background.

For instance, *Excitation:* (Obs. *Sm*) "Pronounced. The word *psychology* came which was pronounced and visualized in typewritten form. Then I saw the German word *Erregung* printed in black. Then in inner speech I said 'I wonder why I selected these words.'"

This imagery though difficult to be traced subjectively is yet

easily traced objectively in the sense that it can be easily brought under conceptual or logical classifications. The dependent verbal imagery, on the contrary, is easy to trace subjectively and hard to trace objectively.

Suppose the observers reacted by uttering just the dependent verbal images in response to the stimulus words (without giving their total introspections) and then the experimenter or any outsider attempted to trace them objectively, as best as he could. Then compare the results obtained by conjecture with the actual connections found in the total introspections of the observers. Such a comparison is easily made by placing all such verbal imagery found in the protocols directly after the stimulus words. For instance, *Horse*, (Obs. Cf.) [suggested verbal images] "The name of a friend of the observer" and "horse."

Introspection: "The sound of your voice lingered. Then there was a sort of general idea. Then a rather pleasurable feeling due to the recognition of a favorite animal. There was a complex vague association there, such as a vague notion of a useful domestic animal. Then I had a visual image of being on horseback, with a sort of inner speech in auditory terms, going riding with a friend of mine who suggested it to me. I heard or pronounced his name and also the word 'horse' pretty distinctly. The localization of the scene of the riding was far down in the direction he (my friend) has suggested. The suggestion of his occurred but two days ago."

Turkey, (Obs. Sm.) "Bronze." Introspection: "First strong visual impression of the typewritten word and its color. Then came the word 'bronze' in the form of inner speech and at the same time a rather imperfect image of one of those big bronze colored turkeys. The color was more distinct than the outline. Not well localized, hovering somewhere around in the air. The color came out distinctly, the shimmer of the iridescent color. Considerably later, *i. e.*, after the shutter was closed, there came the thought that wild turkeys were once abundant in New England, but now almost extinct. Then came the idea of Thanksgiving, but not well defined, just a general idea of festivities."

§6. IMAGES WITH UNFAMILIAR STIMULI

It is known in a general way that the grades of acquaintance, *i. e.*, familiarity and unfamiliarity, with stimulus words or phrases have an important influence on the modes of reaction. Here we propose to examine in particular their influence, especially upon images.¹

By unfamiliar stimuli is meant here those words or phrases in the case of which understanding either did not occur directly (soon after the sensory reproduction of the stimulus) or did not occur at all.

The criterion is thus totally subjective (*i. e.*, according to the observers' modes of reaction) though a number of so-

¹ Their influences on the "feelings", we shall consider later in Part III.

called unfamiliar as also familiar stimuli¹ were provisionally fixed and used by the experimenter.

Some of these objectively fixed unfamiliar stimuli were naturally, by some observers, found to be familiar and some of the objectively fixed familiar stimuli were found by other observers to be unfamiliar, so that the following account of the influence of unfamiliar stimuli is taken from the results of all experiments (as we have seen in the treatment of the images of stimulus words).²

Results. The following results show that there is a striking similarity in the conditions of some images which attend the reactions to unfamiliar words and of those attending unfamiliar phrases.³

The influence of unfamiliar stimuli upon the the images (auditory, motor, and visual) of the stimulus-words, such as their increased frequency, accentuation, repetition, etc., we have already seen in Part I, §·1. The tendency to such imagery already exists in normal reactions, and merely becomes accentuated in difficult reactions owing to the retardation of understanding.

A more important and characteristic influence of an unfamiliar stimulus is its awakening of intermediary (or, preceding suggested) images which were 1, klang-associations, 2, paraphrases, 3, memory images, and 4, synonyms.

1. *Klang-associations.* With absolutely unfamiliar words there occurred quite frequently klang-associations. In the case of some stimuli, different observers had often the same associations: such as, *nosology*—*nose*, *mousquetaire*—*mosquito*, *pistology*—*pistol*, *hyle*—*hyla*, *cabala*—*cable*, *timbrel*—*timber*, *timbre*—*timber*, *synergism*—*syllogism*, *monad*—*Monadnock* (a mountain in N. H.), etc.

Examples. Nosology: (Obs. *L. M.* III—i, 5). "The pronunciation suggested 'noseology'. Then I found myself saying, 'nose-ology', which made me laugh. The mind was blank. I hadn't any effort or tension, but rather relaxation. I had a feeling of the amusing, comical."

Nosology. (Obs. *Ch.* III—i, 5.) "First feeling of total unfamiliarity. But this unfamiliarity was a little bit different from the first one because I recognized the first part of the word. I pronounced it two or three times. The word first suggested nose and made me think of a science of the nose, which I knew, of course, was not the correct meaning of the word."

Hyle (Obs. *L. M.* III—i, 1). "I got no reaction. I just found myself saying 'hyla', 'hyla'. There was a great deal of tension."

Hyle (Obs. *Sn.* III—i, 1). "I was attending to the movement of apparatus just before. Then as soon as the word appeared I pronounced it men-

¹ For examples, see Introduction.

² Part I, §§ 1 and 2.

³ Ribot states that he found practically the same conditions of imagery in both word and phrase reactions, and so dismissed the latter in his later experiments. *Cf. op. cit.*, p. 114 ff.

tally several times. My first thought was that it was connected with hyla, a tree-frog. And then I read it again and thought you gave it because of its philosophical meaning—the word is the same as the Greek ἕλη which would be spelt in the same way. And after that I repeated it several times and was repeating it. There was feeling of concentration particularly marked.”

With unfamiliar phrases, however, this form of association by similarity of mere sounds, perhaps something like parody, did not occur.

2. *Paraphrase.* With unfamiliar and apparently compound words, it was a common tendency to analyze them first into familiar elements and then to make out the meaning of the whole. For instance, *synergism*: syn-energy “working together”, *ultramontanism*—ultra-mont-ism= “doctrine beyond the mountain”; *pachydermata*: pachy-derm-ata= “a class of thick-skinned animals”; *Millenarism*: Miller-ism= “a doctrine of Miller,” etc.,

Example: *Millenarism.* (Obs. Ch. III, ii, 17, Time—(—¹)) “Read the word through half a dozen times and at the last time I divided the word into two syllables. Feeling of effort throughout the whole experiment; tension and the same feeling of hunting. The suggestion that came to me was a man named Miller who had a peculiar theological doctrine something about the end of the world at a certain time, I think. And so the feeling was not a feeling of total strangeness, but it was a feeling of recognition of the word. I suspected the word was probably constructed after the name of that man and stood for his system. Feeling of unpleasantness attached to the strain. Feeling of uncertainty and ignorance. The mind is not yet quite free from work. There seems something still working.”

Pistology. (Obs. Sn. III, ii, 7, Time—(—¹)) “Read the word in inner speech and tried to think of what it could possibly mean. I had that feeling of strain, of unfamiliarity, and then I began groping about. I looked at the first part of the word and recognized the word ‘pistol’ there, and then, I think, I formulated it in inner speech as ‘the science of pistol.’ And then I rejected that. At the same time there was some sense of humor. Then the words ‘science of fishes’ came perhaps by way of analogy with the word ‘piscatology’ (this word did not come into consciousness). But I realized that that is not the meaning of the word. And so I was still trying further and was thinking that I was not able to make out what the word was when the shutter was closed. A feeling of unfamiliarity, and a feeling of groping about for something were there, but the feeling of unfamiliarity was the only one which was in the centre of consciousness. There was also a certain feeling of helplessness though not clearly developed. These feelings of strain and effort passed off more gradually this time than in the other one. There was a little curiosity whether it was not a nonsense word.”

With unfamiliar and difficult phrases, this tendency to paraphrase was more frequent and common than with words. An important feature is the fact that here two kinds of intermediaries, a verbal and a visual, with rather definite individual

¹ The sign “—” signifies that the understanding did not take place within 5 seconds. For details see Introduction.

differences, were pretty clearly brought out. In the case of the verbal intermediary, the mind works mainly with synonyms, for the principal words in a phrase, or with inner speech in the making out of the meaning. There is seldom any trace of visual images of objects.

For instance: "*Truth seeks no corner.*" (Obs. Ms. ph, 4. Time—) "Read it in the same way as before. Turned back to the two words 'truth' and 'corner'. I had no visual image. From the word 'corner' the words 'square place' came by association. Interpreted 'Truth spreads itself.' There was a slight feeling of effort or tension. The mental operation stopped with the understanding."

"*A sin confessed is half forgiven.*" (Obs. Cf. I, Ph. 7, Time—.) "A little doubt still remains with this too. Some almost audible inner speech with distinct articulation and movements. A rapid comparison with the last one but no pronunciation of it. Everything was almost auditory, *i. e.*, words dealing with an imagined sin of a child who confesses to his father some fault he had done. That kind of thinking or imagination seems to bring forth the meaning, namely: 'If you tell him about your wrong yourself, it wakens the good disposition of the person you have offended'."

In the case of the visual intermediary, the mind works mainly with more or less vivid visual images of objects designated by the principal words of a phrase, or with visual imagination, in the making out of meaning.

For instance: "*Riches have wings.*" (Obs. Kn. I, Ph. 3, Time—.) "Internal reading. Then I had a very clear image of a bird with wings. Then the image of the flying away of the bird, which brought the idea, not image, of the going away of riches. Then the feeling of the conviction that the problem was correctly understood. This feeling was accompanied by a peculiar feeling of relaxation and ease."

"*Truth seeks no corner.*" (Obs. Hi. I, Ph. 4. Time—.) "Inner reading with movements of speech organs as before. I imagined and constructed a square in my mental vision. Then smoothing the four corners of the square I shaped it into a circle, and got the following interpretation. 'Truth is perfect.' (After having reported his introspection, the observer confessed that he began to doubt about his interpretation.)

With some few observers the use of one of these types of imagery was so constant and so firmly established that they seldom went over to the other form, regardless of the concreteness or abstractness of stimulus.

An extra observer had visual (or concrete) intermediary imagery nearly all the time as shown in the following protocol:

Obs, *Os.* Ph. No. 1. (*Union is strength*) 3" (stimulus spoken). "Saw white grasping hands, immediately followed by the recollection of the scene of the bridal ceremony in Longfellow's 'Launching of the Ship.' Then I had the sense."

Ph. No. 2. (*Use makes perfectness*) 3" (spoken). "I had a mental image of each word in the sentence. The style of letters appeared in the form between printed type and handwriting. Then followed the visual image of a vague shadowy human figure. Then the meaning.

Ph. No. 3. (*Riches have wings*) 4" (spoken). "Spoken sound remained. I had a mental image of hunting, and saw the white wing of a bird. Then the meaning flashed in.

Ph. No. 4. (*Truth seeks no corner*) 1" (spoken). I had an image of a corner. There was no repetition of the heard words. I payed very little attention to the words. The sense flashed.

Obs. *Cff.* on the other hand, had verbal imagery or inner speech in most occasions in the understanding of unfamiliar phrases.

Several of the other observers approached, in varying degrees, to one or the other of these extreme cases, while the rest represented the middle or neutral class, having no special preference or inclination to either sort of imagery. It was the imagery of this last class that was influenced markedly by concreteness or abstractness in the stimulus.

Here we have, therefore, in these images a pretty definite and also rather important criterion—important because it directly concerns the thinking—for individual differences. The general tendency seems to be that the frequency of verbal and visual intermediary images corresponds nearly to that of verbal and visual suggested images in cases of easy understanding.

3. *Memory images.* If the words or phrases were such as had been experienced once, or a few times, before, memory images often occurred in the form of the recollection of the circumstances under which the words had been experienced, regardless of individual differences and of the concreteness or abstractness of the stimulus words. This kind of memory images usually preceded the understanding, but sometimes succeeded or occurred simultaneously with it.

Example: *Synergism.* (Obs. *E. M.* iii—i, 10.) "First a visual impression of the word, then the pronunciation of the word. Then I saw vaguely the place in a book where the word was treated, but the meaning did not come to me. There was quite a noticeable feeling of strain on account of my hard effort to recall the subject of the treatment in which the word appeared."

Noumenon. (Obs. *Ky.* II, w—19.) "First the tendency to pronounce. Then the realization that it means the opposite of phenomenon came. It reminded me that I looked up that word in the dictionary about two months ago. I had a distinct visual image of the place; I had been looking at the dictionary in the library."

Thinking is so hard that many prefer judgment to it. (Obs. *Ac.* III—iv, 9, Time—3.0".) "I read it and the meaning flashed into my mind at once. But it was just a feeling. Then I had a very vague image of the lecture room and of Dr. S. I felt the statement to be easy and reacted. After I had reacted, in second thought I found it was not sure. The reason of the occurrence of this image is that Dr. S talked about the difficulty of thinking in common people who would rather decide without thinking. I did not recall the idea of it very clearly, whence my hesitation afterward. The tension was only kept up while I was reading and considering. With the reaction it went away and some sense of satisfaction came with it. But it was soon dispelled by the sense of uncertainty and its accompanying feelings which persisted as in other cases of difficult reactions.

The frequency of such memory imagery, with not quite familiar words or phrases, is well known to every one of us especially in the study of a new language. With frequent repetitions these definite associations fall away giving place to mere feeling of recognition or of familiarity.

4. *Synonyms.* A. *With words.* With rather, but not quite, familiar words, there occurred sometimes other more familiar verbal images having similar meanings, or synonyms in widest sense. They were synonyms proper, definitions, and translations.

a, Synonyms proper, or words having similar meanings.

Apperception. (Obs. *St.* II, w—18.) "There was a slight surprise. The word was familiar. It brought the word 'attention'. Then I thought of Dr. S explaining the meaning of the word as mental grasp of the whole. I visualized Dr. S in his recitation room."

b, Definitions: This form occurred especially with technical terms, such as, apperception, parallelism, noumenon, etc.

These forms occurred mainly in observers belonging to the verbal type. The observers belonging to visual or concrete type visualized, in such cases, a concrete instance mostly in the form of memory imagery.

For instance: *Parallelism.* (Obs. *Ac.* II, w—32.) "I spelled and pronounced the word mentally. I saw mentally Dr. S drawing on the black-board the diagram on parallelism, and speaking of the theory, of the statement of the relation between body and mind."

c, Translation. With foreign words this form frequently occurred regardless of individual differences.

Color. (Obs. *Kn.* I, w—10.) "Sound continued. Translated into Japanese 'iro' which was internally spoken. Then the understanding, and I thought of the red color of this card-box on the table."

B. *With phrases.* With unfamiliar phrases there occurred similar forms of synonyms nearly in the same way as in the case of words.

a, Similar phrases.

Example. *A chariot will not go on a single wheel.* (Obs. *Cff.* I, B—I, Time—.) "I tried to recognize the phrase, but failed. Then I recalled a similar expression, 'A college without a library is like a wagon with three wheels.' The situation in which I had heard the proverb came into my mind. I had a slight vague image of a chariot as described in a book. Then I tried to compare two wheels to two qualities in a person's nature, which balance each other. The word 'balance' was internally spoken. I decided that the only meaning I can get out of it was that 'Balance is necessary for success.' The sentence was internally spoken. Feeling of dissatisfaction with reference to my explanation. A feeling that something is wrong with my interpretation."

The translation of foreign phrases was quite common.

For instance: *Laborare est orare.* (III—ii, 6. Obs. *Ch.* Time, 1.9") "First read it through and understood it without translating it. The under-

standing was not complete, so I read it second time translating into English and I got the sense. I experienced two sorts of certainties, the certainty about my knowledge of the Latin words, that is about what they mean in English, and that of the sense. I was at the beginning a little surprised to find a Latin phrase."

To conclude: Unfamiliarity with the stimulus has an important influence on the frequency and kinds of images. 1. Unfamiliar stimuli accentuate the images of the stimuli in general. 2. Intermediary images in the forms of klang-associations, memory images, synonyms, translations, definitions, etc., are direct results of unfamiliarity. 3. In the paraphrase, there is a marked tendency to individual differences in the use of verbal and visual intermediary images. 4. The kinds and conditions of intermediary images both with unfamiliar words and phrases seem, in the main, nearly the same.¹

§ 7. AUSFRAGE METHOD AND THE CUSTOMARY METHOD IN STUDIES OF ASSOCIATION

The results of our study lead to the belief that the Ausfrage method, such as was applied in the above study, is more adapted, in its passive form of reaction, to the study of the actual phenomena of association than the customary method of the so-called association-experiment, because it has the following advantages over the latter: 1, Naturalness of associations; 2, Clearness of the term suggestion; 3, Introspection; 4, Change of conditions. The customary method lacks almost all these conditions essential to the study.

1. *Naturalness of associations.* The observer has only to remain passive to the stimulus letting the associations or suggestions go as they occur without interruption or disturbance by will. With the customary method, on the contrary, the course of associations becomes complicated or jeopardized in a double way: 1, By the observer's mere intention or will to fulfill the artificial requirements of the experiment, and 2, by the actual fulfillment, of them. *a*, The reaction must be given in one word; *b*, The reaction-word must be different from the stimulus-word; *c*, The reaction-word must stand for the first association; *d*, The reaction must be as quick as possible. It is clear that the mere idea of fulfilling these numerous requirements is itself sufficient to change the observer's attitude from a passive or neutral state to an active or selective one. As to the results of actual fulfillment of requirement *a*, it may be asked how, in case the observer has as an associated idea, a complex visual image or memory

¹For the conditions of concomitant feelings see Part III, § 3.

image, he could express them in a single word without selecting at random the name of one of the represented objects or a part of one or some idea such as could be promptly expressed. Of requirement *b*, it may be said that the association existing between an image and its name (as well as that between a perception of an object and its name) is one of the strongest; and the two operate reciprocally; a name usually calls up its object-image and the object-image usually calls up its name. When a name calls forth its object-image in the observer's mind, the natural tendency is to name it again. But this tendency must be checked, for a reaction by repeating the stimulus word is forbidden, though, in such cases, it is psychologically quite different from mere mechanical repetition or imitation of speaker's voice. The result of the fulfillment of the requirements *c* and *d*, *i. e.*, of "the first association" and of "promptness," is that the majority of reactions will necessarily consist of independent verbal associations or what Wundt calls articulatory or pseudo-associations (*Scheinassoziationen*), since they are in general the first to arrive, but are deprived of all psychical traits.

2. *Clearness of the term "Suggestion."* Of whatever kinds they may be, all conscious events are "suggestions" when they succeed the perception of the stimulus-word. Their classification and sorting and sifting are all left in the Ausfrage experiment to the experimenter. The observer has only to state all he has experienced in a certain interval of time, and there is no room for ambiguity in the meaning of terms as such. The term "associations," as it is used in the 'instruction' in the common association experiment, is, on the contrary, ambiguous and capable of more than one interpretation. It may mean purely articulatory associations. It may mean "real" associations *i. e.*, those with object-images. It may mean associations between two object-images, as in the case of so-called "association of ideas." Finally, it may mean the mixture of all these, the most natural to occur in real associations. Not only does each observer differ in these varieties of possible interpretations, but also one and the same observer may fluctuate, sometimes voluntarily sometimes involuntarily, from one to the other of these interpretations according to circumstances.¹

¹Sometime ago, to see how far the results would differ in the same observers according to the differences of these interpretations, I made tests with several observers, in the first series of which the observers were requested to react under the conditions of the traditional association-experiment. In the second series, they were required to react, if possible, after having some suggested object-images or ideas. The protocols, which were taken by the observers after each reaction, show the result that in the first series

3. *Introspection.* In such complex processes as word-reaction, nothing gives a more direct and trustworthy account than the introspection of the observer himself. Objectively alone (*i. e.* without this essential help of introspection), it is almost impossible even to distinguish an immediate, or articulatory reaction from an intermediate, or object-reaction, not to mention the tracing of the same reaction-words to different sources, or of any further exploitation of associations in general.

4. *Change of Conditions.* The occurrence of a special association or suggestion to the exclusion of others is directly conditioned by the resultants of the three factors: Procedure of experimentation, Kinds of material, and Individual differences. Change of conditions reveals to us the real causes of certain forms of associations, which is not only ignored by, but also will be difficult for, the customary method because of the complication of many other factors such as were mentioned in item 1.

So much for the comparison of the new and the customary method in their application to the study of association. The disadvantages of the latter are clear.

On Association Experiments in Applied Psychology

No one can deny that some important contributions to psychology as well as to practical life have come from association experiments in applied psychology. Their method, however, is not absolutely free from weaknesses, as it is based on the same principles as the preceding.

Its study of individual differences of normal and abnormal mentality consists of two processes: 1, The collection of a number of reaction-words by the customary association method; and 2, the interpretation of the reaction-words. This is undertaken as follows: First, such a logical scheme or classification of associations (according to the conceptual relations of the reaction-words to the stimulus-word) is prepared, as will comprehend all possible forms of reactions under these categories. Then the relative frequency with normal subjects of associations of the different sorts thus classified is taken and serves as a standard with which to compare the frequency with abnormal subjects. Now, regarding some of the interpretations or generalizations attained through

the reactions consisted of diverse kinds of imagery, with the majority of articulatory associations (purely verbal). In the second series, the majority of the reactions consisted of memory associations. This illustrates how easy it is to get totally different kinds of associations from the same observer by the mere difference in the interpretation of the term "association."

such steps the following remarks may be made. In the generalization that children, imbeciles, idiots, epileptics, etc., react frequently in phrase form, whereas with adults and normal persons this form occurs very seldom if at all, the probable interpretation is that the former do not perhaps understand, or forget, or neglect the requirement for the use of a single reaction-word, whereas the latter observe the rule. This fact of disobedience, etc., itself may sometimes be regarded as a sign of abnormality, etc., but nothing more, because phrase associations are quite natural and frequent under certain conditions, as is shown in our study. In like manner, the generalization that children and imbeciles, etc., react frequently by egocentric associations, whereas normal persons react very seldom, if at all, in this form, cannot be interpreted as an expression of a fair comparison of the actual associations, because a single word by itself seldom furnishes a clue of egocentricity to an onlooking psychologist, unless it is accompanied by a personal pronoun or some other word, that is, unless it is put into a phrase form which a normal person suppresses. This fact perhaps accounts for the exceedingly small number of egocentric or personal associations with normal persons which is reported in these studies. In some cases its frequency goes as low as the ratio of once in two thousand reactions.¹ In fact with passive reactions in our experiment and according to the direct report of the observers, this form, which is merely our memory imagery,² occurred in the ratio of once in every two reactions. A large discrepancy indeed!

Reactions by repetition of the stimulus word, which are regarded in association studies, as characterizing the reactions of children and imbeciles, etc., were found as common and natural associations with normal subjects in our experiment, as was just stated. Reactions in the form of explanation or definition, another characteristic of children, imbeciles, etc., were often found in our experiments, especially when the stimulus word was not quite familiar or it was a technical term of the meaning of which the observer was not perfectly sure. So that this tendency to explanatory reaction can hardly be regarded as more than an indication of the degree of acquaintanship with words, *i. e.*, literacy or illiteracy.

To sum up, these so-called characteristic forms in children and the abnormal can all be found in normal adults in their natural associations, *i. e.*, when they react according to natural

¹Jung und Riklin: Diagnostische Assoziationsstudien. Leipzig, 1906. Cf. p. 108.

²"Die Einstellung ist eine egozentrische, in sofern das Reizwort vorzüglich subjective Erinnerungen anregt." *Ibid.*, p. 117.

and spontaneous suggestions, as was the case with our experiment, and do not react according to artificial and "sophisticated" associations, *i. e.*, by mere verbal associations, as is the case in the customary experiment with normal observers who are expert enough to obey the "rules."

Remarks on association experiments as a means of diagnosing crimes as well as diseases. The criteria for a so-called significant or critical reaction are: 1, Prolonged reaction time, considered as due to the disturbances of the "emotional complexes"; 2, Apparently unconnected reaction words (having no conceptual connection with stimulus) considered as a "Deck" or "evasive" reaction, when the experimenter is unable to account for it, and as a suspicious reaction when he is able to do so. Sometimes a succeeding reaction is examined according to these criteria, on the ground of the phenomenon of "Perseveration." The final or crucial test is furnished by the confession of the (supposed) criminal or patient.

Now, most of our cases of dependent verbal imagery accompanying memory imagery, if they alone were announced by the observer (*i. e.*, memory associations), no matter whether they are emotional or neutral, significant or insignificant, would satisfy the two conditions just mentioned, for they were slower than the simple articulatory associations and lacked, as a rule, *logical or outward connection to stimulus words.*¹

These criteria are thus helpless in the distinguishing of emotional or significant memory associations from neutral or insignificant memory associations. They are effective only when the observers always react in the form of articulatory or purely verbal associations with insignificant words and in the form of memory associations with significant words. This may naturally occur in laboratory tests, as the observers are trained to articulatory reactions with ordinary words, and are naturally struck by recent memory associations of a few minutes date which, of course, are the strongest and most likely to revive, no matter how insignificant the events were. But

¹Experiments were made recently by Yerkes and Berry (*Am. J. of Psychol.*, Jan., 1909) and also by Henke and Eddy (*Psy. Rev.*, Nov., 1909) to test the certainty of the diagnostic method in the discovery of certain acts executed by the observers shortly previous to the tests, with the same results in both studies, namely: that the method was certain when it had to do with the determination of two alternatives, even if the observers sometimes tried to "fool" the experimenters. In these cases, it must be remembered, we are not dealing with the two forms of reactions, *emotional* and *neutral*, as is commonly presumed, but, in fact, with only the two forms of reactions, *articulatory* associations and *memory* associations (in these cases, with very recent memory associations), as also is plainly seen in the reading of the tables and introspections in these articles.

such is hardly to be expected in the case of patients and criminals in actual practice, because they may be expected to react very frequently in the form of real or memory associations to ordinary words.

PART III

ANALYSIS OF THE SIMULTANEOUS CONCOMITANTS

The foregoing studies have dealt with the conditions of the frequency of images which either preceded or succeeded understanding. In this last part of the study I shall examine the nature of "meaning" as a simultaneous concomitant of understanding, tracing up the following three questions. 1. Whether the concomitants precede or succeed or occur simultaneously with understanding. The preceding and succeeding concomitants may be eliminated from the experience of understanding itself, whatever relations they may have to the latter. 2. Whether or not the simultaneous concomitants are peculiar to understanding or meaning. Those which occur as fully even when there is no understanding may be eliminated from the characteristic constituents of meaning, no matter whether they are constant or not. 3. Whether or not the constituents of meaning can ultimately be reduced to the psychological elements, *i. e.*, pure sensations and feelings. The protocols of the foregoing experiments directly answer the purpose of the first question, *i. e.*, the temporal succession of the concomitants. For the second question an additional experiment was made. The answer to the third question consists mainly of inferences from all the preceding results of introspections. But before entering on the discussion of our results let us take a glance at the main views on the psychology of thought and especially of meaning held by the modern psychologists to whose work we shall have frequent occasion to refer.

§ I. AN HISTORICAL SKETCH OF THE VARIOUS VIEWS OF THOUGHT

1. *Thought as identical with concrete representations (images).* Locke speaks of the possibility of our having pure general ideas free from any particular representations, for instance, of "a triangle which must be neither oblique nor rectangle, neither equilateral, equicrural, nor scalenon, but all and nony of these at once."¹ Berkeley who was quite surprised by this conceptualistic view of Locke, says, "the idea of man that I frame to myself must be either of a white, or a black, or a tawny, a straight, or a crooked, a tall, or a low, or a

¹Essay, Bk. iv. ch. 7.

middle-sized man.”¹ For him meaning, concept or general ideas, as such, have no psychical existence except in concrete representations.

Among modern psychologists James, siding with Locke in opposition to Berkeley, says, “The note so bravely struck by Berkeley could not, however, be well sustained in the face of the fact patent to every human being that we can mean color without meaning any particular color, and stature without meaning any particular height.”²

Binet in regard to Berkeley’s proposition takes, like James, the negative side and refers to two cases as an *experimentum crucis* against it, *i. e.*, cases where we have particular and precise images without having any meaning or thought, and cases where we have thought or meaning without having any particular or precise images. He maintains, also, that ‘*pensée générale*’ can be properly explained by neither conceptualism nor nominalism though probably by “intentionalism” which he himself proposes.³ Bühler speaks of both being right and wrong, that Berkeley is right in his negation of sensuous representation of a general triangle, and Locke is right in his assertion of the existence of the pure meaning of a triangle without any sensory element, and also that the question of general ideas is totally different from that of abstract ideas or (his) thought or knowledge (*Wissen*).⁴

2. *Thought as identical with the verbal image.* This question first took its definite shape in the controversy between Max Müller on one side and Galton, Romanes and others on the other. The former maintains that all thinking when introspectively viewed is merely inner speech. The latter refer, to cases of chess playing, of the construction of machines in purely visual terms, and also to the framing or searching for the words for an existent thought, a fact quite common to us, as insurmountable difficulties in the way of this proposition.⁵

Identity of abstract thought and verbal image. Taine speaks of his abstract ideas as quite different from a particular representation or even from the “confused and floating representation of particular *araucaria*,” alluding to the general ideas of Galton and Huxley.⁶ He says, “We think the abstract character of things by means of abstract names which are our abstract ideas, and the formation of our ideas are

¹Principle, Introduction, 10, 13.

²*Ibid.*, Vol. I, p. 470.

³Binet A.: *L'étude expérimentale de l'intelligence*, 1903, p. 151 ff.

⁴*Ibid.*, p. 363-364.

⁵Max Müller: *ibid.*, appendix (1887).

⁶*L'intelligence*, II, 139.

merely the formation of names which substitute them."¹ Among present authors, Wundt says, "We do not always think in words; we can easily recall actually experienced or dreamed events in mere visual terms. But with abstract ideas, we usually think in words often involuntarily accompanied by the visual image of the words."² Decidedly opposing this theory, James says, "The opinion as stoutly professed by many that language is essential to thought seems to have this much of truth in it, that all our inward images tend invincibly to attach themselves to something sensible, so as to gain in corporeity and life. Words serve this purpose, gestures serve it, stones, straws, chalk-marks, any thing will do . . . 'The bricks are alive to tell the tale'."³ Bühler refers to the fact that the verbal image occurs in thinking only in a sporadic way and is broken and fragmentary without running parallel with thought processes.⁴ Ribot discards the theory simply as "inacceptable."⁵ The other authors of the school of Würzburg, *e. g.*, Marbe, Orth, Ach, Watt and Messer, seem to agree in the rejection of this theory. On the other hand, recently Dearborn has attributed the utmost importance to verbal images even in the comparison of ink-blots by the eye. He reports that he found in his experiment the presence of verbal images, in cases where the judgments were correct, in all of the numerous observers, except one, who had just a "true feeling" of likeness and unlikeness. This observer was, nevertheless, the most successful of all in the judgments.⁶

3. *Thought as identical with the compounds of the three dimensional feelings.* Wundt calls those "intellectual feelings" which attend complex intellectual processes. "They are in general complex total feelings, into which simple feelings and ideational feelings (*Vorstellungsgefühle*) enter as components."⁷ "The feeling of doubt is an oscillating emotional state (*Gemützustand*)."⁸ The "feeling of agreement (which is a kind of *Vorstellungsgefühl*) is introspectively merely a feeling of relaxation with heightened intensity."⁹ "The feeling of recognition (*Wiedererkennungsgefühl*) is a

¹*Ib.*, I, 1st ed., 254.

²Grundzüge der Physiologischen Psychologie, 1902, 5th ed., Vol. III, p. 543.

³James, W.: Principles of Psychology, Vol. II, p. 305.

⁴*Ib.*, 317.

⁵"*Idées Générales*," p. 100.

⁶G. V. N. Dearborn: Experiment on the Judgment of Likeness and Unlikeness of Visual Form, *Journal of Philosophy, Psychology and Scientific Method*, Feb., 1910, p. 60.

⁷*Ib.*, Vol. III, p. 264.

⁸*Ib.*, 265.

⁹*Ib.*, 510.

subjective symptom belonging not to the ideational processes but to the subjective side of the processes,"—the processes of assimilation of ideas.¹ Its subjective quality "seems a sudden and unhindered change between tension and relaxation, which under circumstances can be joined by other feeling qualities."²

Most of the Würzburg authors discredit this Wundtian view of intellectual or cognitive feelings. Orth in his analysis of "*Bewusstseinslage*" (another name proposed by Marbe for intellectual feelings) as introspectively observed by himself and others while serving as observers in Marbe's study on judgment, finds them related to cognition and therefore implicitly to sensation rather than to feeling, and having reference to the object rather than to the subject.³ Doubt, according to him is, introspectively, thoroughly different from sensations, representations, and feelings proper; and the same with feelings of certainty, contrast, agreement, etc.⁴ "What *Bewusstseinslagen* really are," he says in summarizing, "remains to be investigated. So much seems to be certain, that they resisted our analysis and that they are not at all merely another name for the psychical facts that Wundt comprehends under his two new feeling directions, for this contradicts, not only self-observation, but also their great manifoldness."⁵

4. *Thought as judgment and as knowledge is beyond psychical experience.* Marbe in his study on judgment arrived at the conclusion that "there are no psychical conditions of judgment in general that give them the character of judgment as such."⁶ And the same with understanding of judgment.⁷ Accounting for this, he says, "thus we see very easily that understanding of judgment can never be found in consciousness, because it rests upon knowledge, and knowledge is never given in consciousness."⁸ As the processes of judgment are totally beyond consciousness, so in its study there is as little left for psychologists as of physiological chemistry for chemists.⁹ Later authors of the Würzburg school agree in criticising Marbe's reflexive theory of judgment as due to too easy stimuli. Bühler simply speaks of his "thought" as actual *Wissen* and not such potential *Wissen* as Marbe means.¹⁰ James speaks of his feeling of tendency (such as feeling of familiarity, recognition, etc.) as not a "psychical zero," but a "psychical fact" though vague and difficult to name.¹¹

¹*Ib.*, 536.

²*Ib.*, 537.

³Orth, J.: *Gefühl und Bewusstseinslage*. Berlin, 1903, p. 73.

⁴*Ib.*, 71. ⁶*Op. cit.*, p. 42, 43. ⁸*Ib.*, p. 92. ¹⁰*Op. cit.*, p. 361.

⁵*Ib.*, 128. ⁷*Ib.*, p. 83. ⁹*Ib.*, p. 96. ¹¹*Ib.*, Vol. 1, p. 254.

5. *Thought as identical with reproductive tendencies.* Ach calls imageless thought or pure cognition free from any "phenomenological constituents, such as visual, acoustic, kinesthetic sensations or images," "*Bewusstheit*."¹ He says, "When a word, for instance, 'bell' is presented to me and I apperceive the symbol, I understand what it means. I have the *Bewusstheit* of meaning. According to the theory of *Bewusstheit*, it is not necessary for understanding that one have representations . . . such as auditory or visual images of a bell . . . Each representation which is given in consciousness, for instance, the impression of the stimulus word 'bell,' puts, as is well known, a number of associated representations into the state of readiness. This putting-into-readiness of representations, or excitation of reproductive tendencies, suffices for the conscious experience (representation) of what we call sense or meaning."² In criticism of this view, Watt, speaking of meaning as different from the vague, reverberating associations or tendencies, says: "Some maintain that this is a mass of vague associations, word-associations or others, but this is not clear according to the protocol. It rather points to the fact that a concept, such as appear in free self-observation, is something different from vague reverberating associations or a certain number of them."³ Bühler excluding the mere consciousness of tendencies from his 'thought' or 'meaning,' says: "Thought is nothing vague or half-conscious but something clear, and not a sum but a unity."⁴ Titchener speaks of the necessity, in the awareness of meaning, of the co-operation of the both Ach's awareness of reproductive tendencies or 'meaning' and his awareness of relation.⁵

6. *Thought as identical with "fringe"-experiences.* According to James, thought as well as meaning is the feeling of relation which is the felt "glow," "fringe," "echo," or "reverberation" and the transitive experience of mind in distinction to substantive or static experience, *e. g.*, images, sensations, etc. He says, "The meaning of the words which we think we understand as we read, is a sign of direction, . . . or, a bare image of logical movement which is a psychic transition, always on the wing, so to speak, and not to be

¹Ach, N.: Ueber die Willenstätigkeit und das Denken. Göttingen, 1905. p. 210.

²*Ib.*, 216-217.

³Watt, J.: Experimentelle Beiträge zur einer Theorie des Denkens, *Archiv f. d. gesamte Psychologie*, 1905 (4), 289 ff., p. 434.

⁴*Ibid.*, p. 326.

⁵Titchener, E. B.: Lectures on the Experimental Psychology of the Thought-processes, N. Y., 1909, p. 107.

glimpsed except in flight."¹ Further characterizing meaning as feeling of tendencies, he says, "The sense of our meaning is an entirely peculiar element of thought . . . It is one of those evanescent and transitive facts of mind which introspection cannot turn round upon It pertains to the fringe of the subjective state, and is a feeling of tendency, whose neural counterpart is undoubtedly a lot of dawning and dying processes too faint and complex to be traced."² He further characterizes this feeling of tendency as a tendency of a "nascent image,"³ as a feeling antecedent to recall, such as a "ringing in the ear," or "dancing in one's mind" of a forgotten name or word, or of the rhythm of a verse, as the feeling of recognition or familiarity which is a "submaximal excitement of wide-spreading association brain-tracts."⁴

Thus we see Ach's conception of 'reproductive tendencies' is quite similar to the "fringe" experiences of James. Ribot's view seems also to approach these conceptions when he considers meaning or concept as an "unconscious substratum, organized and potential knowledge, harmonics which give detonation to the word."⁵ Hoernle referring to James, says that James reverses the fact of ordinary experiences where "we notice more of meaning than words. Meaning stands in the foreground and images or ideas or sensory elements in the background of consciousness."⁶

7. *Thought as well as meaning as a "transcending" experience.* According to Messer, sensations and sensation-complexes are perceived merely as contents of consciousness; "they exist or do not exist, but do not point beyond themselves; they do not mean." Thought as also perception, etc., on the contrary, possesses a characteristic attribute of 'transcendence.' "No thought thinks upon itself, *i. e.*, on the constituents of consciousness which we can examine in direct retrospection." "He who, thinks," he continues, "that he could sufficiently characterize thought and perception simply by looking at the existent sensations and images, is like one who believes he could find the value of money by merely examining its material."⁷ Further, in regard to the experience of understanding or meaning, he distinguishes two forms attending the reaction to the same stimulus word: 1, general understanding which is further unanalyzable; and 2, more conscious and definite understanding which, he says, sometimes is "not conditioned by the *Aufgabe* [problem or instruction], but is such as would be explained by the predominance

¹*Ib.*, Vol. I, p. 253.

²*Ib.*, Vol. I, p. 472.

³*Ib.*, Vol. I, p. 254.

⁴*Ib.*, p. 258.

⁵*Ib.*, p. 132.

⁶*Mind*, Jan., 1907.

⁷*Ib.*, 113.

of the reproductive tendencies in the general constellation."¹ But "the reaction wherein the *Aufgabe* enters and the acts of acceptance and rejection take place, apparently cannot be explained by the mechanism of mere reproduction and association, and herein lies the justification for distinguishing the processes of thought from those of pure associative reproduction."²

8. *Thought as a third psychical element.* Bühler, as the result of his study on thought, came to the conclusion that thought-experiences are neither analyzable to sensations nor feelings but so unique and specific that they should be considered as compounds of a third psychical unit or element, *i. e.*, *Gedanke*.³ Thought, according to him, is act of knowing (*Wissensact*).⁴ Meaning, which is a conscious knowing, cannot be represented, but only known.⁵ He says that to ask one to explain knowing or thought merely by the terms of the quality and intensity of existent sensations is the same as asking one to explain depth by the terms of height and width.⁶

9. *Thought as indescribable.* All the preceding authors who regard thought as something different from image or feeling proper, agree in finding it as further indescribable. Marbe, in regard to his *Bewusstseinslage* speaks of conscious facts whose contents either totally escape from further characterization or are difficult to approach. Orth says, "Those *Bewusstseinslagen* which were observed by Marbe and by us are of diverse kinds and have only this point in common that they represent psychical facts which could not be further analyzed by us."⁷ Ach says, the description of these *Bewusstheiten* by the observers is very difficult because of the difficulty of verbal expression, as they lack "phenomenological representations" (sensations and images, etc.).⁸ Watt, toward the end of his study says, "An analytical introspection in this direction is exceedingly difficult . . . We know psychologically as much as nothing about the nature of meaning-consciousnesses which accompany an abstract word."⁹ Messer speaks of thought-experiences as further unanalyzable. James speaks repeatedly of the difficulty of description of fringe-experiences; their multitudinous nuances or configurations can be only felt.

10. *Thought, also meaning experiences, as identical with kinesthetic images.* Taking a quite different view from the preceding authors, Titchener, in his recent book on thought-processes, declares that all these authors or their observers who find thought-experiences something different from the

¹*Ib.*, 82.⁴*Ib.*, p. 361.⁷*Ib.*, p. 70.²*Ib.*, p. 122.⁵*Ib.*, p. 363.⁸*Ib.*, p. 41.³*Ib.*, p. 329.⁶*Ib.*, p. 361.⁹*Ib.*, p. 435.

existent elements, *e. g.*, sensations, images, etc., are victims of stimulus-error; that they do not separate or abstract what they infer from what they actually experience or have, but think them together, just as an observer in color-experiments, instead of reacting to an abstracted red sensation, reacts to the red paper, an object, the result of both sensations and inferences; and that the results of their introspections are mere intimations or indications, and not the descriptions of what they have experienced.¹ Referring to James's feeling of relation, he says "the phrase 'feeling of relation' is no more unequivocal, as a psychological term, than the phrase 'idea of object' or 'consciousness of meaning.' It carries an intimation, an indication, a statement-about; it does not describe. And the question for psychology is precisely that: what we experience when we have a feeling of relation?"² The consciousnesses of thought when described are merely kinesthetic images or sensations. All other consciousnesses are not the direct data of introspection but the results of addition by reflection and inference.³ "And all such 'feelings,' he says, —feelings of *if*, and *why*, and *nevertheless*, and *therefore*, normally take the form, in my experience, of motor empathy. I act the feeling out, though as a rule in imaginal and not in sensational terms."⁴ Regarding the distinction between kinesthetic images and sensations, he says, "Actual movement always brings into play more muscles than are necessary, while ideal movement is confined to the precise group of muscles concerned." "The sensed or actual nod (that signifies assent to an argument, and frown (that signifies perplexity) are coarse and rough in outlines; the imagined or mental nod and frown are clean and delicately traced."⁵ He wonders why James does not take the same introspective view of his 'feeling of relation' as he does with the feeling of a 'central active self' in which he (James) finds nothing but 'bodily processes for the most part taking place within the head.'⁶

§2. CONTENTS OF MEANING WITH FAMILIAR STIMULI

What are the actual contents of consciousness at the instant of understanding, the direct psychical experiences which constitute the experience of understanding familiar and easy words or phrases? The protocols of all the foregoing experiments show that there was not one kind of such content only but several. Arranged according to the general order of succession or quickness, they were: 1, Feeling of familiarity or recogni-

¹*Ib.*, p. 145.

²*Ib.*, p. 185.

³*Ib.*, p. 185.

⁴*Ib.*, p. 186, 187.

⁵*Ib.*, p. 20-22.

⁶*Ib.*, p. 30.

tion of the stimulus; 2, Feeling of concept; 3, Feeling of content; 4, Feeling of direction; 5 Half-developed or indefinite images; and lastly 6, Fully developed or definite images.

1. *Pure Feeling of Familiarity.* With a very familiar word or phrase, or in repeated reaction to a stimulus previously understood, the occurrence of just the feeling of familiarity alone or the recognition of the stimulus as the one understood before, was sufficient to release the reaction or cause the stimulus to be felt as understood.

For instance: *Mountain.* (Obs. Sn. III—ii, Time—0.9'') "I read it in inner speech and reacted as the word seemed familiar to me, *i. e.*, when the feeling of familiarity came. I did n't get any further meaning. No imagery, no associations until after I reacted."

Psychology. (Obs. Ch. III—iii—15, Time—0.5'') "The first feeling of recognition of the form of the word was followed instantly by the feeling of familiarity. There was no imagery. In this particular case it appears that the feeling of recognition and the feeling of familiarity are the same thing. There wasn't any imagery, any attempt to define. I recognized the word. Absolute certainty as to understanding."

This type of meaning was quite common and frequent with all observers and regardless of the concreteness or abstractness of the stimulus word. It was the first and quickest to occur of all the types of meaning. It seldom occurred, however, with the mere visual perception of the stimulus word or phrase. Audito-motor reading of the stimulus was necessary to release the reaction even in this type. In active reactions and with familiar abstract stimuli, it was seldom followed by any suggested images in any observers. In active reactions with familiar concrete stimuli, it was sometimes followed, especially with observers of concrete (or visual) type, by suggested images which, however, always occurred after the reaction. And such was the case with all the passive reactions where the suggested images made the terminus of the reaction occurring long after the entrance of the feeling of familiarity or recognition.

2. *Pure feeling of Concept or Meaning.* In the preceding case both sensory and conceptual familiarity or recognition fused so closely together that there is difficulty of analysis, though sensory familiarity evidently predominates in such reflexive reactions. With less familiar stimuli, or when the observers waited longer with very familiar stimuli, these two generally became separated and occurred in succession, the sensory recognition always preceding the conceptual.

Example: *Pomology.* (Obs. Ac. II, w—10.) "Recognized the word and thought it familiar, but on closer examination, I found that I could not understand its meaning."

Heaven, (Obs. Cf. II, w—5.) Did not apperceive the word for a minute. I was not quite attentive. Then I got a purely verbal meaning of it without imagery. By verbal meaning I mean I first recognized it. Then I

had a slight feeling. It was not at all a sort of tridimensional feeling, but an idea of something sacred. But I call it a feeling because it was not definite."

In its frequency and conditions of occurrence, this type of meaning was practically the same as the preceding one except that the latter always preceded the former in case of concurrence.

3. *Pure Feeling of Content.* To this belong the experiences which the observers expressed as "full of meaning," or "content," having "rich associations," or "coming associations," etc., which were accompanied by no particular images or associations. The chief marks were the richness and poorness of content. A feeling of rich content was generally found with stimuli designating topics which observers were interested in or familiar with, and a feeling of poor content with the stimuli indicating uninteresting or unfamiliar subjects.

Example: *Peace.* (Obs. Sn. III,—iii—5. Time—1.0'') "I read it in inner speech. But the inner speech was not very clear this time. And I think, I had a feeling of the meaning of the word. Whether it was different from the feeling of familiarity I am not sure. But it seems to have been something more than a mere feeling of familiarity. This something may be some feeling of moving toward something, or of some possibility of development and is very hard to describe."

House. (Obs. Cf. I, w—3.) "Perception of my voice [inner speech]. Then came the feeling of familiarity. That feeling seems to me to be composed of various kinds of images not yet actual. I would call it almost composite. If I should think about it longer I would have some particular images out of it."

4. *Feeling of Direction.* This feeling is the experience of the mind's pointing to or turning in the direction of the place where a particular object or event referred to by the stimulus was experienced. It is an incipient form of object imagery.

Pedagogy. (Obs. Sn. III—i—14.) "I read the word in inner speech but not very loud and was not quite sure whether I read it correctly. So I read it again and had a faint feeling that I knew the word. Then I thought of the direction of the Dr. B's room and probably also had a very vague suggestion of Dr. B himself. The consciousness of direction was very clear. I had the word 'teaching,' probably in inner speech."

Example: *Head.* (Obs. Cf. I.) "Always an after effect of sound. I listened for the after effect before the recognition of the meaning. Then came the feeling of familiarity followed by a vague idea, almost a feeling of location of upward, top of human head, idea of something above. I had no definite image."

This experience occurred frequently, especially among observers belonging to a rather non-visual type. In passive reactions, and especially with observers belonging rather to the visual or concrete type this experience was, in general, replaced by rather fully-developed object images.

5. *Half-developed Images.* These were faint and vague representations of objects or circumstances, which, on account

of faintness or indefiniteness of imagery, were sometimes termed by the observers "ideas," something "thought of," etc. This was especially the case with a quick recapitulation of many particular past experiences.

Example: *Memory*, (Obs. *L. M.* III—i, 4.) "First a visual impression of the word. Then reading in inner speech. And then I had a bird's-eye view of all that I have been working at for several weeks; I have that subject. Some pleasure and satisfaction in seeing the word. This word was very full of meaning, but there were no particular visual images. Its meaning could not be expressed in so short a time."

Philosophy. (Obs. *Ac.* II, w—65.) "I thought of Plato, Aristotle, Kant and Hegel in connection with their productions. No definite visual or auditory elements.

6. *Fully-developed Images*. These are suggested object-images as well as verbal images such as have already been described (Part II, above). They were in general the slowest to occur. They were generally found in passive (or prolonged) reactions and seldom in active (or quick) reactions to familiar stimuli. In case of concurrence with the preceding experiences, they were the last to occur, that is, they made the terminus of the reaction.

Conclusions

1. These results negate the theory of the identity of thought with concrete representations and also the theory of the identity of thought with verbal images so far as meaning experiences are concerned, as these images are a part only of our fully-developed images,—one of the six types of meaning.

2. A chief condition determining whether or not one shall have a definite image (visual or verbal or other), in understanding familiar words or phrases, is the length of time the process continues. If one reacts quickly, *i. e.*, at the stage of familiarity, or concept, etc., one will not have, as a rule, any definite images, regardless of individual differences, and of the concreteness or abstractness of the stimulus. If one dwells longer upon the stimulus one will usually have some particular representations, the majority of which will be recent memorial associations, in predominantly visual form, in the case of visualizers, and in predominantly verbal form in the case of verbalists, regardless of the concreteness or abstractness of the stimulus.

Remark: Ach suggests that pure meaning appears most prominently in the quick reading of a text (*op. cit.*, p. 261). James speaks of the two kinds of meaning, *i. e.*, dynamic meaning which attends the understanding of a phrase and is "usually reduced to the bare fringe," and static meaning which attends the understanding of an isolated word and is usually accompanied by object-images when the word is concrete and by nothing except word-images when it is abstract." (*op. cit.*, p. 265). Wundt says, "Whether the complication of these elements, ideas, word-sound and word-

script, occurs completely in our consciousness depends besides on which of these elements acts upon us directly in sense perception. The ideas can stay isolated under certain circumstances. The word-sound generally calls forth the object-image. The word-script awakens the word-sound with the object-image." (*Op. cit.*, vol. III, p. 543.) John Mill commenting upon Locke and Berkeley's difference of opinion, says, "While the concentration of attention lasts, if it is sufficiently intense, we may be temporarily unconscious of any of the other attributes, and may really for a brief interval have nothing present to our mind but the attributes constituent of the concepts." (*Examination of Hamilton*, p. 393). This last view seems to come the nearest to the above result of ours if we change the indefinite expression "intense" in the quotation into "brief".

§3. SELECTIVE EXPERIENCES WITH UNFAMILIAR STIMULI

In the understanding of unfamiliar stimuli where the meaning did not come promptly there appeared in consciousness a new group of experiences in the form of judgments, *i. e.*, of approval or rejection of the contents or suggestions as right or wrong. They were experiences or consciousnesses of searching, waiting, selection, rejection, certainty, uncertainty, hesitation, etc., generally attended by feelings of tension and relaxation. These are sometimes regarded as characteristic constituents of meaning. A special test was therefore made in connection with the third experiment to determine how far these experiences are alike and how far they are different in different types of thinking, *i. e.*, in the understanding of phrases and words and in the identification of non-sense stimuli.

Procedure. In the third experiment, beside English words and phrases, a number of Chinese characters, as a kind of nonsense stimulus, were added. With these characters, instead of understanding, the process was one of identification. The five English speaking observers were requested to compare the second of the two stimuli with the first, or standard, and to tell whether the two were identical or different. The standard was exposed for one second and immediately afterward the comparison stimulus which was sometimes identical and sometimes not identical, but always quite similar in shape to the standard. The reactions, all active, were made by pressing an electric key. Introspections were taken in the same way as with understanding-reactions. Easy and difficult stimuli were made by different combinations of complexity and irregularity in the shapes and in the degree of resemblance between the standard and comparison stimulus. For instance the following are some of the pairs thus matched, having shapes apparently similar but not identical.

巳巳 氷永 尊奠 窮窮

These identification-reactions and the two forms of understanding-reactions, *i. e.*, of words and of phrases, were arranged in such a way that a difficult reaction of one form was followed by a difficult reaction of the other form; and the same with easy reactions. The observers were requested, after giving the introspections of each reaction, to report, in addition, what processes (or experiences) in the two successive reactions they found similar in their abstracted forms and what different.

The following is a sample of such protocols:

A. COMPARISON OF DIFFICULT REACTIONS

Understanding. Nostrum, (Obs. Ch. III—iii, 2, time—2.0"). "Feeling of having seen the word came first. I read the word through once. First suggestion was that of the Latin word. And there I got the meaning of the word but not quite certainly. A little bit of feeling of tension is staying. I was n't quite sure of the content. I interpreted it as a sort of fake medicine."

Identification. 患藥 (Obs. Ch., time—) "Not sure. What I tried to do was to look at the separation of the figure rather than to judge by general impression, and found that some are the same while others are doubtful. So that the feeling is one of uncertainty, doubt in this case. Also there was a feeling of strain, tension with unpleasant coloration."

Comparison of the experiences. "The feeling of searching for something, of groping is quite similar with the cases of hard English words. And also the feeling of strain, tension with unpleasant coloration, is just the same."

B. COMPARISON OF EASY REACTIONS

Identification. 找戒 (Obs. Ch. time, 1.0") "Different. The impression came from the general form and not from any one particular line. Feeling of certainty is rather complete. At first there was a feeling of strain and tension which dropped as soon as I reacted. The experiment ended with a little pleasure, because I recognized the difference. So this was a personal feeling attached to the consciousness of success."

Understanding. Peace. (Obs. Ch. time—1.0") "I read it and recognized it at once. No feeling of tension. There was slight visual imagery. Feeling of certainty as to the meaning of the word. In this feeling of certainty there was the feeling that rich associations could be started."

Comparison of the experiences. "The feeling of certainty with the Chinese characters lacks the feeling of rich content. In the Chinese characters there is a feeling of recognition in the strict sense, while in the English there is no revival of the occasion when I first saw it. Thus the recognition of the Chinese character is much more a matter of sensory form than of content."

Results of the Comparison

A. *Feeling of tension and relaxation.* In regard to the qualitative differences of these feelings, all observers agreed in finding them very similar or even identical in character in the three types of thinking: identification of characters, understanding of words, and understanding of phrases. In

regard to the quantitative differences, they found, as a rule, more tension in difficult understanding than in difficult identification, and in difficult understanding of phrases than in that of words.¹

B. *Feeling of content.* All observers agreed in finding decidedly richer content feelings with words and phrases in general and very poor content feeling or none at all with the characters.² In words and phrases, they found words frequently to have richer content than phrases. The content feeling differed according to the particular words and phrases.

C. *Feelings of certainty and uncertainty, i. e., of understanding and of judgment.* Three observers found these feelings quite similar or identical in the three types of thinking, whereas the other two observers found them rather different, not only among the three types of thinking, but also from stimulus to stimulus in each type of thinking.

It appears that the other two observers, instead of comparing these feelings in their pure and abstracted forms, compared them in their complex and concrete forms. This is plainly seen from the fact that in accounting for the differences they referred either to the differences in content feelings or to those in associations or images. For instance:

Obs. Ch. (III—iv, 2.) Comparison—"As to the feeling of certainty this experience [Chinese character] is very much less complex. It simply consists in the recognition of the visual image and practically no associations at all."

Obs. Sn. (III—iii, 13—14.) Comparison—"This feeling of certainty [with the Chinese character] in my mind is connected with the seeing very clearly the whole space where I expected to see a black spot. The certainty in the case of the word is connected with the feeling of easy association. In this case it is only a feeling of difference."

The following is a typical case of the comparison of these feelings in their abstracted form, which was the method of the other three observers.

Obs. L. M. (III—iii, 3.) "There was no meaning except the visual impression. When my eye traversed the second figure and came to the place where it differs I felt that there was something wrong. And then I called up the visual image of the first one and recognized the difference, with a feeling of certainty. This feeling *itself* is just the same as in the cases of the recognition of English words."

The author thus believes this discrepancy is merely an

¹The maximum effort or tension was experienced not with absolutely difficult or unfamiliar stimuli but with partially unfamiliar ones. With totally difficult stimuli, observers abandoned the attempt as hopeless or too complex and therefore promptly relaxed.

²When the reproduction of the standard was faint or merely felt, it was sometimes regarded as having a very poor content; when the reproduction was more or less distinct it was regarded as having no content, but simply as an image.

apparent one arising out of the differences in the ways of abstracting the phenomenon to be observed.¹

So much for the experiences compared by all observers. The other similar experiences, which were compared by some observers and viewed as similar, or practically the same, in their abstracted forms, were feelings of searching, groping, waiting and the peculiar, though frequent, experience of the sudden appearance of images or ideas, feelings of satisfaction, dissatisfaction, etc. These results point to the following conclusions: 1. Selective experiences, as also feelings of tension and relaxation, cannot be regarded as either the sole or the characteristic constituents of the meaning of words or phrases, for they were found as well in reactions to nonsense stimuli. 2. On the other hand, as the *feeling of content* was found to be not merely different but totally lacking in the reactions to meaningless stimuli, this feeling may with great probability be regarded as one of the characteristic constituents of the meaning of words and phrases.

§4. ULTIMATE CONSTITUENTS OF MEANING

What are the ultimate psychical constituents of the six types of meaning mentioned above, *i. e.*, can they be reduced to psychological "elements," to three dimensional feeling and sensation having two attributes, quality and intensity, or to a specific image or feeling? This point the observers were not asked to decide. But from the results of all the foregoing study, the writer may make certain inferences. Let us start at the last type of meaning.

(1.) Fully Developed Images. Of these, visual images of objects, verbal images (in audito-motor or visual terms), suggested organic (as well as kinesthetic) images are so clear in their ultimate constituents as to need no comment here. The constituents of memory images were complex and their dominant factors, moreover, varied according to individual observers and circumstances; but, it seems, a visual factor, though sometimes faint and incipient, was a constant one, for a memory image has always a spatial localization, *i. e.*, reproduces the place where the object or event was experienced. The same thing may be said about the constituents of the

¹It is interesting to note that Rousmaniere had the same result in a similar experiment. The report says in part: "The subjects did not agree in their answers to the first problem. Some found not only that the certainty connected with their belief in the results of their addition seemed to be of a distinct type from that connected immediately with the sense of sight itself Others found but one kind of a feeling of certainty." (Harvard Psychol. Studies, Vol. II, p. 279.)

"indicative images" which are the representation of, or a pointing to, the concrete objects near at hand.

(2.) Half-developed Images. Their constituents, in the main, seem to be the same as those of fully developed images, except that in the former they are faint and incipient.

(3.) Feeling of Direction. This made an incipient stage of object-images having particular localizations, *i. e.*, memory images and indicative images. Consequently a visual factor in its faint and incipient form seems to be a constant factor. In the case of some observers the feeling was sometimes experienced predominantly in kinesthetic terms.

For instance: *Duty before pleasure*. (Obs. Sn. III—ii—15.) "I expected a sentence. I read it in inner speech and had the familiarity-feeling for it and at about the same time, perhaps a little bit later, I had that sort of classification which I frequently have in such cases, and thought this is a moral maxim. Then I tried to get more special cases of some duty which I have. I tried to think, as an example of this, of something connected with my own affairs, but I did n't succeed very well. I had a very vague and indefinite idea of 'duty' and 'pleasure' and there was a kind of location here in the direction of my office. I did n't get any further idea about it, except the thought about something I had planned. But I think it may be this that I had some letters this morning which I wished to read, and was thinking that I would not be able to read them until after my lecture. There was only a tendency to run in that direction without any actualization of the circumstance."

Victory or Westminster Abbey. (Obs. Cff. I, A—14.) "Feeling of surprise because I expected something more, and also because of the peculiar construction of the sentence. The name Nelson came up almost at once, partly because his ship was called 'Victory' and partly because it suggested a saying such as he might have uttered. A vague almost visual image of the picture of Westminster Abbey and feeling of direction, *i. e.*, from the sea where the battle was fought to London as if my eyes were moving from one point to another—I felt inner movements of the eyes. Feeling of meaning was of a great warrior determined either to win or die. In this case there was n't much inner speech. The visual image took the place of the auditory though it was not very definite."

Sometimes the description of this feeling was too indefinite to surmise.

For instance: *Rhapsody*. (Obs. Sn. III—i—17.) "First my attention was good. I read the word in inner speech, and I had a suggestion of it in a musical sense, a certain piece of music called a rhapsody. I think there was a sort of direction toward Mechanics Hall where the concert was held. Two or three years, or it may be one year ago, I heard the Hungarian Rhapsody there. There was a sort of vague association with other concerts. Then I read the word again in inner speech and then I tried to get a meaning for it and that time a very faint, shadowy suggestion of a person in a state of rhapsody was seen. The direction of it was different from that to Mechanics Hall. It was very indefinite and faint and hardly can be called an image at all.

Heaven. (Obs. Gl. I—w—15.) First sound, next understanding. And third, a vague image of the sky or rather opening above. The mind went upwards. No visual image of the color.

(4.) Feeling of Content. The observers often described

this feeling as one of coming associations, incipient suggestions, etc., as already mentioned. This introspection is corroborated by the following considerations in regard to the relation of the feelings of richness and poorness of content to their concomitant conditions, *i. e.*, 1, to the material or kind of stimulus; 2, to the duration of the reaction; and 3, to the conditions under which the stimulus was given.

a. Material conditions. With absolutely difficult or unfamiliar stimuli when the observers had no definite suggestions or associations and their minds were "shut up" or "blank," no content feeling was present. Uninteresting or unfamiliar words usually awoke poorer content feeling than interesting or familiar ones did. Chinese characters (the ones to be compared in the identification experiments) awoke no proper content feeling. With a single definite and vivid general object-image (no matter whether it was visual, organic or kinesthetic), excepting memory images, the observers seldom experienced a rich content feeling, excepting in some cases of passive reaction where the observers exceptionally dwelt long on the suggested images.

b. Duration of reaction. Content feeling was not present in the reflexive type of reaction released by the pure feeling of familiarity or recognition alone. It was replaced or weakened on some occasions by a definite or vivid image occurring later in the development, which shows that proper time relations are essential for the appearance of this feeling.

c. Conditions under which the stimulus was given. Familiar words or phrases presented simultaneously in number and for a short interval awoke no content feeling.¹ Isolated words as a rule awoke richer content feeling than those given in phrases. A single noun awoke frequently a richer content feeling than did a short phrase.

These phenomena of the concomitances of the subjective and objective changes will be difficult to account for adequately in any other way than to assume a content feeling as a consciousness of the actual, simultaneous, and incipient excitation of a number of past experiences or images related to the stimulus. If a large number of such associations is actually excited, the result will be the feeling of richness of content. If the number is small, the result will be the feeling of poorness of content. If there is no association, the result will be the feeling of no content. The lack or poorness of content feeling in the case of sensory familiarity, "pure concept," and a number of simultaneously exposed words and phrases, is simply due to the insufficiency of necessary time

¹ This test was made in one of the auxiliary experiments.

for the actual reinstatement of past experiences, even in an incipient way. Familiar words or phrases have, of course, numerous potential associations, but to have a content feeling it is necessary to actualize some of them, at least. Too vivid or too definite single images work detrimentally to the feeling of rich content, because they absorb too much of the mental energy to awake at the same time numerous other incipient reproductions. Isolated words give richer feeling than phrases under the same circumstances, because there the associations are not circumscribed and the mind can welcome whatever related associations may revive, and the result is the crowding together of these associations in their incipient forms which is felt as richness of content. But this state of experience does not long endure, for one of the strongest of them, such as a recent memory association, will soon push itself up completely, as was often the case in our experiments. When a word is interwoven in a context, only such associations are tolerated by the mind as will conform to the general purport or meaning of the phrase, and all other incoherent associations are suppressed.¹ The richer feeling often attending a single word in comparison with a short phrase is perhaps due also to insufficiency in the time allotted to each word of the phrase for the development of its own content feeling, *i. e.*, the mind moves on too quickly to the next word. The rich content feeling of a memorial association, in spite sometimes of its comparative vividness and definiteness, is very likely due partly to the details and variety of the imagery and partly to emotional excitations. The following instances will illustrate this point.

Philosophy. (Obs. *L. M.* III, ii—2.) "Sense came very quickly; not with the reading of the word but immediately after; and it seemed to have a great deal of meaning, perhaps because of the fact that I had been reading philosophy last night. There was feeling of tension and excitement until I got the sense. The feeling was agreeable."

Sleep is necessary for health. (Obs. *Ch.* III—ii—9.) . . . "The feeling of familiarity was pretty complete because it suggested my own condition, that is, I did not sleep last night very well. So there was a pleasure in it, and thus the feeling of familiarity had a rich content, for it refers to personal interest."

Sleep is necessary for health. (Obs. *E. M.*) "Perfect feeling of familiarity and certainty of judgment. It awoke a good deal of association with it, because I have made an effort without success to sleep after dinner this afternoon."

Regarding the ultimate analysis of this feeling which is the awareness of numerous simultaneously excited incipient

¹ Cf. Huey's result: "The words given in isolation gave a greater variety of association than did the context words." *Am. J. of Psychol.*, Vol. XII, p. 282 ff.

images, it is clear that this feeling has nothing to do with the tri-dimensional feelings which may, or may not accompany it by way of addition. Again, as it is the direct awareness of images themselves, it cannot be called a subjective affection or emotion, excepting in cases of emotional excitations in memorial association which give the meaning a "sense of reality" or "warmth." Emotions in these cases, however, only emphasize and do not make up the content itself, which is constituted of memorial associations. There is need always of incipient images for meaning, whether it is cool or warm, dry or rich. Whereas, emotions of various form can exist without understanding or sense of meaning.

Neither can we describe the content feeling in terms of a definite and specific image for it is, first, the resultant of many images or associations, each of which consists of more than one specific or sensory image (such as a visual, an auditory, a motor, etc.); and, secondly, these images are all only incipiently awakened. The pure feeling of richness or poorness of content, without any subjective trace of images, may be, in the meanwhile, called a "total feeling," not in the sense of the fusion of feelings proper, but in the sense of the fusion of faintly excited different images.

(5.) Pure Feeling of Concept or Meaning. Owing to the vagueness of the customary terms "concept" or "meaning" as regards their psychical constituents, the writer has no direct basis for analysis when the observers simply speak of a concept or meaning without further description. So that so long as there is given no positive description of this experience, the only way at hand is to infer it. The inference is made from the nature of other types of meaning which generally immediately precede or succeed the feeling of concept, or sometimes replace it. The type which precedes is the pure feeling of familiarity, or recognition of the stimulus as the one already familiar. The type which succeeds is the pure feeling of content. So that a pure feeling of concept, meaning or knowing, without any further configuration or content may be identified with the pure feeling of recognition or familiarity, or with something quite similar in nature. If the feeling of concept or meaning has something more substantial, it may be identified with the content feeling. If its nature becomes more materialized, so to speak, it will be found identical with the feeling of direction, or with half or fully developed images. But in such cases it is no more pure.

(6.) Pure Feeling of Familiarity or Recognition. This feeling, as already stated, was the first and quickest to appear of all types of meaning, and attached directly to the stimulus with-

out any intermediary except audito-motor reading when the stimulus was exposed. It thus makes the first or the most primitive stage of the series of reproductive processes. As a reproductive process, there seems to be no important differences between the experience (as such) of familiarity with the stimulus as one experienced before (whether understood or not) or "sensory recognition," and the familiarity with the stimulus as one understood before or known, or "conceptual recognition."¹

In the earlier stage of this feeling there was often no subjective difference between the "sensory familiarity," *i. e.*, the recognition of the stimulus as one experienced before (whether understood or not), and the "conceptual familiarity," *i. e.*, the recognition of the stimulus as one, the meaning of which is known to the observer. This fact is shown in the cases of premature or mistaken reaction, as mentioned before. Thus in their first stage of reproduction both sensory and conceptual familiarity must be very much alike or identical.² The subjective criterion by which the observers became soon after aware of their mistakes must very likely be the presence and absence of content feeling or "coming associations."

So much for the psychological nature of pure knowing as meaning; now regarding the ultimate constituents of sensory familiarity. Let us take the simplest case. Perhaps the simplest, purest and at the same time the most durable type of this feeling is that which was experienced by the observers in the recognition of the Chinese characters in the forms of purely visual nonsense stimuli practically free from associations. Though obviously this feeling is thoroughly originated by the previous impression of the same stimulus, the observers found it difficult to give any positive description of it in terms of psychical elements. By way of negative description, it may be noted that not only was there no one of our observers who ever positively identified this experience with tridimensional feeling but also there were many who reported it as decidedly different. That this feeling is not a mere alternation of the feelings of tension and relaxation is seen in the fact that observers often relaxed (thinking the characters too complex to identify and so giving up the attempt) without

¹The reader should not confuse this case of the reproduction of the feeling of knowing of once known (or easy) stimuli with the processes of knowing for the first time of unfamiliar stimuli.

²It must be remembered that we are dealing here with the reproduction of the feeling of knowing awakened by the perception of once known or understood, *i. e.*, familiar or easy stimuli, and not with the processes of knowing for the first time of unfamiliar or difficult stimuli which are quite complex.

having the experience of familiarity or recognition. Nor can it be regarded as a specific image, for it occurred earlier than any definite image. Even organic imagery or sensation which is more primitive and undifferentiated and consequently more all-embracing than kinesthetic imagery, will no more cover this feeling than do feelings of tension and relaxation. One will, perhaps, find this feeling in its bare form to be localized centrally rather than peripherally, as "spiritual" rather than corporeal. I may say, therefore, that this feeling is a fundamental, not further reducible, retrospective quality of a present impression, *i. e.*, a coloration or configuration given to the present impression by the rudimentary revival of its past experience. The present impression may be any existing content, sensation, feeling, image, idea, thought, etc. It is thus different from ordinary quality or intensity of sensations. It is not an independent element, as it attaches always to sensations or feelings and never occurs alone. The pure feeling of knowing awakened by familiar stimuli is only a special case of this feeling of familiarity; consequently "thought" in such a sense is neither a third element¹ nor a highly elevated something, but merely the most primitive and rudimentary form of reproduction. Thought in the sense of "transcendental" reference² which seems to have more content than pure recognition or knowing, may be found in our content feeling if its introspective aspect is indefinite, or in the feeling of direction or the indicative images if it is more definite. Bühler's "intention"³ in the sense of condensed thought or quick recapitulation may be found in our complex memorial associations as half-developed images. In short all varieties of meaning-experiences are found as belonging to one or other of the different stages of the revival of the related past experiences.

Social and practical custom attaches a certain cluster of associations to a word or phrase as its meaning to the exclusion of others, so that our understanding of a word or phrase, which is a direct or indirect,⁴ incipient or full recalling of such related associations, is, in all cases, a selective or purposive action from the logical or outer point of view. With easy or familiar words such selected associations are, however, ever ready and come promptly without any subjective or psychological experiences of the sort which usually characterize the selective processes, so that psychologically, or from

¹ Cf. Bühler: *Op. cit.*, p. 329.

² Messer: *Op. cit.*, 113.

³ *Op. cit.*, p. 346 ff.

⁴ That is, through the associations of intermediary images whose meanings are well known.

the inner point of view, a cluster of selected associations or meaning once mechanized becomes identical with a cluster of random associations, or pure reproductions. What is then the meaning in case of an unfamiliar or difficult word or phrase the comprehension of which is attended by a series of selective experiences, such as, feelings of effort, suspense, hesitation, searching, rejection, doubt, uncertainty, etc.? The answer is that a meaning, as a *resultant*, remains always the same whether it is reached through a strenuous or an easy process, whether it is consciously selected or mechanically reproduced. These selective experiences were not only common in all types of active or volitional thinking, whether it is understanding of abstract phrases or comparison of sense impressions, but are also found in their pure forms to be very much alike or identical. They were found to be stereotyped and did not develop to such successive series as did the reproductive tendencies, excepting that they showed changes in intensity and oppositions like those of feeling proper. Their apparent configurations and development must be, in reality, those of the content or reproductive series to which they attach. Clearness and unclearness of meaning, abstracted from content-feeling or reproduction, may be reduced to the mere feelings of certainty and uncertainty.

Further inquiry into the ultimate constituents of feelings of certainty and uncertainty and other selective experiences, which can be found in simpler and purer forms in the identification or comparison of purely sensory stimuli, is left for later studies.

SUMMARY OF THE PRINCIPAL RESULTS OF THE STUDY

1. Whether we have audito-motor or visual imagery of the stimulus word depends primarily upon whether the word is exposed or spoken.

2. The kinds of imagery, the frequency of which seemed markedly influenced by the individual peculiarities of the observers, were as follows: 1, Motor speech; 2, visual speech; 3, associated (suggested) word-images; 4, associated object-images in visual terms.

3. The frequency of memory images is primarily conditioned neither by the concreteness or abstractness of the stimulus word nor by individual peculiarities, but by the slowness or quickness of the reaction.

4. The customary method of association experiments seems to be too artificial for the study of natural or real associations. Whereas the *Ausfrage* method seems to be better adapted both to the study of the general laws of association and to the study of individual peculiarities of association.

5. Whether or not one has in the understanding of a word or phrase a concrete representation depends primarily upon the duration, *i. e.*, upon the time one dwells upon it.

6. The characteristic constituents of the meaning of a word or phrase are not selective experiences, but series of different phases of reproduction.

7. "Feeling of concept" may be reduced to either "feeling of familiarity" or "feeling of content." "Feeling of content," which is the awareness of the more or less fused aggregate of incipient associations, seems to be hardly reducible to any specific images. "Feeling of familiarity," which is the most fundamental and elementary form of the reproductive experience and seems to be reducible neither to the feelings proper nor to the so-called intensity-quality attributes of sensations, may be regarded for the present as a third or retrospective quality of sensations or other psychic experiences.¹

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THE PSYCHOPATHOLOGY OF APRAXIA

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It is only under special and favorable circumstances, either of experiment or of disease, that certain complex psychic or motor disturbances can be traced to an exact cerebral localization. The most interesting and at the same time the most complicated of these disturbances seem to be localized in some portion of the left hemisphere, particularly those conditions in which there seems to be a loss of the various types of speech images (aphasia) or a loss of the motor memories of the limb movements for a definite act or purpose (motor apraxia). According to recent researches, the left hemisphere seems to preponderate, in certain requested or spontaneous movements of the limbs in the same manner that it preponderates in speech. For instance in many lesions of the left hemisphere in which the right arm and leg are paralyzed, there may result a motor apraxia of the non-paralyzed left arm, thus indicating the existence of a special action of the motor centres of the left side of the brain. It seems likely that these various complex phenomena are really disorders of associative memory, either for identification or motility. There are not only different types of these disturbances but also different varieties of the same type, from the simplest to the most complex.

It is the purpose of this paper to review the recent literature on apraxia, to attempt to determine what light these studies throw upon a complicated psychic disturbance which seems to have a fairly definite cerebral localization and finally to study two cases of apraxia which have come under personal observation. The harmonizing of certain anatomical data with mental disturbances is of the greatest psychological importance, and nowhere can this be better done than in the problem of apraxia.

Apraxia was a term formerly applied to the intellectual non-recognition of objects, but the more recent investigations have shown that the term had best be limited to certain essential disorders of voluntary acts and movements. The chief disturbance in motor apraxia is an inability to make movements for the purpose demanded by the will, although the subject may understand commands and the use of objects; memory and attention may be normal and the limbs may be free from

paralysis, ataxia or tremor. Liepmann's case of unilateral apraxia which was studied clinically and anatomically in a most thorough manner, has formed the basis of the modern conceptions of the condition. In this case there was successful correlation of the clinical symptom-complex with the pathological findings. Later Liepmann extended his studies to comprise all the motor disturbances of brain disease.¹

Previous to the work of Liepmann the ideas concerning the nature of apraxia were in a very unsatisfactory condition. It was looked upon as a form of a disorder of identification related to mind blindness. Hughlings Jackson's ideas concerning imperception or Allen Starr's description of the loss of object memories, are instances of the older interpretations. Monakow had also observed that certain aphasics showed either an inability to execute certain movements on command or misused familiar objects. Under the name of asymboly there also was described a loss of images of palpation, of kinetic movements and of the use of objects. The term asymboly has also been applied to an imperfect grasp of the nature and use of common objects, such as occurs in confusional or delirious states. In a review of the question of tactile aphasia,² I pointed out that the term can also be used to designate not only a failure to know the shapes of objects and their cardinal qualities, but also the ultimate recognition of the objects

¹ The following is a bibliography of Liepmann's principal writings on apraxia.

- A. Das Krankheitsbild der Apraxie (Motorischen Asymbolie) auf Grund eines Falles von einseitiger Apraxie—*Monat. f. Psychiatrie u. Neurologie*—Bd. VIII.
- B. Der weitere Krankheitsverlauf bei dem einseitigen Apraktischen und der Gehirnbefund auf Grund von Serienschritten—*Ibid.*—Bd. XVII and XIX.
- C. Ueber Störungen des Handelns bei Gehirn-Kranken—1905.
- D. Drei Aufsätze aus dem Apraxiegebiet—1908.
(This monograph is a collection of three previously published articles on apraxia—Kleine Hilfs-Mittel bei der Untersuchung von Gehirn-Kranken—(1905). Ueber die Function des Balkens beim Handeln und die Beziehungen von Aphasie und Apraxie zur Intelligenz. (1907) Die linke Hemisphere und das Handeln (1905).
- E. The section Die Krankheiten des Gehirns in *Lehrbuch der Nervenkrankheiten* (Herausgegeben von Hans Curschmann—1909) is by Liepmann and contains a summary of his latest views on apraxia.
- F. Ein Fall von linksseitiger Agraphie und Apraxie bei rechtsseitiger Lähmung (Liepmann and Maas)—*Jour. f. Psychologie u. Neurologie*—Bd. X, 1908.
- G. Ein neuer Fall von motorischer Aphasie mit Anatomischen Befund (Liepmann and Quensel)—*Monat. f. Psychiatrie u. Neurol.* Sept., 1909.

² The Question of Tactile Aphasia—*Journal Abnormal Psychology*—Vol. I, No. 6, 1907.

themselves. In its broadest sense, therefore, basing it at least upon the available data of our clinical analyses, we may say that apraxia in general is motor perplexity plus a disorder of identification. Apraxia may be divided into the motor and ideational forms. In motor apraxia the limbs do not obey the psychical wish: there is pure motor perplexity. The motor memories for movements of the limbs may be preserved but these memories are distorted, isolated or insufficiently connected with other portions of the cortex. In motor apraxia there is also a defect in the use of objects, although the objects may be perfectly recognized. The subject merely fumbles with objects; he is unable to translate a subjective purpose into an objective action. In ideational apraxia or agnosia the subject misuses objects because there is a disturbance of identification. For instance, he may think a comb is a cigar and so put it in his mouth as if smoking it. The term apraxia should therefore be limited to certain motor disorders, and it is best to refer to the ideational disturbance as agnosia.

Before the case of unilateral apraxia came under Liepmann's observation, the patient was believed to be suffering from aphasia and post-apoplectic dementia. The principal physical symptoms in this case were left facial paralysis, unequal pupils and weakness of the left leg, but no real paralysis of any of the limbs. There was pure motor aphasia and some alexia, but no word deafness, mind blindness, hemianopsia or unilateral psychic blindness. There was a marked disorder of the stereognostic sense and slight hypoesthesia of the left hand. Orientation, memory and attention were normal. The movements of the right arm were poorly executed, ill-directed and fumbling. With the left leg and arm, however, everything was correctly done. It was demonstrated, after careful study, that a typical right-sided motor apraxia was present. The analysis of this peculiar psychic condition presented many difficulties, but it was at last successfully accomplished. With the right hand responses to simple orders, such as touching the nose or buttoning the coat, and the imitation of movements were incorrectly done and with much fumbling. All commands, however, were correctly and promptly executed with the left hand. The patient blundered at every attempt to use objects with the right hand. For instance when a comb was placed in his right hand he would put it behind his ear like a penholder. Writing was defective with the right hand; the left hand produced mirror writing. For further clinical details the original monograph should be consulted.

The interpretation of the symptom complex in this case is a striking example of what may be accomplished by careful

clinical observation. Word blindness and word deafness were both absent, as all orders, movements and likewise the imitation of movements, were correctly performed with the left hand. The question may be raised that because the patient identified and correctly used objects with the left hand, and not with the right, there existed a right unilateral psychic blindness. This objection had no basis in fact, because objects placed in the right visual field were promptly identified. Because of the above condition Liepmann believed that the apraxia was not dependent on defective recognition of objects. In attempting an explanation of the condition he stated that the centripetal stimuli from the limbs on the right were perceived in the left sensory-motor area of the brain (the anterior and post-central convolutions and probably a portion of the parietal lobe), but on arriving there the impulses were blocked and therefore not transmitted to other cerebral centres. The left sensory motor area was therefore cut off from all communication with the rest of the cortex. The patient had lost the kinetic memories of movements of the right side because the left or leading hemisphere was isolated. He perceived the position, movements and tactile sensations of his limbs on the right, but was incapable of synthesizing these elements. The localizing diagnosis of the condition based on the clinical symptoms, showed how carefully the case was observed and analyzed, particularly if this be compared with the later anatomical findings. Because of the absence of word deafness, paralysis, sensory disturbances, hemianopsia, or psychic blindness, the corresponding brain centres must have been intact. The motor aphasia indicated a lesion of the third left frontal convolution and probably of the insula. The motor area and the gyrus angularis could not have been involved, because there was no paralysis and no symptoms pointing to the central optic tracts. The supra marginal gyrus and superior portions of the parietal lobe were probably destroyed. The corpus callosum was probably also involved in the process, because of the interruption of communication with the right hemisphere.

The patient died two years later from an apoplectiform attack. The anatomical findings corresponded in a remarkable manner with the earlier localizing diagnosis. The autopsy showed in the left hemisphere two foci of softening in the third left frontal convolution and a subcortical cyst in the inferior portion of the ascending parietal convolution. In the right hemisphere there was a destruction of a majority of fibres for the face and limbs on the left side, and foci of softening in the supra-marginal and angular gyri. The corpus callosum had entirely disappeared, with the exception of

the splenium. The serial sections of the brain showed these lesions with great clearness. The degeneration of the corpus callosum and of the tapetum, the softening and degeneration of the right internal capsule, the cyst of the insula and the shrunken inner nucleus of the right thalamus, were well marked. This degeneration of the corpus callosum is an important point, for Liepmann has shown that the left hemisphere is influenced by the motor region of the right cortex through the fibres of corpus callosum, and when these fibres are destroyed, the left hemisphere becomes isolated.

It was not long before Liepmann's work was confirmed by other investigators. The fundamental difference between apraxia and aphasia was recognized by Oppenheim, and later Pick reported an arterio-sclerotic and senile case in which apraxic symptoms were episodic. In a case reported by Liepmann and Maas, there was a right-sided paralysis with a left-sided agraphia and apraxia. The agraphia may be regarded as merely one of the manifestations of the apraxia. The right hemisphere was intact. In the left hemisphere, however, there was an area of softening involving the whole of the left half of the corpus callosum. In the serial sections of the brain the softening could be traced along the whole left side of the corpus callosum from the knee through the body to the splenium. In interpreting their case the authors stated that the left-sided apraxia was due either to the separation of the left hand from the memory centres of the left hemisphere, or perhaps the direct pathway for impulses to the sensory-motor area of the right brain had been destroyed. In Strohmayer's case,¹ in which aphasic and ataxic symptoms were absent, and the muscular and palpation senses intact, the patient recognized and named objects correctly, but misused them. Anatomically there was found a lesion of the left inferior parietal lobe, the supra-marginal gyrus, the forceps major and the superior longitudinal bundle. The gyrus angularis was not involved. Abraham² observed apraxic phenomena in two general paralytics and in a later contribution he reported a case of unilateral apraxia which on autopsy showed a lesion of the superior parietal lobe on the left. In Marcuse's case the apraxic symptoms were due to a general senile brain atrophy.³

¹*Strohmayer*: Ueber subcorticale Alexie mit Agraphie und Apraxie, Deut. Zeit. f. Nervenheilkunde, Bd. XXIV, 1903.

²*K. Abraham*: Ueber einige seltene Zustandsbilder bei Progressiver Paralyse—Allg. Zeit. f. Psychiatrie, Bd. LXI, 1904.

Ibid.: Beiträge Zur Kenntnis der motorischen Apraxie auf Grund eines Falles von einseitiger Apraxie—Centralbl. f. Nervenheilk. u. Psychiatrie, Mar. 1-15, 1907.

³*Marcuse*: Apraktische Symptome bei einem Fall von Senile Demenz. Centralbl. f. Nervenheilk. u. Psychiatrie, Dec., 1904.

Here the defects of the voluntary acts were caused by a continuous amnesia. The patient would probably forget the course of a movement after it was once started, in the same way that the tactile asymboly reported by Bourdon and Dide, was due to a kind of a continuous amnesia for tactile impressions.¹ However, very extensive amnesias may be free from any form of apraxia, as in the Lowell case of amnesia, a proof that these particular disorders of memory play little or no part in the mechanism of apraxia.² Pick insists strongly on the psychic element in all cases of apraxia.³

D'Hollander⁴ has reported two cases of apraxia. The first of these occurred in a case of focal general paralysis and was bilateral. Anatomically nothing was found which could explain the condition other than the significant point that the left hemisphere of the brain was smaller than the right. In the second case, one of alcoholic dementia, there gradually developed a slight paralysis of the right arm and leg with a left-sided apraxia, thus showing the supremacy of the left hemisphere in the execution of voluntary movements. For localizing diagnosis the author suggested a lesion on the left side of the brain extending to the central semiovale, and involving the callosal fibres that go from the left to the right hemisphere.

Goldstein⁵ has reported a case in which after the disappearance of a left-sided paralysis there appeared a motor-apraxia limited to the same side. For localizing diagnosis he suggested a lesion in the subcortex of the right central convolution, damaging its connections with the frontal lobe and involving the fibres of the corpus callosum. A year later, an anatomical examination disclosed, among other things, a complete destruction of the corpus callosum throughout its whole extent.⁶

In Rhein's⁷ case the right hand was apraxic; apraxic phenomena were present in chewing and walking while the left hand was capable of only elementary reflex acts. The pos-

¹B. Bourdon and M. Dide: *L'Année Psychologique*, 1904. (See my abstract in the *American Journal of Psychology*, April, 1905, pp. 252-254.)

²Isador H. Coriat: The Lowell case of Amnesia, *Journal Abnormal Psychology*, Vol. II, No. 3, 1907.

³A. Pick: *Studien ueber Motorische Apraxie*, 1905.

⁴F. D'Hollander: *Bulletin de la Société Médecine Mentale de Belgique*, 1907-8 (an excellent summary of all the literature, with a report of two personal observations).

⁵K. Goldstein: *Zur Lehre von der motorischen Apraxie*, *Journal f. Psychologie u. Neurologie*, Bd. XI. H. 4-5-6.

⁶Der makroskopische Hirnbefund in meinem Falle von linksseitiger motorischen Apraxie, *Neurol. Centralblatt.*, Sept. 1, 1909.

⁷J. H. Rhein: A case of apraxia with autopsy. *Journal of Nervous and Mental Disease*, Vol. 35, Oct., 1908.

terior portion of the corpus callosum was found degenerated at autopsy.

In Vleuten's¹ case, there was a sarcoma in the left hemisphere, which by invasion and pressure destroyed the cingulum, the whole of the left half of the corpus callosum and part of the right genu. The right hand and arm was tremulous, while the left hand and arm showed apraxia. Here the left-sided apraxia was produced by a lesion which destroyed the callosal fibres. Bychowski² has reported a case in which the apraxia was found to be due to a cyst in the left hemisphere which destroyed and displaced a portion of the left side of the corpus callosum. In Hartmann's³ three cases of apraxia, one was due to a tumor in the left frontal region, and the second to a tumor involvement of the corpus callosum. In a case recently reported by Tooth⁴ there was an inconstant motor-apraxia of the left hand. At autopsy there was found a tumor occupying a portion of the right frontal lobe, involving the anterior half of the corpus callosum.

In discussing the psycho motor disturbances of various mental diseases Kleist has shown how apraxic phenomena may appear in the hyperkinetic and akinetic motility psychoses. He thinks that these disturbances are probably due to psychic factors and that they may have a definite cerebral localization, particularly in the parietal lobe.⁵ This is of interest if we remember that in some cases of unilateral motor apraxia, the parietal lobe was found involved as well as the corpus callosum. For instance in one case of an akinetic motility psychosis which came under personal observation, it was noted that the subject had lost all knowledge of the use of simple objects. Stransky⁶ has shown how apraxic phenomena may appear in dementia præcox. In these cases he interprets it as due to a loss of unity between the understanding and the will.

Apraxia may also occur as a disorder of consciousness in delirium and post-epileptic states and under both these conditions without any definite focal lesion. Here the apraxia

¹C. F. V. Vleuten: Linksseitige motorische Apraxie, Ein Beitrag zur Physiologie des Balkens, Allg. Zeit. f. Psychiatrie, 1907.

²Z. Bychowski: Beiträge zur Nosographie der Apraxie—Monat. f. Psychiatrie u. Neurologie. Bd. XXV, 1909.

³F. Hartmann: Beiträge zur Apraxielehre, Monat. f. Psychiatrie u. Neurologie. Bd. XXI, 1907.

⁴H. H. Tooth: (Abstract in Review Neurology and Psychiatry, July, 1909, pp. 475-476.)

⁵K. Kleist: Untersuchungen zur Kenntnis der psychomotorischen Bewegungsstörungen bei Geisteskranken, 1908.

⁶E. Stransky: Zur Auffassung gewisser Symptome der Dementia Præcox, Neurol. Centralbl., Dec., 1904.

is ideational, in the sense of a disorder of identification. In these cases the phenomena tend to disappear as the mental state improves. In my study of a delirious state associated with vestibular disturbances, apraxic phenomena were present.¹

Motor apraxia has been found to occur in aphasia. The disorders of movement which one frequently finds in aphasics are very likely not due to any intellectual defect as claimed by Marie, but to a disorder independent of aphasia, namely apraxia. Cases of dementia very rarely show symptoms of apraxia. It cannot be said that apraxia is due to any intellectual disorder because in disturbances of motility where the apraxia is limited to one side of the body, the other side of the body will be found to be absolutely normal. This shows that intellectual defects may be practically ruled out in motor apraxia, at least in the unilateral types, unless it is absurdly assumed that only one side of the brain is demented. In one of my cases of sensory aphasia the patient was bright and alert and yet he ridiculously insisted on eating an egg, shell and all. Here we have an example of apraxia occurring in an aphasic subject without mental defect. Ideational apraxia (agnosia), however, may occur in certain abnormal mental states, such as in delirious conditions or in multiple brain lesions.

There are many striking points of similarity between motor aphasia and motor apraxia. The motor speech mechanism is really a form of movement without objects. The centre for motor speech is located in the left hemisphere and it has also been shown how the kinetic memories for co-ordinated movements likewise preponderate in the left hemisphere, at least in right-handed subjects. In complicated movements which through habit and evolution, have become bilateral, such as the lip-tongue-larynx movements in speech, there is usually a loss of these movements in a lesion of the motor speech area. In the limb movements which as a rule are not bilateral, but in which right and left are independent and in marked contrast, there results a unilateral apraxia when a lesion on one side of the brain is favorably situated.

How then are we to explain these complex phenomena and what light can be thrown upon them by brain anatomy and physiology? Certainly the subject of apraxia opens up one of the most inviting and at the same time one of the most difficult fields in psychopathology. At first it will be well to attempt to analyze apraxia on the basis of the data furnished by those cases where it was possible to make an anatomical examination of the brain.

¹The Cerebellar-Vestibular Syndrome, American Journal of Insanity, Vol. LXIII, No. 3, Jan., 1907.

The point of significance in the majority of apraxic cases was the involvement of some portion of the corpus callosum. The corpus callosum must then be possessed of a definite function, as its involvement seems to have been as invariably present in motor apraxia as a lesion of Broca's convolution in motor aphasia or of the central optic tracts or the cuneus in hemianopsia. What then is the function of the corpus callosum and how are we to harmonize the motor and psychic disturbances with the anatomical findings?

The fibres of the corpus callosum connect the two opposite sides of the brain. In fact in experimental lesions of the thumb centre in monkeys, the degenerated fibres could be traced directly through the corpus callosum to a similar area on the hemisphere of the opposite side. The knee of the corpus callosum sends its fibres to the fore brain as the forceps anterior. The fibres of the splenium go to the hind brain (occipital region) and to the temporal lobes as the forceps posterior and make up the greater part of the tapetum. The body of the corpus callosum connects the two hemispheres in the mid position. Both the left and the right hand centres, corresponding respectively to the movements of the right and left hands, are connected by means of the fibres of the corpus callosum.

By reason of its wide connections, the corpus callosum plays an important part in the execution and association of voluntary movements. In tumors of the corpus callosum, disorders of movements are frequently seen, such as apraxia, tremor and ataxia. In a case of tumor of the splenium of the corpus callosum which came under personal observation, there was a paralysis of the right arm and of the right side of the face and a continual coarse tremor of the left arm. A lesion of the corpus callosum alone causes a left-sided motor apraxia without paralysis or apraxia on the right. A lesion of the left hand centre causes paralysis on the right side and apraxia on the left. It seems that certain left-sided lesions produce a left-sided apraxia because the control of the right hemisphere over the innervation of the left arm by means of the corpus callosum has been cut off. The left hemisphere thus becomes isolated and leaderless.

The following diagram will indicate the various brain lesions which may produce motor apraxia.

(See figure I.)

A lesion at I (the left brain centre for the right arm) will produce a paralysis of the right arm and an apraxia of the left arm because this centre is either isolated or is deprived of the guidance of the right hemisphere. A lesion at II (in the subcortex of the left Rolandic area), injuring the

projection and callosal fibres to the right hemisphere, will also produce a paralysis of the right arm and an apraxia of the left for the same reason as indicated under lesion I. A lesion at III (the left internal capsule) will cause a right-sided paralysis, without any apraxia on the left, because the corpus callosum is not injured. A lesion at IV (in the corpus callosum) will cause a left-sided apraxia. A lesion at V (in the left centrum ovale, catching only callosal fibres) will likewise cause a left-sided apraxia. In both these conditions,

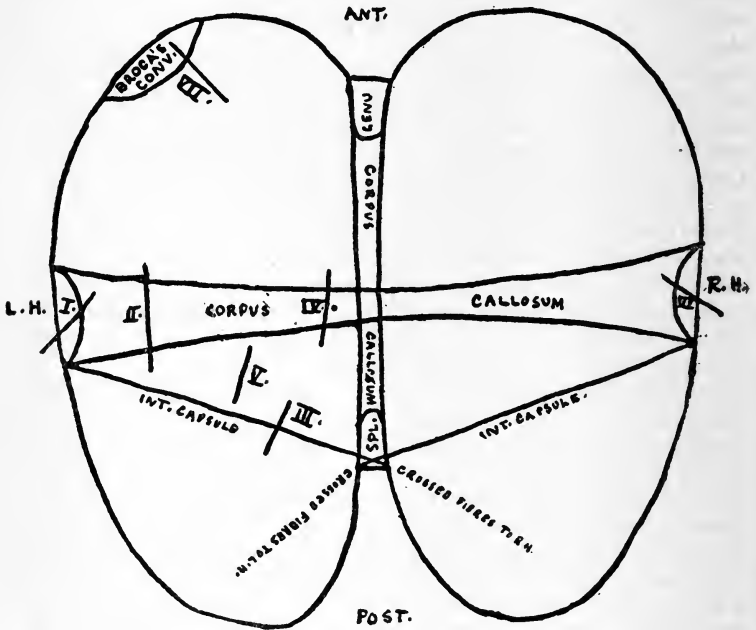


Fig. I

A diagrammatic horizontal section of the brain, to illustrate the anatomical basis of motor apraxia.

L. H. Left brain centre for right hand.

R. H. Right brain centre for left hand.

The anterior and posterior and the lateral course of the fibres of the corpus callosum are shown diagrammatically.

the apraxia is due either to a loss of the guiding influence of the right-hand centre over the left hand or to an isolation of the hand centre from the rest of the left hemisphere. A lesion at VI may cause a right-sided apraxia from an interruption of the callosal fibres passing to the left side of the brain. (Liepmann's first case.) A lesion at VII (Broca's convolution) would

produce motor aphasia, and if the lesion be of sufficient size and extent to catch the radiations of the genu of the corpus callosum (or forceps anterior), there will be likewise caused a left-sided motor apraxia. That this latter combination is not impossible, is shown by the case recently published by Liepmann and Quensel and also by the second of my reported cases.

We must admit that the psychical condition of apraxic subjects presents great difficulties of analysis, even more so than the analysis of disturbances of the language mechanism in the various types of aphasia. Probably with increasing knowledge of apraxia some of these difficulties may be overcome. Motor apraxia in its strictest sense is rarely bilateral as can be easily seen from the reported observations. It is in ideational apraxia or agnosia that the bilateral nature of the disturbance may be detected. In fact, agnosia seldom or never occurs as a pure isolated disorder in focal brain disease. When it does occur in focal disturbances of the brain, it is merely as a complication of the motor type of apraxia. Agnosia is most frequently found in diffuse brain affections (senile dementia), in delirious states particularly due to epilepsy or alcohol or in the motility psychoses. Hence agnosia is a more general disturbance, while apraxia is a focal disorder limited to certain limbs or to muscle groups.

In motor apraxia the limbs do not obey the psychical wish, although that wish and the motor image of the willed movement may be clearly present in the mind of the subject. It is this inability, on the part of the subject, to transfer the psychical wish into a specified innervation of a limb, which causes motor apraxia. In ataxia it is the elementary condition of the movements which is at fault, while in apraxia there exists a disharmony between the purpose of the movement and the idea of the object with which the purpose is carried out. In other words, in motor apraxia there is a deficient adjustment to a purpose. In motor apraxia the co-ordination of movements is normal, but there is a faulty intrapsychic process and so the ultimate purpose of these movements becomes incorrect. The motor apraxic cannot translate the subjective idea of a movement or of the use of an object into its objective reaction. Motor apraxia therefore may originate from either an insufficiency of the directing idea, from derailment or irradiation of the directing idea upon the neighboring ideas, from the omission of portions of the motor act and the predominance of the last portion of the act or from the substitution of the directing idea by another idea.

All normal voluntary activities seem to be due to two

mechanisms—the representation in the sensory sphere of the successive series of partial acts which make up the entire act (the kinetic formula for movements) and the faculty of changing this sensory representation into external movements (the ability of exteriorization). The kinetic formulæ for movements are really the memories of successive movements and can be compared to certain chain or sequence reflexes, a point upon which I had previously insisted in my studies on amnesia. In motor apraxia, this kinetic formula is defective. Therefore it acts in an abnormal manner upon the innervation of the specific action, according to which of the kinetic elements is disturbed. Motor apraxia consists, therefore, in a rupture of the physiological connections between the innervation of a specified limb and the ideas concerned in the carrying out of a specified purpose. It is really a motor dissociation, in which special kinaesthetic memories lie outside of the field of general motor innervation. This isolation of the kinæsthetic memories is probably due to an isolation of a certain portion of the left hemisphere in which these motor memories are stored up, an isolation resulting in most cases, from a rupture in the conduction of the fibres of the corpus callosum. The centripetal stimuli reach the centrifugal pathway without entering the special ideational or motor centres by a kind of “short circuit.” This is pure motor apraxia; when in addition there exists a disorder of identification, we have motor apraxia plus asymboly or ideational apraxia (agnosia). In all cases of motor apraxia the chief difficulty seems to lie in the inability of the subject to transfer the directing ideas or rather the willing of the directing ideas to the motor sphere because they are partially cut off, isolated or derailed.

The behavior of apraxic patients is a subject of much interest. Movements of substitution which are so common in motor apraxia, may be compared to paraphasia. Occasionally the subject may begin a movement and only partly complete it. Here the reaction is referred to as curtailed. Sometimes a movement may bear no resemblance whatsoever either to the usual acts of everyday life or to a specially skilled reaction. Under these conditions we describe it as a formless or an amorphous reaction. On still other occasions the first part of the movement may be immediately followed by the last part without any intervening motions. This is called a short circuited reaction (“Kurzschluss Reaktion”). Perseveration is the monotonous automatic repetition of an act. Sometimes indeed the subject becomes petrified, as it were, in the attitude of executing a simple or a complex act either requested or spontaneous. To the first form is applied the term clonic perseveration; the latter is known

as tonic perseveration. Clonic perseveration strongly resembles the recurrent utterances of sensory aphasia, while tonic perseveration is analogous to the mutism of a severe motor aphasia.

Scheme of disturbances of kinetic formula for movement.

A → B → C → D	= Normal Motor Reaction.
A —————→ D	= Short Circuited Reaction.
A → B	= Curtailed Reaction.
A → A → A →	= Clonic Perseveration.
A	= Tonic Perseveration.

FIG. II.

A graphic analysis will make this clearer (see figure II). Let A. B. C. D. represent the kinetic formula for a certain movement, each letter indicating one element of the action. The normal movement could occur only when the individual elements acted serially. Any change in the position of the elements or any omission, would result in disordered activity or apraxia. For instance if the action took place as A-D. with the intervening elements omitted, we would have a short circuited reaction. If the movement was A—B. with the other two elements omitted, then the action ceases soon after it has been started; in other words, it has become curtailed. If the action should be A. A. A, the first portion of the action will then tend to be indefinitely repeated. A clonic perseveration could result. If the action was A. alone, and no further elements of the kinetic formula followed, the subject then would become stuck at the first of the series. Here we have a tonic perseveration. Many apraxic subjects show a marked lack of spontaneity, a condition which strongly resembles, if it is not identical with tonic perseveration. As a rule most requested or spontaneous movements in apraxic subjects are ill directed and fumbling, show a deficient adjustment to a purpose and yet they are not ataxic.

Von Monakow¹ has brought forth an ingenious theory to explain certain focal disturbances of the brain, particularly aphasia and apraxia. According to this theory, mere anatomical interruption of the continuity of fibres in the central nervous system will not fully explain the various form of focal disorders. Therefore there must be a special form of action at a distance from which may arise temporary or permanent suspensions of function. To this action at a distance, von Monakow applies the term "diaschisis." This diaschisis resembles certain physiological irradiations, in which a reflex effect can spread in various directions from a focus of reflex discharge.

¹ Von Monakow: Neurol. Centrblatt, Nov. 16, 1906.

The study and analysis of two cases of motor apraxia which came under personal observation will now be taken up. The difficulty of correlating the motor reactions with the mental state of the subject will excuse the lengthy details of the reports. In fact, in apraxia as well as in aphasia, a rigid examination scheme has but a limited value, as not only do the types and the conditions vary, but the same type may present different aspects in different subjects. Our present knowledge of apraxia is due entirely to minute clinical investigations, to which, when possible, the anatomical findings have been added.

Case I. When the patient K., who was twenty-four years of age, first came under observation he had been suffering from severe headaches for two months. On several occasions the headaches became so intense that vomiting followed. Up to this period the patient had been perfectly healthy. An examination disclosed the following condition. Only the essential neurological details are given. Tongue tremulous and protruded to the right, tremor of outstretched hands, the right knee jerk and right Achilles jerk were brisk; while the left knee jerk and the left Achilles jerk were absent. There was a subjective sense of weakness and numbness on the left side of the body. There was no ankle clonus or Babinski reflex. The pupils were equal and reacted promptly to light and accommodation; there was no paralysis of the ocular muscles and no nystagmus, but an ophthalmoscopic examination disclosed a blurring of the optic disc without swelling (beginning optic neuritis).

About three weeks later a sudden but transitory paralysis of the left arm and leg took place. Within a few days this improved until only a slight weakness could be detected. The physical condition has since remained the same with the exception of a blunted sensation in the left hand, associated with a weakness of grasp. Word deafness and motor aphasia were absent during the entire course of the disease. The patient was right-handed.

There was no hemianopsia and no unilateral mind blindness, because objects placed in each visual fields were promptly recognized. An analysis showed a typical motor apraxia of the left arm, while the right arm was entirely free from any motility disturbance. This unilateral apraxia was not dependent on any defective recognition of objects, because the patient could correctly select objects with either hand, but did not know how to correctly use them with the left hand after they had been selected. He was always oriented, and there was no disorder of memory either for recent or remote events. Unilateral agraphia was absent; he was able to write spontaneously and to dictation fairly well with either hand.

There was no apraxia of the left leg or of the muscles of facial expression for either side. This appearance of a motor apraxia in a limb from which a previous transitory paralysis had disappeared, resembles the observation of Goldstein, to which we referred above.

EXAMINATION FOR APRAXIA, MOTILITY AND SENSATION OF THE LEFT ARM

The general movements of the left arm were weak and fumbling. The dynamometer for the right hand registered seventy, while for the left hand it was twenty. The left arm was not paralyzed and all the movements could be fairly well performed but they were ill-directed and awkward. The abnormal motor reactions of the left arm on analysis seemed to be entirely due to a motor apraxia and not to any motor weakness or ataxia. With the eyes closed the movements of the left arm were decidedly more fumbling and ill-directed than when the eyes were open. In spite of the fumbling and slight weakness and spasticity of the left arm, he could spontaneously lift it above his head, flex, extend, pronate, supinate, fairly well extend and flex the fingers and give a fair grasp, although there was a disinclination on the part of the subject to spontaneously use this left arm. This tendency to akinesia was probably due partly to the patient's appreciation of his localized motor apraxia and partly to a state of tonic perseveration. An examination of the sensation of the left arm, forearm and hand showed anæsthesia and hypoalgesia.

The testing of the muscular sense of position showed that when the right forearm was placed at a right angle to the right arm, after some fumbling he was able to correctly imitate the position with the left arm. When the right arm was placed vertically above the head he promptly placed the left arm in the same position. With the eyes closed, when the right arm was passively elevated above the head and the patient was requested to imitate the position with the left arm, he merely placed the latter horizontally on the same level with the shoulder. Right arm passively extended horizontally forward with the palm upwards; he imitated it by placing the left arm in the same position but with the palm downwards. Right index finger placed at right angles; with the left hand he merely made a fist.

REACTION TO REQUESTS

	<i>R. Hand</i>	<i>L. Hand</i>
Buttoning coat	Correct	Grasps edge of coat
Touching tip of nose with right fore- finger	"	Puts palm to mouth
Touching right ear with right fore finger	"	Places finger back of head

	<i>R. Hand.</i>	<i>L. Hand.</i>
Movement of sewing	Correct	Fumbles
Movement of cutting with scissors	"	Fumbles
Snapping fingers	"	Makes fist
Turning hand-organ	"	Makes fist
Movement of turning key in lock	"	Makes fist
Movement of shaving	"	Places hand behind ear
Movement of combing hair	"	Rubs head with palm of hand
Movement of use of cork-screw	"	Fumbles
Military Salute	"	Fumbles

In the three successive reactions with the left hand in which the patient made a fist in response to different requests, we have an example of clonic perseveration or the frequent monotonous repetition of an act.

REACTION TO THE USE OF OBJECTS

	<i>R. Hand</i>	<i>L. Hand</i>
Comb	Correct	Rubs hair with smooth back of comb
Tooth brush	"	Correct but fumbles
Hair brush	"	Holds back of brush several inches above head without any brushing motion
Key	"	Cutting movements as though it were a knife
Match	"	Correct but fumbles
Spoon	"	Cutting and stabbing movements
Cigarette	"	Grasps it clumsily, puts it to chin and then puts wrong end in mouth

In the reaction to requests and to the use of objects the left-sided apraxia became more marked when the patient's eyes were closed. The same fact was noted in testing for the sense of position in the apraxic limb. For instance, in one series of tests a key, a match and a drinking glass were used correctly with the right hand and decidedly awkwardly and fumblingly with the left. When attempts were made to use these same objects with the eyes closed, the left hand seemed to become petrified, as it were, after an abortive start. In other words we seem to have here the phenomenon of tonic perseveration. On other occasions, even with the eyes open, he would start to use an object correctly with the left hand, and then would hopelessly fumble, either going into a condition of tonic perseveration or would finish up with a substituted movement, as if the object or the request had been changed. Sometimes, too, in the use of objects with the left hand, the movements would bear no relation to the nature of the object; they would become decidedly amorphous. The stereognostic sense was entirely lost on the left hand and to a certain extent, at least for larger and coarser objects, on the sole of the left foot. The increase of apraxia when the eyes were closed, was not due to any astereognosis, because the patient was always told the nature of the object. At no time

was there any loss of the knowledge of the use of objects. He was always able to describe their uses and from a number of objects spread before him he was able, on request, to pick out the correct one with either hand. The patient always knew when he used an object incorrectly with the left hand. The apraxia seemed therefore not dependent on any defective recognition of objects; in other words, it was not ideational but was almost entirely a motor disorder. His knowledge of the use of objects was always correct, even when he was not permitted to touch the objects.

In order to further show that there was no disorder of identification and that the inability to correctly use objects with the left hand was purely a motor disturbance, the following observations are of interest. When the patient was shown a match and requested to describe its use, he replied "To make fire." When asked to show the use of a match he did so correctly with the right hand. With the left hand, however, he clumsily grasped the entire match in his fist, leaving only a small portion of the head exposed and then made stabbing instead of scratching motions with it. When a cigarette was given to him, with the right hand he correctly placed it in his mouth. With the left hand he grasped the cigarette so clumsily in his fist that it was almost broken in half, then he placed it to his chin, then after some hesitation brought his fist to his mouth, still holding the cigarette tightly and with the mouth end completely covered up with his hand. When a match-box was placed in the left hand he promptly opened the cover, took out a match and showed correctly how to use it. When the match-box was placed in the right hand, attempts to repeat the performance with the left hand brought out a very typical motor apraxia. After considerable fumbling and perplexity he opened the cover, took out each match separately and allowed each to fall back into the box without any attempt at scratching them. When a pipe was placed in the right hand he promptly put the stem in his mouth. With the left hand, however, he took it by the bowl and placing the bowl to his chest said "I know it is a pipe, but I can't use it with this hand." It appears that these details point out the significant fact that the inability to use objects with the left hand was due not to any lack of knowledge or identification of the object, but to a derailing of a motor wish into a false motor reaction.

The imitation of movements also brought out a left-sided motor apraxia as follows:

	<i>R. Hand</i>	<i>L. Hand</i>
Saluting	Correct	Fumbles
Shaving	"	Merely places hand on cheek
Shining shoes	"	Grabs ankle

When requested to perform these same actions with the left arm and the eyes closed, fumbling and lack of direction in movements became greatly increased. It seems from these observations that visual impressions could partially correct the left-sided motor apraxia. When the visual stimuli were cut off by having the patient close his eyes, he went completely off the rails. The movements of the left hand without objects, were as apraxic as when objects were used.

In this case we are probably dealing with a brain tumor for which the following localizing diagnosis may be suggested. The tumor is probably in the right motor sub-cortex or centrum ovale, involving a portion of the fibres of the pyramidal tract and of the parietal region and probably a large portion of the fibres of the corpus callosum. A lesion here from its position could cause the motor disturbance of the left side of the body, that is, a transitory paralysis and weakness of the left arm and to a slighter extent of the left leg, and also a typical motor apraxia of the left arm. This latter is due to a loss of the guiding and directing influence of the right arm centre upon the left arm through a destruction of the fibres of the corpus callosum.

In the second observation we have the combination of an aphasic speech disturbance and a left-sided motor apraxia, both occurring in a right-handed subject. Such a combination is of importance, for it demonstrates that a left-sided brain lesion may be so situated as to cause an aphasia and a left-sided apraxia.

Case II. The patient L., 57 years of age, a right-handed man, began to suffer with a severe headache localized on the left side of the head, combined with dizziness and a weakness of the right arm. Shortly afterwards he suddenly began to repeat "oilcloth, oilcloth" spontaneously and in reply to all questions. This recurrent utterance lasted about an hour, after which speech became normal, although a little dysarthric. Somewhat later that same day he again suddenly began to talk in a jargon. In this jargon German sounds (his native language) predominated, while all knowledge of English (an acquired language) was completely lost. This condition lasted for four days and during this time the left-sided headache, dizziness, weakness and numbness of the right arm and leg continued although no actual paralysis was ever noticed. All requests and commands were understood. He knew the hours for meals and his various wants of his everyday life. At the end of four days he suddenly regained his normal speech in both languages. Since then when fatigued or frequently in the late afternoon he will forget the names of objects in English and will call them only by their German names. This is a

feature of interest which I had previously pointed out in the Lowell case of amnesia. It is indicated that the acquired memories in certain forms of amnesia were the first to disappear, while the deeper and more closely knit associations were preserved.

The neurological examination may be briefly summarized as follows: Marked arterio-sclerosis with a high blood pressure, speech occasionally dysarthric, no hemianopsia, grip of right hand decidedly weaker than that of left, no paralysis, pupils slightly unequal, the right reacting slightly to light, the left rigid, the right knee jerk diminished as compared to the left, no Babinski reflex, no facial paresis or deviation of the tongue. The gait was weak but not hemiplegic while the station was normal. There were no sensory disturbances in any of the extremities. All objects were quickly and correctly recognized and named and their uses accurately described. The amnesic aphasia was episodic and usually appeared only after the fatigue of a long examination or in the late afternoon. The patient was clearly oriented and showed no signs of intellectual defect.

An examination of the motility of the left hand showed that while it was slightly weaker than the right, it was free from paralysis, ataxia or tremor. All voluntary movements were present, yet it was possible to demonstrate a typical left-sided motor apraxia. The false motor reactions of the left hand were due to a motor apraxia and not to any inability to understand requests, because there was no intellectual defect and no word deafness. The facial muscles were free from apraxia. The inability to name objects, although he could indicate their uses with the right hand, took place only during the temporary amnesic aphasia due to fatigue. Repetition of printed letters was normal. Copying was correctly done with each hand. All objects were correctly recognized in each visual field and therefore there was no unilateral mind-blindness. Astreognosis was absent. The apraxia may be tabulated as follows:

REACTION TO REQUESTS

R. Hand *L. Hand*

Making a fist	Correct	Correct
Spreading the fingers	"	Shows palm of hand
Using cork-screw	"	Fumbles, first part of action correct, then makes cutting instead of pulling movements
Saluting	"	Correct
Smoking a cigar	"	Saluting movements

The attempt to show the use of a cork-screw with the left hand is an example of a curtailed reaction, while the persist-

ence of saluting movements to different requests indicates a clonic perseveration.

REACTION TO USE OF OBJECTS

	<i>R. Hand</i>	<i>L. Hand</i>
Scissors	Correct	Fumbles but correct
Key	"	Turns it upside down and holds it there
Match	"	Holds it like a pencil and makes writing motions
Pencil	"	Correct
Shoehorn	"	Rubs it against leg

A number of other objects were correctly used in the left hand but in a fumbling and awkward manner. The imitation of movements was also somewhat apraxic on the left. The imitations of movements of the right arm with the left arm led to some interesting reactions. When the patient was requested to make certain movements with the left arm alone, an apraxia always resulted, but this apraxia practically disappeared if the patient was allowed to imitate the movements of the right arm with his left. The apraxia to imaginary movements or in the use of objects was increased when the eyes were closed; or if apraxia was absent when the eyes were open, it tended to appear when the same tests were made with the eyes closed. These observations, demonstrated as in the previous case, that visual impressions are able to partially correct a motor apraxia in the same manner that a subject is less ataxic when the eyes are open.

For an anatomical localizing diagnosis we would suggest a probable area of softening at the angle of the third left frontal convolution and the Sylvian Fissure, extending below to the white matter of the corona radiata and to the radiations of the genu of the corpus callosum (forceps anterior). A lesion here would cause a motor aphasia and also a motor apraxia of the left arm, because the guiding influence of the right side of the brain upon the left side of the body would be cut off. The motor centre would thus become isolated. Here again we see the importance of the integrity of the callosal fibres from preventing any motility disturbance. The weakness and numbness of the right arm is probably due to either a backward extension of the lesion or to a pathological irradiation involving the anterior central convolution on the left.

Any analysis of these two cases demonstrated that the chief difficulty lay in an inability to transfer a subjective choice process into an objective reaction. The cause of this disorder could be easily traced to a definitely localized lesion in the brain, which disturbed the kinetic memories for move-

ments and produced new and abnormal combinations. The disordered movements and misuse of objects could be partially corrected through visual impressions, probably because these impressions may have stimulated certain non-affected portions of the brain, to function in a normal manner.

NOTE ON SOME OF THE PHYSICAL FACTORS
AFFECTING REACTION TIME, TOGETHER
WITH A DESCRIPTION OF A NEW
REACTION KEY

By FRANK ANGELL

The history of reaction-time investigations discloses a curious combination of painstaking accuracy in regard to the functioning of the time-measuring apparatus together with a more or less happy-go-lucky arrangement at the other end of the experiment.

Thus the chronoscope and its standardizing instruments have been the subjects of minute and laborious investigation, whilst the reagent, after assuming his 'convenient and comfortable position' has usually been left to his own devices in carrying out the reaction prescriptions. Whether however the reagent obeyed the directions, whether, for example, wrist and forearm movements did not enter into play where finger movements were prescribed, are matters which the experimenter has rarely been in a position to determine. Indeed it has only been of comparatively recent date that investigation has been directed to the initial pressure, the "antagonistic motion" of the break reaction.

Among other neglected factors in these experiments has been the effect of the tension of the reaction key spring. This has usually been set at a 'comfortable and convenient' resistance, and variations within these 'comfortable and convenient' limits have been regarded as negligible. This may be the case, but it is a minor question quite as well worth investigating as a variation of 2 or 3 sigma in the readings of the chronoscope.

The question was taken up as a "minor study" by two students in the advanced course in psychology—Miss Lanktree and Miss Morrison. The lever arm of a Morse key was set at tensions of 10, 20, 50, 100, and 200 grams respectively, and for each tension 10 series of 10 reactions each were taken. The first of these series does not enter into the averages given in Table I as the fall-hammer showed irregularities in the chronoscope at the time this series was taken. Each day the 5 series were run through twice—the second time reversing the order of the first, the order of the several series changing from day to day. The 15 volt current was furnished by a mercury rectifier and maintained at the same strength for

each day, usually at 0.47 amp. The mean variation of the hammer readings ran from one to three sigma—not very accurate but sufficiently so for the purposes of the experiment.¹ The stimulus came from a sound-hammer placed behind the reagent, who occupied a room adjacent to that of the experimenter. The reagents noted the condition of attention as good, moderate or strained (the last referring to the accompanying muscular tension) and in addition marked the reaction as sensory or muscular. Almost all of the reactions were characterized as sensory;—seemingly from strain sensations in the ear adapting it to the direction of the stimulus. L. had already been reagent in another investigation for more than a semester and both had been experimenting on reaction time in the second year's course of laboratory work.

Table I gives the results of the experimentation for the several tensions of the reaction key. It shows noticeable differences for all tensions in case of L. and a noticeable difference between the tension of 200 grams and the remainder of the series for M.

TABLE I

*Reaction times for different tensions of spring of telegraph key.
Reagents L. and M.*

Tension Grams	L		M	
	r. t.	m. v.	r. t.	m. v.
10	129.8	8.0	136.9	8.6
20	127.3	7.5	136.1	6.6
50	122.4	4.9	135.2	9.1
100	120.8	7.7	135.1	10.9
200	116.0	4.7	127.0	7.9

The introspections do not indicate any marked change in the attitude of the reagents for the stronger pressures of the key. Once M. notes a strain in the hand with 200 grams, but the reaction itself is noted as sensory, *i. e.*, the sensory content of consciousness at the moment of reaction was strain sensations in the head or ears directed toward the source of sound. The increase in tension, therefore, would not seem to result for the reagent in a direction of attention to the hand and a change to the muscular form of reaction. L. asserted that there

¹It is, perhaps, worth while to note that the chronoscope "newer Construction" used in this investigation, with only 4 years of use is a much less steady instrument than our old chronoscope; also "newer Construction" which has weathered 20 years of general laboratory service.

was no noticeable difference to her in the set of the hand for the 10, 20 and 50 grams of pressure, but that the transition from any of these to 100 grams and from 100 to 200 was marked. Nevertheless L's reactions show a steady decrease in quickness from 10 to 200 grams. The shortening of the reaction time with increase in tension of the key spring is, therefore, probably physical and due to an acceleration of the motion of the reacting finger imparted by the recoil of the spring. The tension at which the acceleration would be noticeable would depend on the manner of reaction: ten grams pressure might accelerate a finger reaction but not one from the wrist or elbow. As a result of long practice, L. had become skilled in the finger reaction. This is possibly the reason why, for this reagent, the effects of each tension were noticeable. A measurement of the rapidity of the free recoil of the spring for 10 grams of tension showed that a separation of the contacts of $\frac{1}{3}$ of a millimeter—more than sufficient to break the current—took place in 0.0005 sec. which of course is considerable faster than the reacting finger can move at the beginning of its course. This is to answer the possible objection that for the weaker tensions, the reacting finger moved up faster than the key-bar.

PART 2. EXPERIMENTS WITH TRIGGER REACTION KEY.

As is the case with much of its apparatus, experimental psychology found the ordinary telegraph key already in use and adopted it for its own purposes. The key is very convenient in manipulation and the motion it calls for is 'natural.' Serious objections to it are the antagonistic motion with the break reaction and variability of extent of the reaction motion. Another easy and natural motion is that of the finger in pulling a trigger, with the advantage of a very slight tendency, if any, towards the opposed reaction, though it may well permit an anticipatory contraction. The experiments presently to be described were carried out with a new key of the trigger type.

The elimination of the antagonistic reaction would, in itself, hardly be a sufficient reason for adding another instrument to the already long list of reaction keys. This trigger key, however, measures the force and extent of the reaction movement as well as its time. As is evident from the accompanying figure, the key is simple in construction. A cylindrical, self-registering spring balance is mounted horizontally. The movable end is provided with a ring for the reacting finger and in front of this stands an adjustable post serving as a brace for the hand. Electrical contact is made through the horizontal adjustable rod R. mounted parallel to the cylinder. This rod is connected with the binding-screw S¹¹, the other binding post S¹².

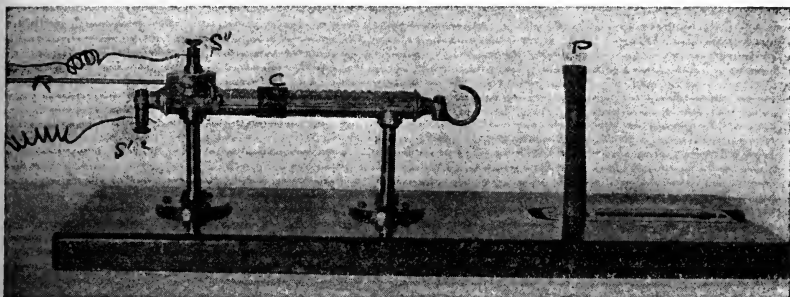


Fig. 1.

being connected with the index of the balance. In reacting, the reagent closes the hand around the post P.—adjusted to the proper distance from the ‘trigger’, and inserts the forefinger in the ring as far as the first joint. When the stimulus comes the reagent pulls the ‘trigger’, which breaks the contact between the rod R. and the scale index. The pull carries along the registering ring C. which is left in place at the forward end of the pull thus showing the force and extent of the reaction movement. If it is desired to change the initial tension of the spring, the rod R. can be pushed along the side of the scale and the spring set at any tension. In this way the influence of the initial tension on the reaction movement can be easily ascertained. Any anticipatory pull for 0 grams of tension is signaled by the failure of the chronoscope hands to move.

For the sake of comparison of the keys, pairs of series of reactions were taken with each of 10 reagents, each key being used in one series of a pair. The number of reactions in a series was 20, and one series in each pair was taken in halves with the other series sandwiched in between the halves to compensate possible practice effects. As the object of this experiment was merely to test the relative trustworthiness of the two keys, it will be sufficient to make a general statement of the results.

Although the reagents were inexperienced in reacting, the reaction times as well as the variations differed in no marked degree from those of experienced reagents with whom the processes of reaction had not passed over into the mechanical stage.

The figures showed the shorter reaction time for 7 of the 10 reagents with the telegraph key. If we were to ‘guess’ at the reason for this difference we should say it might in part, be due to the upward push of the key spring, and in part to the stronger tendency towards muscular reaction involved in

the reacting position for the telegraph key. With the trigger key, the hand and arm lie in a position producing less muscular strain than is the case with the Morse instrument, and no tension is required to hold in place the moving part of the instrument. Accordingly the content in consciousness of muscular strain is usually less for beginners with the trigger form of key and the reactions tend more to approach the sensory form.

The differences of proportional mean variation between the two keys were less marked than differences of reaction time and in general, so far as the data go, we should be inclined to say that taking merely the question of the time factor into account, the trigger key is as trustworthy an instrument as the old key.

FORCE AND EXTENT OF THE REACTION MOVEMENT

So far no attention has been paid to the other factors of the reaction given by the trigger key, *i. e.*, to the factors of force and extent of pull. The object so far has been merely to compare the time data of the instruments, and with such *Versuchstiere* as beginners in psychology there is every incentive to keep the conditions of investigation as simple as possible.

The experiment that follows is a preliminary survey with one reagent of all the data given by the trigger key—the force and extent of the reaction movement in relation to each other and to the reaction time. The reagent for this purpose was Miss Shumate who had done much reacting both in the regular course of laboratory work and in investigations. Before coming to this work and during it, she had been using the telegraph key for reacting to light stimuli with variable signal. Under these conditions her reactions were of the sensory type. She had also taken part in the comparison experiment along with the unpracticed reagents, but had reacted eight periods instead of one. The results for seven of these periods (omitting the first where the data are imperfect) were:

	Tel. Key	Trig. Key
Reaction time—median of 7 series	257	225
Av. of m. v. of 7 series	26	22

A number of series was next taken with the trigger key set at different tensions from 0 grams to 1,000 grams—the reagent noting each time the extent, and consequently the force, of the reaction pull. The instructions to S. were simply to mark the condition of the tension—classing it as “high,” “medium,” or “low” and to note extent of pull. Reactions

which the reagent classed as of "low attention" are not included in results. The interval between signal and stimulus was varied slightly to prevent reactions from becoming mechanical. In response to inquiry, the reagent said that her motions were not influenced through noting their extent and force; in the process of reaction she had in mind chiefly the time factor with no thought of making the pulls uniform in extent.

About 40 reactions were taken each period, distributed in four series corresponding to four different tensions of the key. Under these regular conditions, with but slight interruption for introspections, the effects of practice became very noticeable, so that at the end of a month the reagent's average reaction time had decreased 75 to 100 sigma. Table II gives the figures for all the reactions of this experiment. Taking the April result, where practice effects practically disappeared, we find with this key, too, an increase in reaction time with increasing tension of the spring and along with this a tendency towards a decreasing absolute mean variation. With the weaker tensions the reactions were fairly mechanical; the reaction motion followed the stimulus without a conscious will impulse. With the higher tensions this was less the case, but whether with 1,000 grams, for example, each reaction motion was preceded by a deliberate impulse, or whether a general state, or 'set' of preparation for stronger impulses preceded the entire series, we do not yet know. In order to get mental conditions that were as far as possible constant, the introspections were reduced to a minimum. The tendency towards a smaller *m. v.* with the stronger tension indicates however, that the latter condition was the case.

To what is the increase in *r. t.* with increased tension due:— to the greater time required for the greater impulse or to the greater resistance to the movement of reaction? The indications from the table are that the latter is the case. The columns headed at¹, ap¹, at², ap², give the averages of the three quickest and the three slowest reaction-times respectively, together with the averages of the length (and strength) of pulls corresponding to these reactions. The table shows no agreement for any given resistance between length of reaction time and strength of pull¹; of the 32 series tabulated, the quickest reactions give 16 longer and 14 shorter pulls than the slowest, while for 2 series within the limits of error of reading, the pulls are equal. In one of these series (10 reactions) the times ranged from 133 to 177 sigma, while the pulls were all of 1,775 grams; in the other, the time range was from 144 to 183,

¹Curiously enough Ach (*Willenstaetigkeit und Denken*, S. 158) assumes that the opposite is true: *i. e.*, he regards it as "sicher" that the quicker reaction follows the stronger impulse.

TABLE II. TRIGGER-KEY

Reaction data of *S* for different tensions of springat¹ means average of $\frac{1}{4}$ of shortest reactionsat² means average of $\frac{1}{4}$ of longest reactionsap¹ means average pulls for at¹ap² means average pulls for at².

Grams tension	No. react.	Date	av. r. t.	m.v.	at ¹	ap ¹	at ²	ap ²	av. pull. m. m.
0 grams	11	Apr. 20	139	10	123	608	153	650	33
0 "	10	" 22	135	18	125	812	179	787	42
0 "	10	" 25	143	11	129	767	158	742	41
0 "	9	" 27	132	9	116	800	145	782	41
0 "	11	" 29	154	24	124	717	169	733	38
av.			141	14					39
100 grams	9	Apr. 20	146	7	133	875	154	862	35
200 "	11	" 22	139	10	127	908	155	925	38
200 "	9	" 25	176	9	135	887	187	900	36
200 "	10	" 27	145	16	124	908	169	975	39
200 "	10	" 29	159	7	149	825	172	833	33
200 "	11	" 18	149	14	125	883	167	871	35
av.			152	10					36
300 grams	20	Mch. 2	190	17	167	955	207	960	34
300 "	17	" 14	172	9	162	955	185	860	32
400 grams	20	Mch. 16	185	15	163	970	209	1075	33
400 "	25	" 21	176	14	153	1075	211	1050	35
500 grams	19	Apr. 8	154	26	124	1070	198	1110	31
600 grams	10	Apr. 11	162	15	139	1200	184	1225	32
600 "	10	" 18	153	5	143	1250	160	1242	33
600 "	11	" 20	145	13	135	1167	173	1133	29
600 "	8	" 22	153	24	128	1275	184	1287	34
600 "	10	" 25	176	9	162	1208	189	1200	32
600 "	10	" 27	139	7	129	1283	147	1312	36
600 "	10	" 29	157	11	129	1175	147	1175	30
av.			155	12					32
800 grams	18	Mch. 23	177	24	148	1388	215	1360	30
800 "	20	Apr. 13	138	14	116	1370	161	1395	31
800 "	20	" 15	162	12	144	1435	185	1425	33
av.			159	17					31
1000 grams	10	Apr. 18	159	12	141	1567	171	1583	30
1000 "	11	" 20	163	14	145	1542	188	1525	28
1000 "	10	" 22	176	18	146	1566	203	1533	29
1000 "	9	" 25	169	8	158	1600	186	1550	29
1000 "	10	" 27	163	8	149	1600	173	1600	30
1000 "	10	" 29	171	9	157	1567	184	1575	30
av.			167	10					29

with a uniform pull of 1,600 grams. There is therefore here no proportion between the reaction-time and strength of motor impulse for any given resistance. In addition, the *m. v.* show no increase with increase in resistance; indeed there is, for this reagent, a slight tendency towards a decrease of this quantity. If the observed increase in reaction time with increased tension were due to increased time in the discharge of the motor impulse, there would probably be an increase in the resulting *m. v.*

The last column in Table II gives in millimeters the average distance pulled for the several tensions. The length of the reacting scale for 2,000 grams of weight is 105 millimeters. Consequently the distance pulled may be found from the formula $d = .0525 (l-t)$ where l is the strength of the pull and t the initial tension at which the index was set. For this table l is taken as the average of ap^1 and ap^2 which is very close to the average pull for any tension, and in some cases coincides with it. The table indicates a tendency to decrease the distance pulled with increase in initial tension t the falling off amounting to about one centimeter in going from 0 grams initial tension to 1,000 grams. We have accordingly with increased tension of pull an increased reaction time with a tendency towards smaller mean variation and a decrease in distance pulled. As this article is merely a preliminary survey of the field with one reagent, any attempt to evaluate the results would be premature. It is hoped that experiments with additional reagents supplementing those already performed, will show that the trigger key is helpful in the investigation of reaction processes.

PRECISION OF MEASUREMENTS APPLIED TO PSYCHOMETRIC FUNCTIONS

By F. H. SAFFORD

In three closely related articles,¹ Dr. F. M. Urban has treated Psychometric Functions by the aid of statistical methods. For convenience these articles will be referred to as *I, II, III*, respectively. The object of this paper is to discuss his use of these methods from the standpoint of physical measurements only, and not to enter into the psychological questions involved.

In considering the results of physical measurements it is necessary to keep in view the precision of the observations, the extent to which deductions may be carried, and the methods of computation which will give the results without unnecessary labor. It is of course useless to expect good results from insufficient data, and equally useless to sacrifice good data by an incomplete analysis.

In order to be able to discuss the three articles intelligently it is desirable to refer to several fundamental principles of computation.

In most cases the term "precision" should be restricted to fractional precision, *i. e.*, the ratio of the error of a quantity to the entire quantity. When a result of a measurement of any kind is stated as 25,306, it is understood to show that the result lies between 25,305.5 and 25,306.5, or that the value is known with an error of not over one unit in the last digit. The fractional precision in this case is thus, approximately, one part in 25,000. Had the original result been 2,530,600, the fractional precision would have been the same. The two ciphers at the end are "insignificant" digits, serving only to indicate the position of the decimal point. The cipher between 3 and 6 is a significant digit, and the same is true of the ciphers in such a result as .12500. If the last two digits are

¹I. The Application of Statistical Methods to the Problems of Psychophysics. The Psychological Clinic Press, Philadelphia, Pa., 1908.

II. Die psychophysischen Massmethoden als Grundlagen empirischer Messungen, Archiv f. d. ges. Psychologie, Vol. 15, Part 3 and 4, Leipzig, 1909.

III. Die psychophysischen Massmethoden als Grundlagen empirischer Messungen, Archiv f. d. ges. Psychologie, Vol. 16, Part 1 and 2, Leipzig, 1909.

written, they indicate, as before, that the result is between .124995 and .125005. When the result lies between .1245 and .1255 the correct form is .125. But in .0012500 the first ciphers are now "insignificant," serving as before to locate the decimal point. When a measurement is actually a counting of individuals, the last digit is not subject to an error in the sense used above. In general, the last digit of a measurement is liable to an error of one half-unit in that place, and to an average error of a quarter-unit. The precision of a measurement may be indicated for practical purposes by stating the number of significant digits, and it is not influenced by the position of the decimal point.

In the case of the four arithmetical processes the precision of the result is usually the same as that of the least precise element, a principle which may be deduced as follows. The product of .234 and 126.5 is 29.6010; but if the given numbers are measurements, they should be considered as $.234x$ and $126.5x$, where x indicates unknown digits. In adding the several partial products, each column containing an x must be rejected, leaving the result 29.6. This result is not as impressive as the former, but it is the only one justified by the data. The proof of the rule for division is similar. When a child is taught to annex ciphers to a dividend to facilitate division, he learns a rule which has no place in computation of physical measurements; and a division which is carried on after all the digits of the original dividend have been "brought down" presents a familiar exhibition of false accuracy. In addition and subtraction, the position of the decimal point affects the precision of the result, which is usually the precision of the numerically largest element, the use of an x at the end of each measured element affording a quick and reliable means of testing a result. When two elements in a subtraction are nearly equal, the result is often disappointingly inaccurate, so that an original precision of seven digits may be reduced even to one digit. Of course, logarithmic work is subject to similar criteria. The logarithm of 54.32 taken from a seven-place table may range from 1.7349198 to 1.7349997, so that 1.7349 or 1.7350 is as accurate as the given number will permit. If the given number were 54.32000, indicating a precision of seven digits, the seven place table would be properly chosen for use. Conversely, if the logarithm of a number is 2.34127, the number is anywhere from 219.4143 to 219.4194, *i. e.*, is correct to five digits only. Thus, in general, the number of places in the log. table should be that of the digits in the number. With few exceptions the precision of a result is not more than that of the data, and it is usually less.

It will be necessary later to employ the term average deviation of the mean. If the sum of n quantities is divided by n , the result is the arithmetic mean. The sum of the differences between the arithmetic mean and the quantities, taken without regard to sign, and divided by n , is the average deviation of a single observed quantity. If this deviation is divided by \sqrt{n} , the result is called the average deviation of the mean and gives a precision measure in common use by physicists. It is customary to compute this to two places of significant figures and then to retain in the arithmetic mean no digit beyond this second digit, since more than these are useless. The combined result is often written in the form $35.123 \pm .012$.

The experiments which were the basis of Dr. Urban's articles are described in *I*, page 1, and in *II*, page 261. A set of brass cylinders externally identical and of weights varying by four grams from 84 to 104 gms. was arranged at equidistant intervals determined by numbers from 1 to 14, around the circumference of a circular table. Standard weights of 100 gms. were placed at the odd numbers. The individual to be tested was requested to lift each weight in turn, and to give his judgment as to the relative weight of each cylinder at an even number and the standard cylinders at the preceding odd number. The table was rotated so as to bring the weights in succession under the hand of the observer, who stated his judgments at the rate of eleven and one-half per minute. Each experiment consisted of 50 comparisons of each weight with the standard, and the results of each experiment were separately tabulated, the judgments being classified as heavier, lighter or equal.

Seven observers were employed, of whom the first three performed nine experiments, and the others six. The "frequencies" for each of the three types of judgment were computed by dividing the total number of judgments of each type and for each weight by the total number of judgments in the nine or six experiments. Observer I° gave the judgment "equal" 28 times for the 84 gm. weight and 56 times for the 88 gm. weight. So that the two frequencies were $28 \div 450$ and $56 \div 450$ respectively. This process gave eventually a table of frequencies having seven entries, one for each weight. By interpolation, frequencies were found corresponding to weights varying by single grams from 84 to 108, but only the seven original entries were experimental results. After the plotting of these extended results for each observer and for each type of judgment, smooth curves were drawn through the twenty-five points of each plot. The curves for equality judgments were somewhat like the ordinary probability curve, while those

for lighter and heavier judgments were low and high respectively at the right ends and *vice versa* at the left ends.

The equality curve for observer I° was treated most elaborately and so will require the most attention in this paper. The original frequencies for observer I° (*I*, table 85) were:

.0622, .1244, .3311, .4422, .4644, .0911, .0533.

The number 28 above mentioned, which is the total for nine experiments, is the sum of widely varying components, viz., 4, 3, 4, 4, 2, 5, 2, 3, 1. If we follow the procedure previously explained and compute the average deviation of the mean, these values give $3.11 \pm .34$. In this manner the original frequencies revised and with useless digits omitted are:

.0622 \pm .0067, .124 \pm .019, .331 \pm .045, .442 \pm .025.

.464 \pm .045, .0911 \pm .0061, .0533 \pm .0099.

Thus a three place log. table is sufficiently accurate for the computation, though it must not be inferred that this means rough approximation; for, on the scale adopted by Dr. Urban, a change of a unit in the third decimal place of an ordinate is not visible to the naked eye. Granting the use of these ordinates to four digits and no further, it should be observed that these ordinates and the corresponding abscissae 84, 88, etc., give seven points and no more for the purpose of defining the psychometric curve of this observer.

At *I*, page 126, Dr. Urban proceeded by the use of Lagrange's formula to obtain the equation of this curve in Cartesian co-ordinates, since in that form the result is easily differentiated, thus enabling one to locate the maximum ordinate. In this computation the seven ordinates were treated as if exact to any desired extent, *i. e.*, in the divisions, zeros were added to the dividends giving some quotients to twenty-four significant figures. Such precision is equivalent to stating the volume of the earth with an error of not over one cubic inch. On an inserted sheet (*I*, p. 129), giving only a portion of the details, there are over fifteen hundred digits, and but for two accidents the equation of the psychometric curve would have coefficients correct to eighteen digits. One accident is that certain of the results are computed to only eleven digits; the other is an error in the computation of $a, \phi(x): (x-84) m_1$, where the coefficient of X^4 is given as .003035422098013889, when it should be .003035422092013889. This error gives the ordinate of the point for which $X=100$ the value .4650 instead of .4644 as in the data. Newton's method of differences gives a formula which is theoretically the same as that by Lagrange, and requires about one-tenth of the labor. While this is not in a form for easy differentiation, the method of approximation will quickly locate the maximum ordinate, giving its location far closer than the data will warrant.

Certain theoretical topics about this curve must now be treated. In *I*, page 139, this statement occurs. "Interpolation by Lagrange's formula has not the character of a definite hypothesis on the nature of the psychometric function but it is rather a method of completing a set of observations." Lagrange's formula gives the equation of a curve through n points, whose degree is not greater than n and the n points determine the curve completely. In fact, for any set of n points only one curve exists of the type which Lagrange's formula assumes. The use of Lagrange's formula certainly makes a definite assumption about the type of the psychometric function, which is all the more open to objection because all of the probability curves, both symmetrical and asymmetrical, are definitely excluded. An infinity of curves of the same general form, if desired, but of higher degree may be made to pass through any n points, so that it seems utterly useless to spend energy in locating within one one-thousandth of an inch the maximum point of a curve whose only claim for consideration is the fact that its equation is simpler than that of other curves. Circles were once considered the only perfect curves; hence it was argued that heavenly bodies must have circular orbits. As a last comment on the quotation above, if Lagrange's formula enables one to complete a set of observations, why not make fewer observations and use the formula instead? Thus from two observations one could obtain an indefinite number of new "observations," but unfortunately all would lie on a straight line through the two original points.

Without detracting from the very able mathematical treatment of the theoretical psychometric curves, it is important to notice that the observation curves in *I* and *II* show such divergence from the theoretical curves in *III* that deductions from the latter are not applicable to the former. But the former are not a necessary conclusion from the seven points. From a mathematical standpoint it is desirable to have more points for the curves, and these must not be obtained by any formula of interpolation, since this process introduces an assumption about the curves, in fact is equivalent to defining the curves completely. Under the conditions provided in these experiments the deductions may quite as well be obtained from the plots themselves, since the observations are not sufficiently precise to justify such exhaustive mathematical treatment.

THE PSYCHOLOGY OF DROWSINESS

AN INTROSPECTIVE AND ANALYTICAL STUDY

By H. L. HOLLINGWORTH, Barnard College, Columbia University

The investigation of sleep and dreams seems to the writer to have neglected to explore in an adequate way a region of normal conscious life which merits more attention. This region is the state of drowsiness, which usually precedes the sleep state, and which is especially prominent and long drawn out in conditions of over work or unduly protracted waking hours. Much, of course, is known of the dreamy mental states so clearly described by Crichton-Brown¹ and intimately related to the auræ which frequently precede epileptic seizures, and of the various disturbances of sensation and perception in neuraesthesia, psychæsthenia and the many pronounced types of alienation.² Something is known of the variously named hypnoid, hypnagogic or pre-sleeping state which is often found to precede the hypnotic trance, and of the dissociations found in hysteria. The "dreamy mental states" described by Crichton-Brown in his Cavendish lecture were such experiences as "double consciousness—loss of personal identity—a going back to childhood—vivid return of an old dream—losing touch with the world—deprivation of corporeal substance—loss of sense of proportion,—momentary black despair—being at the Day of Judgment," etc. And they are asserted to be "abnormal in their essence and morbid in their tendencies."

But references are few in the literature to the suggestive and quite common hallucinations and perceptual complications experienced by supposedly normal people in the state of drowsiness, and such search as the writer has been able to make has disclosed no careful description or analysis of this state. Prince³ has recently asserted that the pre-sleeping state has "certain marked characteristics which distinguish it from the alert state of waking life and is worthy of study in itself." M. Maury⁴ in the report of his experiments on dream

¹Dreamy Mental States—The Lancet, July 6, 13, 1895; Nos. 3749-50.

²Paton: Psychiatry, pp. 26-127.

³Mechanism and Interpretation of Dreams. Jour. Ab. Psy., 1910, p. 139.

⁴Le Sommeil et les Rêves. p. 42, etc.

production describes the so-called "hypnagogic hallucination" which he regarded as constituting "the chaos out of which the dream cosmos is evolved," and Herschel¹ has described "sensorial visions" which occur during the waking state, but these seem to be merely the familiar entoptic phenomena of waking life. Sully² calls attention to the presence of transition states between sleeping and waking and to the comparative ease with which sense illusions occur in these states. These seem to be the "hypnagogic states" of Maury, "states of somnolence or sleepiness in which external impressions cease to act, the internal attention is relaxed, and the wierd imagery of sleep begins to unfold itself." But Sully's chief emphasis is on the persistence of the dream hallucination proper into the postsomnial condition.

Conceivably the state of drowsiness might throw considerable light on dream formation, the relation between the latent and the manifest content of dreams, and the various ways in which external impressions and central dispositions are transformed and related in the serial dream. Drowsiness is the transition state between waking consciousness and dream life, and careful observation of this state should be able to catch dreams in the making and to disclose the tendencies which attain their maximal operation in the sleep state proper. Whether or not it be true that the genuine dream is experienced only in moments of awaking from or falling into the sleep state is immaterial. The dream as a more or less systematic articulation and fabrication is quite distinguishable from the unique fusions which come in moments of drowsiness. In the experience of the writer and his observers these latter are more often momentary perceptual states, flashlights of imagery of unwonted vividness which may as such be repeated in successive moments but which do not tend to lead on to new situations as do dream states. And yet these perceptual fusions show striking points of similarity in their composition to the various units of the serial dream.

The purpose of this paper is to describe several typical cases of the drowsiness hallucination and to analyze out some of the principles which clearly contribute to their formation. Two observers, the writer (H) and his wife (L), have for the last two years been recording these experiences, with the result that an accumulation of cases has been acquired which seem to show sufficiently pronounced characteristics and similarities to make their discussion worth while. The observations have invariably been made in the pre-sleeping

¹"On Sensorial Visions" in Popular Lectures on Scientific Subjects.

²Illusions, p. 184.

state, for neither of the observers has the drowsiness hallucination in any marked degree in the pre-waking state. In all cases the observer has been aware of the hallucinatory character of the experience, and has immediately written out the description or narrated it to the other observer.

The imagery type of the observer, as will be shown later, seems to be to some extent, a determining factor in the composition of the hallucination content, in that modes which are only vague and seldom used in the waking state become vivid and active in the drowsy condition, while the type modes fall into the background. It will then be of interest to know that according to frequent imagery tests L is predominately visual and visual-verbal in type with almost no auditory or motor tendencies, while H is very highly auditor-motor, both as to imagery and memory type, has frequently had auditory and motor hallucinations, especially marked in childhood, but is a very poor visualizer. Typical cases are given in the following paragraphs, and in the subsequent analysis these cases will be referred to by number.

Case I. Observer H. On board ocean-liner, dressing for dinner in suit purchased abroad, sitting drowsily on edge of berth and thinking that the suit had turned out to be a bad investment and had been forced on to me by a tricky salesman. Planning to buy cloth this time to be made up in United States and wondering if it would pass Customs. Suddenly the rush of water heard through the porthole becomes transformed into the husky voice of a salesman trying to sell me a suit. I fall to musing in the process, wondering, while he talks, at his husky voice and why he has no more inflection.

Case II. Observer H. At Victor Herbert's opening concert, 1909, L—, E—, and myself were talking of the cartoons of Mr. Bug in "Life." L— described a cartoon in which the six legs of Deacon Fire-fly were represented as grasping different objects such as a Bible, a prayer book, etc., while Mr. Bug held playing cards, a bottle, a cigar, etc. I had been working all day on comparative nervous anatomy, preparing a lecture on complications of stimulus and response, and had my head full of segments and nervous arcs. The orchestra played Grieg's "Wedding Day at Hegstad." In the last bar there were three finishing blasts with full orchestra. I had become very drowsy and these blasts seemed to me to be movements of some huge bug which came sailing from behind the wings, suddenly alighting on the stage, first on the two hind feet, then bringing down the middle pair, and finally the two front feet with the final blast. The visual elements present were of huge, vague, rather reddish brown jointed legs, the feet not clear and only the lower ventral side

of the body dimly suggested, but flashing out at each "land" of the feet.

Case III. Observer H. In bed, winter '09, with "grippe." Kept tossing from side to back, then to other side. As I tossed the numbers 50, 2, 36 kept running in my head, appearing clearly visually as 5236, and auditorially as "fifty two—thirty six." Now these (50, 2, 36) were the combination numbers of my gym locker, which I opened by turning the knob left-right-left-right, four turns, very much as I now tossed in bed. In my tossing the numbers rang and rang in my head, the left side seeming 52, the right side 36, the back 5236. It seemed that if I could juggle these numbers into the right combination I could find a comfortable position.

Case IV. Observer L. Tossing experience similar to above, but seemed to be going to Brooklyn and back, Mrs. M.—who lives there having lately been uppermost in mind through conversation, letters and a recent visit.

Case V. Observer H. Played checkers nearly all day on steamer. Retiring to cabin before sleeping time, threw myself drowsily on my bunk and fell to ruminating over some projected experiments on the comic, wondering whether to follow method of order of merit or that of assigning numerical grade to each comic situation. I decide, but in my half awake consciousness the decision takes the form of a move in checkers. I decide to move my white man up to the king row and mentally see C—jump it with his black.

Case VI. Observer H. Lying in bed talking. In a pause I see a large marble toad-stool which seems to stand on a hill and to resemble the dome of the New York University Library, around which runs the Hall of Fame. On top of the toad-stool bell were stamped in large black letters, three names, "Jastrow," "Gillis," and another blurred one which I could not make out. At once I told my companion of the vision, saying "I see a curious hall of fame," etc. That evening I had read some comments on Jastrow's magazine article on "Malicious Animal Magnetism" and had also seen in a comic paper a picture called "The Annual Ball of the Mushrooms." Psychoanalysis threw no light on the name "Gillis" nor on the blurred name.

Case VII. Observer L. Had a bad toothache, and though very sleepy and worn out could not sink into a sound slumber because of the pain. For several hours I lay in a state of semi-consciousness, tossing from side to side in a drowsy effort to find a comfortable position. All day I had been very intently working on a coat which I was making, and my tossings back and forth were all in terms of the seams on the coat, *i. e.*, as I turned to the right the seam down the

right side of the garment was inspected, then as that position gradually grew unbearable the seam began to wrinkle, to pucker, and to become quite unmanageable. Thereupon I decided to work awhile on the other seam, and turned to the left side and carefully basted and pressed the seam on the left side of the coat. But, though it behaved very satisfactorily for a time, it too, soon began to wrinkle and the thread to snarl. In despair I attacked the seam on the right side again, that is, I turned over to my right side once more. This continued for an indefinite time. I was in despair. I feared the garment would be quite ruined. All through these hours I was conscious of the slight flapping of the window blind and twice I replied quite sensibly to the questions of my companion, and noted the striking of the hours on a clock in another apartment. But the illusion that I was wrestling with the seams of a refractory garment was not dispelled till I fell asleep at daylight.

Case VIII. Conversations during the drowsy state.

(a) L— Let's hurry and get there by ten o'clock.

H— That's easy. I could get there by a nickel to ten.

(It was then 9.50)

(b) H— (As L rises from the sofa where she has been resting and leaves the room) So you want some water, huh?

L—(Entering again) What did you ask about a drink?

H—Nothing.

L—But you asked me something about water.

H—(drowsily) It was n't an asker, it was just a sayer.

(c) H is said to behave when drowsy much like a child or like a half-intoxicated man,—thus: he approaches an old pillow spotted with ink blots and asserts in a child-like way quite without provocation or connection,—“Red ink! black ink!” pointing to the spots meanwhile.

(d) L asks question to which H replies, quite without relevance, “I don't think you could see the manuscript.”

(e) L— “How curious the moon looks behind the clouds!”

H—Yes, just like a thin place in the sky.”

Case IX. The writer has frequently recorded fantastic experiments and conclusions developed either alone or during discussions, late at night. At the time of their conception all of these plans and insights seemed highly rational, strikingly original and wonderfully significant, and the observer has usually marvelled that nobody had ever seen the thing so clearly before. He has frequently gone ahead after the midnight hours and prepared the material for one of the revolutionary experiments or demonstrations just conceived. But when the plan or conclusion has been gone over on the following morning the most striking thing about it has been its splendor

as a work of unbridled imagination, but its absurdity as a scientific achievement. The argument is found to abound with fallacies or the experimental procedure with sources of error that were lightly bridged over the night before. The experience is no doubt a very common one. It seems much like the nocturnal aeronautic inspection of a line of march which must be gone over on foot when daylight comes.

Case X. The reason for citing the two following literary references will be seen later. They are typical drowsiness figures.

(a) This passage from Stevenson's letters to Henry James (Stevenson, Letters, p. 435) must have been written late at night and in a state of drowsiness. In fact Stevenson says earlier in the letter "my wife being at a concert and a story being done," indicating a late hour, and (especially in Stevenson's case) fatigue. Speaking of the "Henry James chair" the writer of the letter says, "It has been consecrated to guests by your approval and now stands at my elbow gaping. We have a new room, too, to introduce to you—our last baby, the drawing room; it never cries and has cut its teeth. Likewise there is a cat now."

(b) De Balzac, who wrote the following passage must have been a night worker,—“He saw his teeth departing one by one like brilliantly dressed ladies from a ball room.”

The experiences here recorded can hardly be classed as dream states though it is true that only a little elaboration would be needed to make them develop into such states. They all occurred during waking moments, and frequently (See Cases 2, 5, 7) there is clear evidence that the observer is actively engaged in some waking employment or lively thought process or is conscious of external events. Yet most of them are hallucinatory in character. Examination of such cases as those given above reveals several rather clearly defined principles of composition or general tendency. Chief among these are the following, the exposition of which seems to constitute a fairly true, though perhaps, only partially complete analysis of the state of drowsiness. Other experiences of much the same kind could be given, some of which are withheld only because of their close personal character, but all point in the same direction as the cases here given, some of them even more definitely.

I. TRANSFORMATION OF IMAGERY TYPE

Modes ordinarily vague and feeble become here dominant and vivid, even tending to replace customary imagery habits. Thus H, who is predominantly auditory and motor in type and can only with difficulty summon up visual images of even

the most moderate vividness, has, in the drowsy state, visual experiences which constantly amaze him by their clearness. (See Cases 2, 3, 5, 6.) L, to whom sharp visual imagery is a common habit, but who, in her waking consciousness cannot understand what kinesthetic imagery is like, tends, in the drowsy state, to relive motor experiences almost exclusively. (See Cases 4 and 7). Along with this emphasis of unusual modes goes the subordination of dominant modes, so that in the drowsy state as in dream life, images even of these unusual types seem to exceed by far in intensity the clearest images of the waking state.

It seems to be generally true that with increased age, increased book learning and, in general, with practice in verbal modes of thinking, sense imagery gives way to word imagery of one kind and another.¹ Parallelling this fact, many people have complained to the writer that with maturity they lose their long drawn out delight in books, and especially in descriptive literature. They tend more and more merely to scan such passages and hence to read books much more quickly. They may regret the loss of the old source of satisfaction, the character of which they do not understand. Evidently what happens is that sense imagery is waning and description no longer has its old power of awakening interest or calling forth emotion. In drowsiness this state tends to disappear. The dominant modes in which one has become accustomed to think in the more rigorous sense, seem to tend toward sleep more quickly, while the lower, more strictly sensory centres remain active or go to sleep more slowly.

(*Note.* Since the preceding paragraph was written the writer, in re-reading Professor Titchener's striking analysis² of his own imagery processes, has found what seems to be another clear instance of the transformation of imagery type. Although this observer says it is possible for him to "trust to the guidance of kinæsthesis," this mode of imagery does not seem to predominate in his daily life. The following statements show the apparent superiority of his visual and auditory imagery. "I rely in my thinking upon visual imagery . . ." etc. "My visual imagery, voluntarily aroused, is extremely vivid." ". . . visual imagery which is always at my disposal and which I can mould and direct at will." But he also has "vivid and persistent auditory imagery" and never sits down to work without a "musical accompaniment." Kinæsthesis seems to play a rather sub-

¹Galton: *Inquiries into Human Faculty*, p. 60.

²Experimental Study of the Thought Processes, 1909, p. 9.

ordinate rôle, although we do find the remark, "As a rule I look to all three kinds of prompting in the course of a single hour." But, and this is the interesting point in the present connection, ". . . *when I am tired* (italics mine) I find that vision and audition are likely to lapse, and I am left alone with kinæsthesia.")

In this state the condition of early childhood is reproduced and sense imagery may become vivid, intense and grotesque. This tendency, along with the absence of sensory stimulation probably accounts as well for the greater vividness of images in dreams. The frequency and character of dream imagery has sometimes been taken as an index of the type habits of waking life, but the transformation tendency shown in the observations here presented seems to show clearly that this is not the case on the state of drowsiness. The difference may, perhaps, be explained by supposing that in sleep all the centres are more nearly equally quiescent, while in drowsiness the type centres slumber first, thus giving prominence to modes not usually relied on.

2. SUBSTITUTION

Within the content of the drowsiness fusion it will be seen that a present impression, a preservative tendency or, perhaps, even a pure memory element often substitutes itself for some other datum whose rôle it fills in the perceived composition of the hallucination. Illustrations of these forms of substitution are afforded by the cases here reported.

(a) Sensory Substitution. Here a present sensory impression takes upon itself the task of impersonating more ideal or memory contents, of bearing their qualities, carrying out their behavior and in a general way acting for them. Thus in Case I the sound of the waves washing against the sides of the boat assumes the rôle of the foreign salesman, becomes his voice and seems to constitute his conversation. In Cases 3, 4, and 7 present motor processes (tossings, turnings and other changes of position) become the vehicle on which are borne memory experiences of a day or two before.

(b) Perseverative Substitution. Case V in which the thinking out of the technique of an experimental problem seemed to be carried on in terms of the manipulation of the white men on a checker board, affords an excellent illustration of the tendency of perseverative impressions to play the rôle of other data.

(c) Ideal Substitution,—in which an ordinarily revived image becomes the substantive for experiences more or less remote or assists in the interpretation of a present impression is rather difficult to demonstrate for two reasons. In the first

place it is not easy to draw the line between perseverative impressions and supposedly revived images. Thus in Case I the huge bug which was conceived as alighting on the platform in order to apperceive the three successive orchestra blasts, was evidently a pure object of "creative imagination," for no previous impression had corresponded to a creature of such dimensions. Yet the character of this imagery content was probably determined by certain perseverative tendencies arising from the prolonged consideration of the various types of nervous anatomy. In the second place, substitutions of the sensory and perseverative type usually involve a correlative displacement of the ideal content for which they act, and in setting up ideal substitution as a third type, on the basis of the data at hand, one is perhaps, merely paraphrasing what he has already said.

At any rate it is clear that interchange of ideal with both sensory and perseverative content and interchange of sensory with perseverative content occurs. Whether one pure ideal datum may act as substitute for another, future observation may show. DeBalzac's simile (x, b) which sounds much like the conversation of a drowsy man, seems to be a case of such substitution. More will be said of such literary figures in the following paragraphs.

3. FLUID ASSOCIATION ON A SENSORY BASIS

with removal of constraining mental sets and controls. This leads to bizarre analogies, naïve statements and unusual verbal combinations. In this respect the state of drowsiness seems to be quite like genuine dream consciousness, in which such free association tendencies are so pronounced. Thus De Manacéine¹ points out "the tendency which compels us during sleep and during enfeebled states of consciousness generally to associate everything which presents some common resemblance, for example—words according to their sound, and images according to some accidental and external resemblance. The same tendency is observed in the uneducated and very markedly in the insane. . . Any resemblance in color or form is enough to associate images which are altogether (1) heterogeneous." Again, "There is a well known tendency in dreams for the perpetration of bad puns, sound leading sense, as happens frequently with the insane, idiots and young children." Prince² in referring incidentally to the characteristics of the pre-sleeping state has written, "ideas course through the mind in what appears to be a disconnected fashion, although probably determined by associations.

¹Sleep, p. 283 ff.

²*Op. cit.*

Memories of the preceding day and of past thoughts which express the interests, desires, fears and anxieties of the psychological life and attitudes of the individual float in a stream through the mind like a phantasmagoria."

Examples a, b, c, d, and e under Case VIII afford concrete illustrations of such uncontrolled accidental association. Formal, practical and conceptual constraints being removed, resemblances of a sensory and ordinarily unnoticed kind, which seem to involve only lower nervous centres become predominant, verbal plays (b), naïve confusion of related concepts (a), absurd juxtapositions (d) and attention to irrelevant details (c) abound in the state of drowsiness. Not infrequently similes and affective chords are hit upon which with only a little treatment would become adequate literary figures (e).

Indeed, many of the choicest paragraphs to be found in the works of imaginative writers bear all the birth marks of a drowsiness conception. The illustration from Stevenson's letters (10a) is clearly a case of unusually vivid imagery, sensory substitution and uncontrolled association. In this connection Marsh's study¹ of the favorite work hours of 160 eminent writers is extremely interesting. Some of Marsh's conclusions are as follows:

"If the poets and novelists are roughly designated as an imaginative class and the historians, clergymen, essayists, critics, journalists, philosophers, etc., as a broader intellectual class we shall find the former predominant in the morning and night groups and the latter in the day ones;" ". . . of the after midnight workers all are of the imaginative type;" ". . . excitation of some sort is most often the precondition of the highest imaginative work." ". . . numerous and well-patronized methods of mental stimulation—from ordinary walking, riding or music to hourly service of blackest coffee, greenest tea or strongest opium or to constant use of tobacco, before and during composition. The extensiveness of this practice among the imaginative writers is striking."

One might mention in the same connection the fact that Mark Twain is said to have done most of his writing in bed, if not while actually sleepy at least in the sleeping posture, and the further fact that certain favorite poets have produced nothing of note since they were induced to sign the temperance pledge. The use of drugs and the preference for night hours both point in the same direction. It seems to have been shown plainly that the apparent stimulating effect

¹Diurnal Course of Efficiency, Archives of Psychol. No. 7, 1906, pp. 59-69.

of such drugs as are used depends on the fact that they narcotize the higher centres, on the functioning of which depend our control processes and constraining mental sets. And much the same condition seems to be the cause both of the involved serial dream states and of the vivid perceptual drowsiness complications. The similarity of these states to the oft described alienation psychoses will at once remind the reader of Nordau's fervent chapter¹ on Mysticism.

The artist and the poet must in some way get out of the world of percepts and into the world of pure sensory qualities. And this is not an easy thing for most of us to do. Most of us were there when we were children and the most prosaic of us tend to slip over the frontier in the pre-sleeping state or when under the influence of artificially induced drowsiness. A very few of us are vagabonds enough to be able to wander back and forth at will, and these are the artists and poets.

4. ISOLATION OF ASSOCIATION TRAINS

This characteristic of drowsy states is closely related to that described in the foregoing section. The difference lies in the fact that there we were dealing with single perceptual or ideational contents while here we have to do with such serial chains of associations as may sometimes be set up. In the drowsy state proper, in the experience of the writer, these chains do not develop,—the genuine drowsiness complication being either a simple "flashlight" hallucination or else a sort of "boomerang" composition, tending to return always upon itself rather than to lead on to further and new associations. But such experiences as those described under Case IX seem to belong to much the same state. These fantastic thought systems evolve most easily in times of fatigue, loss of sleep or unduly prolonged intellectual work. When the drowsy state is thus extended over a long period of time, association chains and reasoning show much the same behavior that perceptual or ideational states do in the drowsiness state proper. The essential thing is the release of all intellectual inhibition. An idea, plan or desire is thus able to make unimpeded progress from stage to stage of its development with what seems at the time to be unerring logic. Its evolution is accompanied by the strong emotion and the feelings of exuberance, bouyancy, confidence and eager enthusiasm characteristic of the night worker. In my own case the feverish plans, insights and conclusions developed in midnight hours have almost invariably faded into pale grays on the arrival of the next "waking consciousness"

¹ Degeneration, Ch. I.

much as did Maeterlinck's "Bluebird" when brought into the sunlight.

The drowsiness thought process behaves much as do the familiar dream states in which cosmic riddles are solved and impossible mechanical devices evolved. One recalls in this connection the oft-told case cited by Crichton-Brown of the man who determined to write out the solution arrived at in order to preserve it from the amnesia which usually developed on awaking. When morning came he looked eagerly for the paper on which he had written during the night and read there only the single mystic sentence, "A strong smell of turpentine pervades the whole."

Rivers and Weber¹ have shown that mental fatigue, anæsthetization of the muscle involved, or small doses of alcohol may have the same effect, viz.: a momentary falling off of fatigue due to disregard of secondary afferent impulses which are the basis of the fatigue feeling. Much the same situation seems to be present in the drowsy state and the disregard of obstacles and treacherous points in the chain of reasoning is probably due to quiescence of the higher centres which control both motor output and processes of inference. Such a condition is reconcilable with any of the current theories of sleep and with most theories of epilepsy, to the intellectual auræ of which the drowsiness hallucinations seem to bear a close resemblance.

5. GRANDEUR AND VASTNESS

Closely connected with the transformation of imagery type and the isolation of association chains is the tendency toward grandeur and vastness which usually characterizes the drowsy states. This is true of the simpler perceptual complications as well as of the further developed thought processes. Thus in Case II the idea of a gigantic insect, and in Cases 3, 4, and 7, the interpretation of limited motor processes in terms of long journeys or of complicated activities such as dress-making and opening combination locks, and in Case I the personification of monotonous noises, show the tendency to magnify simply sensory impressions. In another case, not recorded here, a space of perhaps three feet was taken to represent the ocean.

When a chain of reasoning is involved, all projects are fertile and all outcomes expansive. The common tendency for the disagreeable, the undesirable and the unfavorable fact to oblivesce seems here to be especially strong. The drowsiness experience, in the case of the present observers at least,

¹On the Effect of Small Doses of Alcohol. *British Journal of Psychology*, Jan., 1908.

resembles that following upon the inhalation of diluted nitrous oxide gas,—“the mental symptoms consist in convictions of emancipation, relief and happiness, in grand and “sublime ideas which in their expansion seem to break down all barriers of doubt and difficulty and to make a wish and its realization one. . . . It is at the point where the habitual control or check of the highest centres is withdrawn and where subordinate centres are free to indulge in unwonted activity that the expansive dreamy thoughts and exalted feelings present themselves in the progress of nitrous oxide gas intoxication.”¹

6. AMNESIA FOR PROCESSES AND EVENTS

occurring during the drowsy state comes quickly. This is shown by the tendencies of these experiences to escape observation unless special interest directs attention to them. Further, unless they are recorded or reported promptly they are soon forgotten or elaborated by the retrospective attempts of waking consciousness.

7. ABSENCE OF SYMBOLISM

So far as the writer has been able to discover there is no evidence of special symbolism in these states except in so far as they reflect the recent experiences or occupations of the individual. The composition of their content seems to consist chiefly in “flashlight” perceptual complications of the memories of recent experiences with perseverative tendencies and present sensory impressions. Only in so far as the data from these three sources is somewhat dependent on the fundamental interests of the observer can the drowsiness psychosis be said to be symbolical.

(*Note*). Sidis (Experimental Study of Sleep) has given “extreme suggestibility” as one mark of the hypnoidal state. This state bears a close resemblance to the condition of normal drowsiness, and may, perhaps, be identical with it. But suggestibility seems to be a general statement of the possibilities of the pre-sleeping state, rather than an introspective description of the drowsiness consciousness.)

By way of summary we may say, finally, that the drowsiness hallucination seems to be a “flashlight” perceptual fusion or complication, and is further characterized by transformation of imagery type; sensory, perseverative and ideal substitution; fluid association chiefly on a sensory basis; and by isolation of association trains when they develop; and that it is accompanied by tendencies toward grandeur and vastness, by rapidly developed amnesia and by absence of symbolism.

¹Crichton-Brown: *op. cit.*

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XIV. AN EFFECT OF FATIGUE ON JUDGMENTS OF THE AFFECTIVE VALUE OF COLORS

By ETHEL L. NORRIS, ALICE G. TWISS, and M. F. WASHBURN

A state of fatigue may naturally be expected to lessen the pleasantness of pleasant experiences and to increase the unpleasantness of unpleasant experiences. The present study is an attempt to get experimental confirmation, within a certain very limited sphere, of this conclusion drawn from general experience. As the source of pleasant and unpleasant affection we chose colored papers. With regard to the source of fatigue, evidently a number of possibilities were open: we might have used some form of physical fatigue, but we chose mental fatigue instead. Here, again, we might have produced fatigue in our observers by means of some kind of mental work, such as mental arithmetic, quite different from the work of judging the affective values of colors. We undertook, however, the problem of finding how far judgments of the affective value of colors are influenced when the observer is required to perform a long series of such judgments. That is, the fatigue was produced by the same kind of mental process as that upon which it was supposed to act.

Our method was as follows: A piece 2.9 cm. square was cut from each of the ninety colored papers in the Bradley series, comprising eighteen saturated colors, namely, red violet, violet, blue violet, violet blue, blue, green blue, blue green, green, yellow green, green yellow, yellow, orange yellow, yellow orange, orange, red orange, orange red, red, violet red; together with two shades and two tints of each color. Each piece of paper was placed on a white background before the observer, who was required to look at it for ten seconds and to record her judgment of its affective value in numerical terms, using the numbers from 1 to 7 to designate respectively the following degrees: very unpleasant, moderately unpleasant, slightly unpleasant, indifferent, slightly pleasant, moderately pleasant, very pleasant. The colors were shown in wholly irregular order. After judgment had been passed upon the whole ninety, without any pause in the operations the observer was given in succession the first six colors of the experimental series, in their original order, and required to record anew her judgment of their pleasantness or unpleasantness. In no case did the observer report remembering what her previous judgment had been. The whole proceeding took from three-quarters of an hour to an hour. The effect of fatigue upon the affective tone of the six colors selected to begin the series was then calculated as follows. When the number assigned to a color at the end of the series differed from the number assigned to the same color at the beginning, the amount of the difference together with its sign, *i. e.*, whether it was an increase or a decrease, was noted, and these differences were averaged, regard being paid to signs. Thus for one observer the affective value of one of the six colors dropped two numbers, that of another color dropped one number, that of a third rose one number, and that of three colors showed no change. The total change for all the colors tested was then $1-3$, or -2 ; and dividing this number by 6, the number of colors tested, we find the average fall in affective value to be .3.

There were thirty-five observers, all women and all but four college students. Out of these, the averages of seven showed a rise instead of a fall in the affective value of the colors at the end of the series, and for three the affective values were exactly the same at the beginning and at the end. Twenty-three observers show an average drop of from .1 to 1.5. In the case of three observers every change in the affective value of a color was an increase: in the case of ten, every change was a decrease.

It is unnecessary to point out that we cannot be sure of producing uniform degrees of fatigue by this method. Aside from individual differences in physical condition and previous fatigue, the process of judging required is undoubtedly more fatiguing to some people than to others. Thus one of the three observers for whom the affective values were greater at the end than at the beginning of the series was an artist, to whom the colors had probably more interest than to the other observers. Again, in the case of those observers who were acquainted with the method the knowledge that the end of the series was approaching produced a cheering up which might have been expected to counterbalance fatigue; although this did not prove to be the case with two out of the three authors of this study. When a drop in the affective value of a color does appear at the end of the series, we have no assurance that it is produced by fatigue; but since the other sources of variation might be expected to produce a rise as often as a drop, the results do indicate that *for sixty-five per cent. of our observers fatigue was the prevailing source of change.*

Two further facts may be noted. The total number of points by which the saturated colors were raised in affective value at the end of the series was 29; the total number of points by which they were lowered was 35; the excess of lowering over raising is then only 6. The corresponding excess for tints is 31, and for shades, 40. Shades, tints, and saturated colors were selected with about equal frequency for use as the test colors at the beginning and end of the experimental series. These results, then, seem to mean that *under the experimental conditions described, the effect of fatigue in lowering affective value is very decidedly less marked in the case of saturated colors than in that of shades and tints.* On the other hand, the variations from other sources than fatigue seemed to influence saturated colors, shades, and tints to nearly the same degree, if we may judge from the fact that the percentage of cases where the affective value of a color was the same at the beginning as at the end of a series was, for saturated colors, 40; for tints, 34, and for shades, 45. It looks, however, as though the affective impression made by saturated colors, whether pleasant or unpleasant, were so definite that fatigue induced by this method alters it but little; although we might expect that *continuous* experience with a saturated color would cause a rapid drop in its pleasantness.

Secondly, we undertook to find what kind of judgments were most influenced by fatigue. When we counted the number of times each numerical judgment from 2 to 7 appeared in connection with the first six colors of the series, and found in what percentage of this number the judgments were lowered at the end of the series, there appeared to be no uniform relation between the degree of pleasantness or unpleasantness in the first experience and the amount of lowering of affective value in the second. We noted that other sources of variation appeared to affect extreme judgments, 1 and 7, more than moderate judgments: the percentage of cases involving no change whatever in affective value was highest for the judgments 7 and 1. We at first thought that this result pointed to a conclusion regarding the variability of a given individual's affective reaction to a given color, which might be expressed in some such terms as that we are less likely to change our minds with regard to the objects of our extreme likes and dislikes than with regard to those which produce more moderate affective reactions. But later reflection showed us that the real cause of

the fact that the extreme judgments appeared to be more constant than the moderate ones lay in the conditions of the experiment. If the first judgment upon a color has been a moderate one, there are three possibilities with regard to the second: it may express the same affective value as the first, or a greater affective value, or a less one. If on the other hand the first judgment has assigned either the highest or the lowest affective value to a color, there are only two possibilities with regard to the second judgment: it may be the same as the first, or it may vary from it in one direction only. It naturally follows that the percentage of cases showing no change will, if there is no constant tendency present, be greater where the first judgment has assigned the highest or the lowest affective values.

XV. A NOTE ON THE AFFECTIVE VALUES OF COLORS

By M. F. WASHBURN

In the preceding study each of thirty-five observers was required to record in numerical terms her judgment on the pleasantness or unpleasantness of ninety colors, each color being presented in the form of a paper square 2.9 cm. a side, on a white background, and looked at for ten seconds. From the results thus obtained the verdicts of the different observers on a given color have been selected out, and their average calculated together with the mean variation. The whole series contained ninety saturated colors besides two tints and two shades of each color. To avoid what seemed unnecessary labor, the calculations to be discussed were made only for the lighter tint and the darker shade of each color: thus for eighteen tints and eighteen shades.

It appears that for our thirty-five observers, all women and nearly all college students, *the affective value of the tints is highest* (average from all observers, 4.7); *that of the shades is next* (average from all observers, 4.1), *and that of the saturated colors is lowest* (average from all observers, 3.6). Further, *that the affective reaction to saturated colors, whether pleasant or unpleasant, is more positive than that to shades and tints, and that to tints more positive than that to shades*, is indicated by the fact that the total number of judgments '4' (indifferent) is for saturated colors, 50; for tints, 89, and for shades, 101.

Among saturated colors, the order of increasing pleasantness, together with the average affective value assigned to each color by our observers, is as follows: green yellow, 2.1; orange and yellow green, 2.6; red violet and green, 3; yellow, 3.3; yellow orange and blue green, 3.4; red orange, 3.6; violet red, 3.7; violet blue and blue, 3.8; orange yellow and blue violet, 4; violet, 4.4; orange red, 4.5; green blue, 5.3; red, 5.6. *Pure red is the pleasantest saturated color, and green blue comes next. There is a tendency to dislike yellows and yellow greens.*

Among tints, the order of increasing pleasantness is the following: violet red, 3.4; green yellow, 3.8; orange, 4.3; yellow and orange yellow, 4.4; yellow orange, 4.5; blue green, red orange, and red, 4.6; green blue and orange red, 4.7; green, 4.3; yellow green, 5; violet blue, 5.1; blue violet, 5.5; red violet and violet, 5.9; blue, 6. *Blue is the pleasantest light tint, and indeed the pleasantest color in the whole series.*

Among shades, we have the following order of increasing pleasantness; yellow, 2.3; orange yellow, 2.7; blue green, 3.7; red violet, green yellow, yellow orange, and orange, 3.8; violet red, 3.9; red orange and orange red, 4.3; violet, 4.4; blue violet, green blue, and green, 4.5; red and violet blue, 4.8; blue, 5; yellow green, 5.3. *Yellow green is the pleasantest dark shade and blue comes next.*

It might seem that a study of the mean variations of these averages

would be of interest, as indicating the amount of unanimity in the tastes of our observers. But further thought reveals the fact that the mean variations are necessarily involved with the degree of pleasantness or unpleasantness indicated by the averages, and can have no independent significance. The smallest mean variations must belong to the highest and lowest averages, the largest mean variations to the averages of medium amount. For evidently if the average affective value of a color is four, the mean variation of that average may rise as high as three, since judgments from one to seven are possible: but if the average affective value of a color is six or two, the mean variation can hardly rise above one and a fraction, since there can be no judgments above seven or under one.

THE DISCRIMINATION OF ARTICULATE SOUNDS BY RACCOONS.

By W. T. SHEPHERD, Ph.D.

The present paper in some respects may be considered as a supplementary report to one on the same subject made by Professor L. W. Cole, and published by him about three years ago.¹ In the work reported by Professor Cole, in which I assisted, four raccoons were used and in this later work, now to be presented, the same animals were employed. The results which are given here have not previously been published, and only in a minor degree can they be considered merely supplementary to the work of Cole. The experiments to be reported are concerned with the Discrimination of Articulate Sounds by Raccoons.

It is commonly believed, and with some degree of reason, that the higher mammals can be taught to respond to their names, or to express it more accurately, to discriminate articulate sounds and to make appropriate motor responses thereto. It is well known that cats, dogs, horses and other domesticated animals learn to respond to their given names; but it is not known, from well conducted experiments, whether there is in these cases a discrimination of quality, of loudness, or of time of the sound. The results that have been obtained with animals under experimental conditions have been few, and in some cases the differentiation of tone, and intensity has not been made. Thorndike, it will be remembered, found that cats were apparently able to discriminate sounds made by him, though not with a great degree of delicacy.² The sounds that Thorndike used were quite complex in character, such as, "I must feed those cats" and "My name is Thorndike." In his work on the functions of the temporal lobes Kalisher reported³ having been able to get dogs to discriminate sounds made by an harmonium, but he was more interested in producing the association for the purpose of determining (after extirpation of different parts of the

¹Concerning the Intelligence of Raccoons, *Jour. Comp. Neur. And Psych.* Vol. 17, p. 211.

²*Animal Intelligence*, 1898.

³Eine neue Hörprüfungsmethode bei Hunden, *Sitz. d. Kgl. Ak. d. Wiss.*, X, 1907. p. 204 ff.

cerebral cortex) the cortical centres for sound perception than ability in his animals to discriminate sounds.

At the time the experiments were begun the raccoons were about six months old, and they had been trained for two months on various motor acts, reported in Cole's paper. In the early training period we had spoken to the animals, using different names, but the naming and calling was not done regularly and systematically. During the course of these preliminary experiments some of the animals had given indications of associating the sounds with reactions, and one in particular reacted often. Since no record of these experiments was made we cannot say how often the stimuli were given, and how well or poorly each animal reacted. For a period of two months following these trials no experiments on sound discrimination were made. Then the present work was begun.

For these tests each raccoon was placed in a separate cage which had a wire netting front. The four cages were arranged in different parts of the room and I sat at a distance of from four to eight feet from the cages. The names of the raccoons were called in irregular order, and I noted whether each responded only to the sound of his own name or to all the names. Each animal was fed when he responded to his own name (and was not fed when the other names were called). The animals were named, Jack, Jim, Tom, and Dolly. During these experiments all were kept very hungry.

1st day: In 168 experiments (42 trials for each animal) in which the reaction of turning and looking at me was taken as response, Jack reacted correctly 6 times, 3 doubtfully correct; Jim, 11 correct, 3 doubtful; Tom, 9 correct, 5 doubtful; Dolly, 1 correct, 2 doubtful. All the animals responded to the first call, but it is likely that was only an attention reaction; *i. e.*, a movement following a stimulus given by one familiar to the animals.

2nd day: After the preliminary trials of the first day I no longer fed the animals for this simple reaction. Responses were only noted correct when they climbed up the side of the cage, in addition to looking at me. Each raccoon was fed after his name was called, whether or not he gave the proper reaction. In 188 experiments (47 for each animal) Jack reacted correctly 21 times; Jim, 9; Tom, only 2 doubtful; Dolly, 1 correct with 4 doubtful. The results of these two days' experiments indicated that Jack and Jim could easily learn to respond to their names.

3rd day: Throughout the remainder of the experiments each animal was tested separately and the other three were kept out of his sight. I called the name of the animal, waited

10 seconds, if need be, for response, and then, whether or not a response was obtained, the animal was fed. Alternately with the name, the words "no feed" were called, and at these times the animal was not fed. In each case, as noted above, the correct reaction was considered to be obtained only when the raccoon looked at me and climbed up the side of the cage. In 50 trials on each kind of auditory stimulus Jack correctly reacted to his name 39 times and to "no feed" 22; in 30 similar trials Jim correctly reacted to his name 21 times and to "no feed" 15; Tom in 30 trials correctly reacted to his name 16 times, to "no feed" 10, with 13 doubtful in all; Dolly in 30 trials correctly responded to her name 15, to "no feed" 5, with 1 doubtful.

The experiments were continued in this way for 18 days, at the end of which time all the animals appeared to know their names perfectly. Thirteen days after the beginning of the experiments Jack correctly reacted to his name every time (25 trials), and incorrectly to the "no feed" signal only 3 times (25 trials). About the same percentages of correct and incorrect responses were obtained for Jack during the 8 succeeding days of the experiment. It appeared to me that Jack's few errors from this point might be accounted for by his eagerness for food.

After the names appeared to be well learned, as further test of auditory discrimination, I called other names, in addition to the individual's name, such as "box," "floor," after each name: *i. e.*, I called Jack, "box," "floor" in succession, and not alternately. No substantial difference in the percentage of proper responses was noted. It seems evident, therefore, that the animals had formed the habit of responding only when the appropriate sound was heard, and of not responding to other sounds. To further test the animals, I called the names and sounds in varying tones of voice, the lowest to the highest possible to me, and also had other persons call the words and names. With all the animals the responses were strikingly characteristic of discrimination.

May we, therefore, conclude that raccoons discriminate names or articulate sounds? The answer to this question will depend to a large extent upon the acceptance of the later experiments as conclusive. A serious objection to such a conclusion may be urged by some. It may well be said that, in the major part of the experiments, where the name and "no feed" were called alternately, the raccoons had learned to react alternately and that they reacted only to the rhythm of the stimuli. On the other hand, it must be remembered that following those experiments of alternate calls, there was a series in which the names were not called in any regular

order, and there was the same percentage of correct responses. Moreover, the addition of extra sounds to the names did not alter the proportional number of responses. Both of these later tests permit us to conclude, therefore, that the discrimination did take place.

It is of some interest to note that, Jack, which judging from the results of the earlier tests and from others to be reported in a later paper, was the most intelligent animal of the four, learned to associate the name with the proper response in 270¹ trials, Tom took 375 trials, Jim 425, while 500 trials were required for Dolly. This individual difference in animals experimented upon has been a noticeable feature in other experimenters' work, but to the mind of the writer not sufficient attention has been paid to it, and therefore animal psychologists have been content to work with a small number of animals (four, two, or even one) and to draw from their results too broad conclusions.

¹That is to say I called Jack's name 270 times and the other words "no feed," etc., in addition.

BOOK REVIEWS

Die Sprachstämme des Erdkreises. VON PROF. DR. FRANZ NIKOLAUS FINCK in Berlin, Druck und Verlag von B. G. Teubner in Leipzig, 1909. p. viii, 143. Aus *Natur und Geisteswelt*. Sammlung wissenschaftlich-gemeinverständlicher Darstellungen. 267. Bändchen.

Die Haupttypen des Sprachbaus. VON DR. FRANZ NIKOLAUS FINCK, Professor an der Universität Berlin, Druck und Verlag von B. G. Teubner in Leipzig, 1910. pp. vi, 136. *Ibid.*, 268. Bändchen.

These two latest additions to this excellent series of German handbooks on all manner of topics from superstition to forestry, and from the theatre to electro-chemistry, cannot fail to be of value to all interested in the comparative study of languages, although it is quite evident that a number of the problems raised of late years by the special investigations of the speech-forms of the American aborigines have not come to the author's attention. The first volume, "The Linguistic Stocks of the Globe," is a decided improvement upon the list in the second edition (1879) of Friedrich Müller's "Allgemeine Ethnographie," as may easily be proved by a glance at the indexes of the two books, and Müller's list has long remained the most complete and accessible to the German public. But the investigations of the last twenty-five years have put it altogether out of date, both as to accuracy and as to completeness. Dr. Finck classifies the languages of mankind under four races: Caucasian, American, Mongolian, Ethiopian (African and Oceanic Negroes). Under the Caucasian he lists the Indo-Germanic, the Hamito-Semitic, the languages of the peoples of the Caucasus (Caucasian in the minor sense), the Dravidian tongues of India and the Basque and Etruscan, besides certain other long extinct forms of speech belonging to Asia Minor, etc., such as Elamite, Chaldic, Hittite, Lycian, etc. There is too much mixture of race and speech in this classification. While, doubtless all the peoples of the Caucasus belong to the Caucasian or "white" race, ethnologists will hardly follow the author in separating the Dravidians entirely from the Australian aborigines and making them full-fledged Caucasians, against which view there are also arguments of a linguistic character. As members of the Mongolian race, the so-called Austro-Asiatic tongues (Kolarian, Mon-Khmer, Khasi, Nicobar, Semang, Senoi), Austronesian (Indonesian, Melanesian, Polynesian,—Malayo-Polynesian), Indo-Chinese (Tibeto-Burmese, Siamo-Chinese), Ural-Altaiic (Samoyed, Finnic-Ugric, Turkic, Mongolic, Tungusic, Japanese, Korean, etc.), Arctic or Hyperborean (Yeneseian, Jukaghir, Chukchee-Kamtchatkan, Ainu, Aleuto-Eskimo), a classification impossible to justify in the light of the most recent investigations. The studies resulting from the Jesup Northwest Pacific Expedition, under the direction of Dr. Franz Boas, have rendered it extremely probable that the languages of the so-called Paleo-Asiatic peoples of Northeastern Asia (Koriak, Kamtchatkan, Chukchee, Yukaghir, etc.) will be finally classed with the American Indian tongues. The Ainu must still be recognized as isolated among the Asiatic peoples, but there is more reason for affiliating them with the Caucasian race, than there is for so doing with the Dravidian. The inclusion of the Kolarian, the Mon-Khmer and the Melanesian in one group is open to fatal objections, while the Semang and Sakai of Malacca are hardly to be looked upon as Mongolian, nor can one be sure in placing there the Nicobarese, etc. And there is no good reason for cutting off the Eskimo from the rest of the American aborigines

as Dr. Finck does. The Sumerians of ancient Babylonia, about whose ethnological relations there is still not a little doubt, are here listed as Mongolian. The languages of the American race receive the most lengthy treatment (pp. 68-105) of any of the groups, the author following the regional method of cataloguing the chief stocks (North Pacific, North Atlantic, Central, Amazonian, Pampas, Andine or South Pacific), with indications of many of the smaller isolated tongues within these large areas. For North America Powell seems to have been followed generally, with some reference to later authorities (to judge from certain portions of the text). The modifications in the Powellian list, made necessary as the result of the more recent investigations of American philologists (and not included in Dr. Finck's summary) will be found in the articles on "Linguistic Stocks" in the "Hand book of American Indians North of Mexico" (Washington, Vol. I, 1907) and in the article on "North American Indians" in the forthcoming new edition of the "Encyclopedia Britannica." Dr. Finck, however, notes some of these latter results, *e. g.*, the inclusion of the Adaiyan with the Caddoan, the Piman with Uto-Aztecan, etc. The latest researches of Lehmann in the Central American region seem likely to lead to some changes in the arrangement of the linguistic stocks between Mexico and Panama. To the linguistic stocks of South America the writer of this review has devoted considerable attention, and a monograph on that subject is preparing for publication. Dr. Finck's list of South American stocks, while, of course, not exhaustive, takes in such comparatively recent items as the recognition as independent forms of speech of the Trumaian, Bororoan, Makuan, Miranhan, Guatoan, etc. The Onan of Tierra del Fuego is, without justification, classed as a dialect of Tsonekan (Patagonian); the evidence in hand still makes it necessary to list it as an independent stock. The independent character of Atacamenan, is however, recognized. On the whole, the list of South American stocks is fairly accurate so far as it goes, and free from any important errors. The languages of the Ethiopian race include those of the Negroes of Africa (Paleo-African, *i. e.*, Bushman, Hottentot; Neo-African: Bantu, West-Sudan, Central-Sudan, Nilotic, etc.) and the Oceanic Negroes (Australian and Tasmanian, Papuan, Andamanese). The most recent studies of the linguistic relations of the peoples of New Guinea and adjacent islands will necessitate some modifications of the lists in this region. In the introduction the author touches upon the question of the human "Ursprache," but wisely remarks in conclusion (p. 7) "discussion of the temporal sequence of the various linguistic stocks is impossible, and even the degree of their antiquity cannot be settled, since we are altogether ignorant of the supposed unitary primitive tongue of all mankind." In the second volume on "the Chief Types of Language," Professor Finck selects and discusses, with considerable detail, the grammatical and morphological peculiarities and characteristics of types of human languages (Chinese, Greenland Eskimo, Subija,—a language of the Zambezi region in South Africa, Samoan, Arabian, Greek and Georgian of the Caucasus). These eight languages are treated as "typical representatives of eight groups, to which, in my opinion, can without any great violence, be assigned the languages of the whole earth (p. v.)." If one takes as criterion the idea-content of the word, these languages, "with the gradual strengthening of the fragmentary character and the increasing morselizing of the idea masses present before the beginning of speaking," the languages in question can be arranged in the following order: Eskimo, Turkish, Georgic, Arabic, Chinese, Greek, Samoan, Subija. From another point of view, that of the organization of the elements of the sentence, etc., quite another order is necessary, and Dr. Finck distinguishes them thus: Root-isolating (Chinese), stem-isolating (Samoan), root-inflecting (Arabic), stem-inflecting (Greek), group-inflecting (Georgic), subordinating (Turkish), incorporating (Eskimo), and ordinating (Subija). Still other arrange-

ments, from other points of view are of course possible. The difficulties of such a type-theory as that set forth by Professor Finck are apparent from consideration of the languages of the Old World, but they multiply and intensify themselves when the linguistic stocks of the New World are carefully examined. The "Handbook of American Indian Languages North of Mexico," soon to be published by the Bureau of American Ethnology, under the competent editorship of Dr. Franz Boas, will, for the first time, present accurate and convincing evidence upon many points connected with the speech-types of the aborigines of the United States and Canada. Suffice it to say, for the present, that the Eskimo of Greenland can hardly serve as representative for all the Indian tongues of that region, much less for all those others of Mexico, Central and South America as well. Under any system of type-listing there must be many more than one type among the many scores of linguistic stocks living and dead in primitive America. A valuable part of this volume, and one especially interesting to psychologists, will be found in the analyses of texts accompanying the discussion of each linguistic type. The first volume has an exhaustive index, and the presence of one of some sort would not have injured the second.

ALEXANDER F. CHAMBERLAIN.

Studies in Spiritism, by AMY E. TANNER, Ph. D., with an introduction by G. Stanley Hall, Ph. D., LL. D. New York, Appleton, 1910. 408 pp.

This volume records the findings and verdict of a patient investigation sustained by a scientific conscience and enthusiasm. It represents constructively a logical interpretation of a group of phenomena whose psychological importance, though distinctive, seems modest when compared with the far-reaching conclusions attached to them by the popular verdict in favor of the supernatural. The convincing emphasis of the book is its indication that the "psychic research" platform is not only logically inadequate but psychologically perverse.

While the psychology of Paladino has been relegated to the limbo of fraud and credulity, the psychology of Mrs. Piper remains; for there seems no doubt that her sittings, whatever their more subtle or questionable implications, represent distinct if evasive phases of a secondary personality. Therein lies their interest, and not in their supposed evidential revelations. For exhibiting clearly and with illustrative detail the evidence that mediumistic trance is psychologically a form of lightly or deeply held secondary personality, Dr. Hall and Miss Tanner deserve credit and gratitude. Though the position,—and it would be surprising to find it otherwise,—has been favored and presented by other psychologists, it has not as yet received so clear a statement, so full a demonstration, nor indeed so original an exposition.

It is difficult soberly to take space to recount the endless records by which the advocates of Mrs. Piper's supernormal powers support their claim. In the "test" messages some objective control is exercised; and complex coincidences,—difficult, if not impossible to appraise,—enter to make or mar the case. Miss Tanner pursues the only way open to the dauntless critic: she analyzes the incidents, lays bare the constant sources of error, the looseness of interpretation, the ready play of chance, and with the structure thus stripped of prejudicial veneer she displays its card-board architecture. For the apologetics that have been used to make coincidence startling, and to read mysteries into commonplace trifles are no less amazing when one considers the intellectual standing of the protagonists. The psychological transgression is no less astounding; the credence given to long-range memories, the scant appreciation of the efficiency of suggestion, the neglect of control experiments, as well as the amateurish attitude towards such every-day foibles as "fishing," fooling, and lying, arouse pity or irritation, according to temperament.

Yet the great bulk of the "evidence" is of yet looser construction, and depends upon the presumption that Mrs. Piper's inspired hand can write messages revealing details that the terrestrial Mrs. Piper could not normally have acquired. Once more the truth is simple. It is abundantly clear that Mrs. Piper's auditory centres are keenly alert when her eyes are closed in trance; her surviving consciousness listens acutely, "fishes" adroitly, and her reeling in to suit the sporting impulses of the victim is nothing less than professional. As in dreams, the subject unwittingly contributes the data for the solution, and then marvels at the revelation when it appears. As for the spiritual hypothesis, why not be frank and say with Dr. Hall: "It is an utter psychological impossibility to treat this subject seriously."

Mrs. Piper pretends to be controlled by the actual disembodied Richard Hodgson. Not only, however, does the latter fail to prove his identity, but he is suggestible, ignorant, inconsequential and Piperian. With alacrity he summoned from the spirit-world wholly fictitious personages, as well as the shades of the known departed; he fell into the most simple logical traps, and through Mrs. Piper's organism exhibited pique and ill-temper at being exposed,—quite out of the rôle of the shrewd exposé of mystery that Hodgson was. A few whiffs of this atmosphere sends one back gasping to the fresh air. "Spiritism is the ruck and muck of modern culture, the common enemy of true science and of true religion; and to drain its dismal and miasmatic marshes is the great work of modern culture.

We have largely evicted superstition from the physical universe, which used to be the dumping-ground of the miraculous. Superstition to-day has its strongest hold in the dark *terrae incognitae* of the unconscious soul of man towards which researchers to-day are just as superstitious as savages are towards lightning, eclipses, comets and earthquakes."

Taking seriously the proposition that telepathy is supported by premonitions and experiments, that trance messages really foretell the future and reveal the past, that the controls of mediums bring back credentials which are adequate for the identification of the recently departed, psychology accepts the challenge and undertakes to show that a pervasive bias and a defective insight have shaped the data to distorted or imaginary significance. The evidence for this position cannot be summarized. Those who are interested in acquiring a hold upon it have now available Miss Tanner's presentation. On the other hand, recognizing that subconscious abnormalities arise spontaneously, and grow by what they feed upon, psychology finds in the encouragement given to the medium's sittings, in the serious systematic acceptance of the spiritistic hypothesis, and in the devout personal reactions of sitters, the hot-house atmosphere and the coddling ministrations that such parasitic growths absorb. The conspicuous suggestibility of such temperaments makes them assume the forms that excite interest and claim attention. They are allied to a recognized group of hysterical manifestations in the nearly normal, which in turn grow to troublesome intrusion or withdraw to manageable control according to the wisdom and insight with which they are met. The modern attitude towards such phenomena is a therapeutic one. The mediumistic or secondary personality is to be appeased, persuaded, suppressed, and the patient's resources united and made to see and to live life steadily and whole. Such a consummation can never be, if the abnormality is displayed, cherished, and embraced as a means of livelihood.

Dr. Hall is confident that "the mysteries of our psychic being are bound ere long to be cleared up. Every one of these ghostly phenomena will be brought under the domain of law. The present recrudescence here of ancient faiths in the supernatural is very interesting as a psychic atavism, as the last flashing up of a group of old psychoses soon to become extinct.

When genetic psychology has done its work, all these psychic researches will take their places among the solemn absurdities in the history of thought; and the instincts which prompted them will be recognized as only psychic rudimentary organs that ought to be and will be left to atrophy."

University of Wisconsin.

JOSEPH JASTROW.

The Metaphysics of a Naturalist; Philosophical and Psychological Fragments.

By the late C. L. HERRICK. Bulletin of the Scientific Laboratories of Denison University, Vol. XV. Granville, Ohio, 1910. 99 pp.

This book aims to supplement and, to some extent, to unify such of the distinctive philosophical teachings of Professor Herrick as have already been published, by adding to them and correlating with them material brought together from papers and manuscripts hitherto unpublished. The first chapter is entitled "The Summation-Irradiation Theory of Pleasure-Pain." It gives an analysis of feeling and of emotion, and explains them in terms of physiological tensions and adjustments, basing the arguments on bodily structure and function and upon introspection. There is also included a table of the other classes of mental processes, with their physiological parallels. At the end of the book are four short, less technical and less distinctive, chapters on the freedom of the will, the problem of evil, immortality, and ethical conclusions. The book is chiefly concerned to present the metaphysical theory of dynamic monism, and to explain, in terms of this theory, the concept of consciousness, the relation of mind and body, individuality, matter, life, etc. Some of the fundamental conclusions are: Existence (being) and energy are identical; Energy is pure spontaneity; Unimpeded infinite energy would seem to us indistinguishable from non-existence; Force arises from the interference of energy, and implies resistance; The complexity of resistance measures the quality of the force, the degree of resistance measures the quantity of the force; Matter is a subjective interpretation of forces in a state of relative equilibrium; Consciousness is the focussing of diverse forces upon the complicated neural equilibrium; Conscious states are epiphenomena, due to the constant becoming between energy and force. The writer makes frequent reference to the theories and results of the natural sciences, especially those of physics, physiology, and mathematics, and he takes over into his metaphysics, almost directly, such scientific concepts as inertia, resistance, motion, energy, vortices, vectors, etc. According to the editors, the book is intended as a contribution to work on the methodology of the sciences, of the sort done by Tyndall, Huxley, Kelvin, Helmholtz, Mach and Ostwald.

W. S. FOSTER.

Les rêves et leur interprétation. Par PAUL MEUNIER et RENÉ MASSELOU. (Collection Psychologie Expérimentale et de Métapsychie). Bloud et Cie, Paris, 1910. 211 p.

This is an essay in morbid psychology, both of the authors being psychiatrists. The first chapter, entitled the psychological mechanism of dreams, gives a partial résumé of the scientific literature of dreams, chiefly of French work, supplemented by contributions from the authors' own observations. The second chapter discusses the diagnostic value of dreams. While there is much difficulty in distinguishing the truly prodromic or symptomatic dream from accidental dreams without pathological significance, the authors, nevertheless, conclude that dreams are in some cases of considerable value in diagnosis and the following chapters are devoted to a discussion of the distinguishing characteristics of dreams in infections and intoxications, neuroses and insanities, which have diagnostic significance, *e. g.*, the color, red, persistently appearing in dreams is a frequent phenomenon in premenstrual periods, cardiac affections, premeningeal attacks, inflammatory infections of the eye and the aura of epileptic attacks. Terrifying hallucinations and zoöscopy are characteristic not only of

alcoholism but are liable to occur in all toxic affections. Stereotyped dreams occur in epilepsy and hysteria.

In psychoses, the dream may reveal an obsession or an impulsive tendency before it has been manifested in the waking state. Finally, in mental pathology the persistence of dreams is a sign of the manifest activity of morbid processes and in convalescence, the type of dreams may be of great importance for determining the state of the patient.

The book is disappointing in that it takes no account of Freud's *Traumdeutung* or of De Sanctis' later work, by far the two most important contributions to the psychology of dreams and without consideration of which any discussion of the subject must be inadequate.

THEODATE L. SMITH.

L'année psychologique, publiée par ALFRED BINET. Paris, Masson, 1910. 500 p. Seizième année.

Besides the usual literature, the author himself has monographs upon the physical signs of intelligence, on Rembrandt in relation to the new style of art criticism, the mental states of the insane; while with Simon he gives us an extensive study of hysteria and on insanity with consciousness of it, of the maniacal depressive type, the systematized form, and dementia, retardation, formulating a new classification. Finally comes a brief article on judicial diagnostics, while the bibliography occupies pages 382 to 500.

A beginner's history of philosophy, by HERBERT ERNEST CUSHMAN. v. 1. Ancient and mediæval philosophy. Boston, Houghton, Mifflin (1910). 406 p.

This work is dedicated to Professor Palmer and is intended as a textbook for sketch-courses in the history of philosophy. It is for the student rather than the teacher; and is written on the background of geography and literary and political history and uses many tables. The present volume begins with the early Greeks and ends with Ockham.

Psychologie des Kindes, von ROBERT GAUPP. 2d enl. ed. Leipzig, Teubner, 1910. 163 p.

This work is divided into three parts. The first treats the psychology of the little child, beginning with a brief historical sketch of child psychology, a discussion of its methods, literature, the development of the first year of attention, speech, Gemüt, will, thought, lies, impulses, sense play, etc. The second part is on the psychology of the school child, beginning with entrance, and discussing memory, attention, power of achievement and control, fatigue, power of judgment, writing, the child and its relations to art. The third part treats of children who are psychically abnormal.

Trick methods of Eusapia Palladino, by STANLEY LE FEVRE KREBS. Reprinted from *The Reformed Church Review*, Vol. XIV, July, 1910. Phila., 1910. p. 337-383.

This author concludes that Palladino uses no confederate, that there are no traps or sliding panels, that all her phenomena are produced in an area within the stretch of her arm or leg, certainly if lengthened a little by the use of a flower stand as a reacher and a shoe-ledge as a fulcrum for levitation. The author does not believe that she has any extraordinary or telekinetic power. If she had she ought to be "lifted out of the realm of showdom;" nor does he believe that the hypothesis of survival after death will be proven by any phenomena like hers. It is all a deception of sight and touch, "the psychological atmosphere being helped along by intentional suggestions." She always dresses in black and her cabinet is painted black inside; he would have her dress in white. He thinks, too, that if she were placed at the broadside of a table and had only one per-

son control both her hands and both her feet, "John," her control, would be put out of business. He would tie her ankles and wrists with a slack of only four or five inches, but none of these she will allow.

Über Ermüdungsstoffe, von WOLFGANG WEICHARDT. Stuttgart, Enke, 1910. 66 p.

This is an interesting and compendious account of the large subject treated. Symptoms of extreme general fatigue are first described; then the fatigue of special parts and organs, investigations on immunity and fatigue stuffs, the attempts to apply chemical and physical means to muscle extraction and to albumen, how kenotoxine can be influenced, active immunization, anti-somatic influences, how pathological processes can be influenced by antikenotoxine.

An introduction to the study of hypnotism, experimental and therapeutic, by H. E. WINGFIELD. London, Baillière, Tindall & Cox, 1910. 175 p.

This book is an attempt to supply a simple answer to the question What is hypnotism? and makes no effort to range itself with the many larger works on the subject but intends rather to serve as an introduction to these. The matter is treated mainly from the experimental point of view and the author does not attempt to include anything that those already familiar with the subject did not already know. Its chapters are on the subconscious, the methods of inducing hypnosis, its phenomena and stages, other hysterical phenomena, treatment by suggestion, and the case against hypnotism.

The concept standard, a historical survey of what men have conceived as constituting or determining life values; criticism and interpretation of the different theories. By ANNE M. NICHOLSON. Teachers College Columbia University Contributions to Education, No. 29. New York, Teachers College, 1910. 138 p.

The chapters are: the fundamental categories and principles, the standard in primitive societies and the genetic point of view, review of the conception of the standard and its method of functioning from the first historic to present time, standard as conceived in epochs Judæan, Medieval, Renaissance, Protestantism, Cartesian, the English School, German idealism, the materialistic concept of this standard, its function in national crises.

A text-book of psychology, by EDWARD BRADFORD TITCHENER. New York, Macmillan Company, 1910. 565 p.

This work was written to take the place of the author's *Outlines of Psychology* in 1896 which has passed beyond the possibility of revision. Still it follows the general lines laid down in the *Outline*, although with less space devoted to nervous physiology. The work in its present form will be gratefully received by teachers and it is unquestionably the best in its own specific, if restricted, field.

The qualities of men, by JOSEPH JASTROW. Boston, Houghton, Mifflin Co., 1910. 183 p.

A study of the qualities of men in which a physiological interest in humanity is prominent may properly be expected to undertake the analysis of the fundamental factors in human nature, their transformation, values in growth, education and vocation. This is the basal problem in the psychology of human traits. For the elucidation of this theme, the author is preparing a work entitled *Character and Temperament*, but in the preparation for this work he found the more general bearing of the problems of human quality of growing importance and felt the need of a more general form and statement and a wider appeal. Hence this book, the chapters of which are the sensibilities, the ideals of appreciation, the support of the

sensibilities, the analysis of quality, quality in circumstance, compatibilities of quality, the poetic qualities, the social encouragement of quality, its upper ranges, its interactions with environment, its relation to careers and the realm of practice.

Die Phantasie nach ihrem Wesen und ihrer Bedeutung für das Geistesleben, von A. SCHÖPPA. Leipzig, Dürr, 1909. 144 p.

The chief topics here are the essence of phantasy, its relations to psychic life, with a good section on the playing, speaking, narrating, drawing, child, on the instruction of the fancy in childhood, phantasy in everyday life, in poetry, rhyme, rhythm, saga, legend, idyll, romance, fable, drama, phantasy in music, in the plastic arts, in science, and finally in religion. The author's psychology is mainly under the influence of Wundt, Lipps and Mach.

Die Sinnesorgane der Pflanzen, von G. HABERLANDT. Sonderabdruck aus der vierten Auflage der physiologischen Pflanzenanatomie, S. 520-573. Leipzig, Engelmann, 1909.

This reprint is an excellent little epitome of its subject, discussing the relations of the organ to the stimulus, with many cuts of sensory hairs, bristles, statoliths, stalks, leaves, with experimental observations on the connection of statoliths and geopterception. The writer discusses the light sense in leaves, the nature of their papillary epidermis and of optical spots, etc.

The metabolism and energy transformations of healthy man during rest, by FRANCIS G. BENEDICT and THORNE M. CARPENTER. Washington, Carnegie Institution, 1910. 255 p.

The first part of this book is introductory, telling what has been done before and elsewhere. The second is statistics of experiments; and the third and most elaborate is the discussion of results, which are not, unfortunately for the reader, summarized.

Der Traum und seine Deutung, nebst erklärten Traumbeispielen, von E. J. G. STUMPF. Leipzig, Mutze, 1899. 188 p.

This book, although not new, may have a certain added interest just now on account of the prominence which the problem of dream psychology has assumed in this country owing to the recent influence of Freud. Stumpf treats in the successive chapters, day and night in their reciprocal relations, and the nature and essence of dreams. These are the two sections of the book. If the author had designed to block every one's endeavor to get at the root idea of his treatment without reading every sentence in the book, he could hardly have succeeded better, for there is no index or titles of any kind, apparently no summaries or epitomes, nothing italicized; so that as it is the book stands like a castle, attractive outside and doubtless full of good things within, but open under no conditions to casual visitors but only those who desire to reside in it.

A first book in psychology, by MARY WHITON CALKINS. New York, Macmillan, 1910. 419 p.

This book is written under a growing conviction that psychology is best treated as a study of conscious selves in relation to other selves and to outer objects. This book differs from an introduction to psychology, with which it is liable to be confounded, for here the approach is simpler and more direct. In the former book, psychology is treated both as a science of selves and of ideas and all is discussed from both points of view. Here the double treatment is abandoned. Here, too, the author has tried to embody the results of functional psychology, that is, taking an account of bodily reactions and environment which accompany thought, feeling and will. An appendix, too, treats of the physiology of the nervous system

and the senses and abnormalities. "This is, then, a new book, not the condensation of an old one, yet it does not teach a new form of psychology." The chief sections here are, methods and uses, perception and imagination, other sensory elements, their combinations and differentiations, effect, attention, productive imagination, memory, selective association, recognition, thought, conception, judgment, reason, emotion, will, faith and belief, the social and religious consciousness. The appendix includes pages 273 to 417.

Straight goods in philosophy, by PAUL KARISHKA. New York, Roger Brothers, 1910. 207 p.

This name, we take, it, is a pseudonym. The author, who has already written several other very stimulating but inadequately appreciated works, is evidently a thinker born and trained. He here gives up the more erudite subtleties of metaphysics and speaks to the plain man and tells him that philosophy simply means wisdom in work. It is really impossible to give an adequate conception of this work, which certainly shows a very wide repertoire of insights and interests on the author's part. It has nearly forty chapters. Some of them are loving everybody, the professional philosophy, healing the body by mind, posing, the things we hate, sympathy, the funeral of a living corpse, weeds, man and woman, thoughts that kill, food, why women are sly, old age, the law of opposites, privileged people, the essentials of a philosophic life. The book is certainly original, suggestive and stimulating.

Educational psychology, by EDWARD L. THORNDIKE. 2d ed., rev. & enl. Teachers College, Columbia University, N. Y., 1910. 248 p.

This book is a revision of a work which appeared in 1903. Its primary purpose is to provide students in advanced courses in educational psychology with the material which they would otherwise have to get at lectures at great time and cost. The author has admitted the influence of special training upon more general abilities. He treats the measurement of individual differences, the influence of sex, of remote ancestry or race, of immediate ancestry or family, of maturity and environment, the nature and amount of individual differences in single traits, the relation between the amounts of different traits in the same individual, the nature and amount of individual differences in combinations of traits, types of intellect and character, extreme individual differences, and exceptional children, with several appendixes.

The World a Spiritual System. An outline of metaphysics. By JAMES H. SNOWDEN. N. Y., Macmillan, 1910. 316 p.

The author is evidently in an apologetic state of mind, at least in his preface, quoting various definitions of metaphysics, such as "a blind man looking on a dark night for a black cat that isn't there." However he tells us that the difficulties and the confusion are more apparent than real. He discusses in successive chapters the nature of metaphysics, including its definition, method, assumptions, spirit, object and system. He then discusses the different points of view from which the world is regarded, viz.: from that of plain men, the scientist and the metaphysician; the subjectivity of space, with its theory and reasons, that of time, subjective reality, the soul's knowledge of itself, its fundamental character, general character; how we reach objective reality; its nature, including the world as phenomenon; as life, as thought, sensibility, will, the general character of the world and man as its key. Then follow the relations of the world and God as revealed as cause in its relations to man, and finally the applications of idealism as seen in the relations of mind and body, immortality, problem of evil, idealism in religion and life, with a brief suggestive course of reading and some account of the chief modern writers upon these subjects.

The use of the Theory of Correlation in Psychology. WILLIAM BROWN, Cambridge. Printed privately at the University Press, 1910. p. 83.

Some Experimental Results in the Correlation of Mental Abilities. WILLIAM BROWN. *The Journal of Psychology*, Vol. 3, 1910, 296-322.

An objective Study of Mathematical Intelligence. WILLIAM BROWN. *Biometrika*, Vol. 7, 1910, p. 352-367.

The use of the Theory of Correlation in Psychology. WILLIAM BROWN. Cambridge. Printed privately at the University Press, 1910. p. 83.

The paper, which is a doctor's thesis, consists of three parts, the first of which contains an exposition of the theory of correlation, the second the history of the use to which this theory has been put in psychology, and the third the description of a series of investigations undertaken by the author. The third part has appeared separately under the title "Some Experimental Results in the Correlation of Mental Abilities" in the *British Journal of Psychology*, 1910, Vol. 3, p. 296-322.

Starting from the notion of the regression curve and regression line the author develops formulæ for the coefficient of correlation, for the correlation ratio, for the probable errors and for multiple correlation and then proceeds to discuss some other methods of measuring correlation. The method of ranks and its criticism by Pearson is discussed in some detail. Pearson's objections against this method are directed (1.) against the use of rank as a quantitative measure of character, and (2.) against the formulæ derived by Spearman, for which new ones are substituted. Rank must not be used as a quantitative measure of character, because this assumes that the unit of rank is the same throughout the scale, which is not the case since the unit of rank between mediocrities is practically zero, while it is very large between extreme individuals. This argument of Pearson gains additional interest in view of Cattell's classification of men of science according to ranks attributed to them by a number of more or less prominent men. If Pearson's argument should turn out to be correct, the supposition for averaging the ranks attributed to the same man in the different classifications are not given. Brown's short presentation of the theory of correlation is all the more significant, because it carries with it the authority of Pearson who read the proof. The notation used is the one customary in biometric treatises, which is perhaps not the most fortunate. The difficulties for the reader increase, if new signs are introduced without definition, as happens to be the case on p. 7.

Brown's discussion of the significance of the coefficient of correlation is very interesting. He insists on the fact that this quantity has a significance as a measure of the degree of community or identity of causation, if the regression curve is linear. He considers a general answer as to the significance of the coefficient of correlation impossible, but he tries to make it clear by an example known by the name of Weldon's experiment. A dozen dice are cast a number of times and the number of dice showing four or more spots is recorded. The results of the 1st, 3d, 5th, . . . throws obviously will be in no relation to those of the 2nd, 4th, 6th, . . . throws. We now make the results of the even throws dependent on those of the uneven. We stain six of the dice red and we make the even throws only with the six white ones, leaving the red ones on the table but counting indiscriminately the dice which show four or more spots. The results of the even throws will be correlated to those of the uneven throws and it is shown that in this case the proportion of factors common to the two series is given by the coefficient of correlation itself. This, however, is an exception since the extent of identity of causation as a rule is measured by an unknown function of the coefficient of correlation. Brown thinks that "a general lack of knowledge of the mathematical theory of correlation among psychologists" is responsible for the fact that in psychology, comparatively

little use is made of this theory, but it is the reviewer's opinion that this lack of interest may be attributed largely to the difficulty of explaining the real significance of the coefficient of correlation. Bruns, Lipps and Lachmann have supplied examples, where this quantity is void of significance and almost every one can construct examples where it is misleading. The specialists ought to give us a clear presentation of the theory of correlation and its applications to psychology and demonstrate at least the conditions under which the extent of community of causation is measured by an uneven function of the coefficient of correlation, because in this case one would be sure that a negative value of the coefficient of correlation does not indicate a positive correlation.

The historical part of the paper shows that the theory of correlation has been used in psychology chiefly for the study of the relation of different mental abilities to one another and to general intelligence. The first investigation showing any mathematical precision was undertaken by Clark Wissler, and was followed by one by Aikins and Thorndike; the correlations between mental abilities were generally low. Spearman, instead of working on large groups, took groups of small size, making up for this deficiency by subjecting his raw data to a mathematical treatment. He finds a hierarchy among the different school subjects and concludes that these different mental activities are saturated with one common fundamental function (or group of functions). This essential element in intelligence is supposed to coincide with the essential element in sensory functions. Spearman's results were tested by Thorndike, Lay and Dean, who concluded from their results that there exists a complex set of bonds between the formal side of thought and its content, and that there is nothing whatever in common to all mental functions or to any half of them. C. Burt confirmed Spearman's results in so far as he found a hierarchy in the subjects tested, but he believes that the central factor is voluntary attention. The author then mentions the paper of Pearson on the relationship of intelligence to physical and mental characters, the work of Miss Elderton, who evaluated the data collected by Heymans and Wiersma and those of Ivahhoff, the later work of Thorndike and of his pupils and his own Objective Study of Mathematical Intelligence (in *Biometrika*, Vol. 7, 1910, p. 352-363). He found that algebra and geometry show hardly any correlation, a result which coincides with the one obtained by Burriss that the coefficients for the correlation between algebra and geometry is nearly as low as that between mathematics and a non-mathematical subject.

The author's own experimental investigations were undertaken with a view of ascertaining the correlation of certain very simple mental activities to one another and to general intelligence as measured by school marks, teacher's judgments, etc. The experiments were made on tolerably large and fairly homogeneous groups of students, who were as far as possible identically situated in respect to previous practice, general training and intelligence. The tests comprised crossing out letters (two letters, four letters and all the letters) in a page of meaningless words, adding up digits in Kraepelin's *Rechenhefte*, bisecting and trisecting lines, measuring the Mueller-Lyer and the vertical-horizontal illusions, memorizing nonsense syllables and memorizing poetry, and combination (tested by the method of Ebbinghaus). The observation that a large proportion of the subjects show a negative vertical-horizontal illusion (*i. e.* underestimate the vertical line) is curious and of interest to the experimentalist also. The table of the coefficients of correlation shows no hierarchical arrangement except in one case where spurious correlation may be suspected. Extraneous sources of correlation, such as, *e. g.*, differences in the discipline, may influence the results in a constant direction and thus produce the hierarchical arrangement. The question as to the existence or non-existence of a central function is not answered definitely, but a number of results may be

taken as arguments against its existence. A definite answer can be given only on the basis of experiments on much larger groups, which will give results with smaller probable errors. Brown's results may be considered to bear out to some extent the views of Thorndike and to contradict those of Spearman.

F. M. URBAN.

Der Begriff des Instinktes, einst und jetzt; eine Studie über die Geschichte und die Grundlagen der Tierpsychologie. HEINRICH ERNST ZIEGLER. Zweite, verbesserte und vermehrte Auflage. Jena, Gustav Fischer. 1910. VI+112.

This book sketches the historical development of the concept of instinct and discusses the modern significance of the term. The author points out that in early Greek thought no sharp distinction was made between the characteristics which were attributed to human and to animal consciousness. But in the system of Plato abstract thought was held to be the essential activity of mind; since this capacity cannot be ascribed to animals a sharp line of demarcation was now drawn, for the first time, between the human and the animal mind. And perhaps the most valuable contribution which the author offers to his readers is his elaboration of the thesis that ever since the time of Plato there have existed side by side, a tendency to magnify or even to humanize the animal mind, and a counter-tendency to relegate it to a low level on the scale of consciousness, if not to deny its existence. The doctrines of the Christian church were influenced by Greek idealism, and the Platonic conception of the animal mind was appropriated and emphasized by the theologians. But if animals are wholly lacking in intelligence how is one to explain the manifest appropriateness and efficiency of their behavior? The question was answered by an appeal to instinct,—a concept which had been introduced by the Stoics,—and instinct was conceived to be an institution of nature in virtue of which animals are enabled to react appropriately without themselves being able to foresee, or even to perceive, the appropriateness of their reactions. Instincts were held to be divine creations, and they were even cited as proofs of the wisdom of their creator. This view was defended by Aquinas, Descartes and others, and it came to be a dogma of theology,—and Ziegler cites Altum and Wassmann as its modern representatives. The position which the vitalists assumed was not essentially different. This dogma was opposed by Montaigne and by Gassendi; and subsequent contributions to the humanizing or anthropomorphic movement were made by Leibnitz, Condillac, La Mettrie, Brehm, Vogt, Büchner, and numerous others. A new era in the history of instinct begins with Darwin. Instinct is no longer regarded as the peculiar characteristic of animal endowment; numerous human instincts are shown to exist and to be of profound significance. Moreover the fact that instincts are appropriate and serviceable is now explained from natural causes. Ziegler discusses and rejects Lamarckianism,—among whose representatives he mentions Haeckel, Preyer, Hering, Wundt and Semon. His own view of instinct is based upon the Weismann conception, and has, as the author shows, much in common with the view of Lloyd Morgan. He enumerates a list of criteria which differentiate instinctive from intelligent behavior, but the list contains nothing which is essentially new. The difference between instinctive action and intelligent action is referred to the assumption that the former is due to inherited paths in the nervous system, while the latter is due to acquired paths.

In an (illustrated) appendix Ziegler discusses the brain anatomy of the bee and the ant, and points out that the three classes within the colony (queens, drones and workers; males, females and workers) which manifest typically different instincts, also possess typical differences of brain structure.

The book is written by a zoölogist, whose discussions frequently display a lack of critical insight into the problems of comparative psychology. But his historical sketch is a valuable contribution to the literature.

J. W. BAIRD.

The phenomenology of mind, by G. W. F. HEGEL. Edited, with an introduction and notes, by J. B. Baillie. London, Swan Sonnenschein, 1910. 2 v.

The translator well says that this has long been recognized as a unique product of Teutonic genius, "on the whole perhaps the most remarkable treatise in the history of modern philosophy." This is true both as to the style of thinking, its expression and the comprehensiveness of its theme. It is an attempt to give an exhaustive analysis of the life history of the human spirit, to reduce its complex and involved harmonies to their elemental leading motives, and to express these controlling ideas in an orderly and connected system. The courage that made this effort possible was due to the state of the intellectual atmosphere of the times, which was charged with grandiose ideas that were capable of stimulating and sustaining philosophical enthusiasm and exciting and intoxicating speculative ambition. The writer thinks that Hegel was inspired by Kant to sail these unknown speculative seas with only a fraction of his scientific knowledge and none of his philosophical prudence. Still there is an enormous wealth of presentative material behind this treatise which shines through it. The discussion is often foreshortened and the scheme of the work is out of proportion, some points being treated with great elaborateness and others very concisely. The last part of the work is especially unsatisfactory and it is no excuse that it was written hastily just before the battle of Jena in 1806.

The translator certainly had a difficult task before him and he merits the very hearty thanks of all students of philosophy who have for years looked, no matter how well they read German, to this work with mingled feelings of curiosity and awe.

Manual of mental and physical tests; a book of directions compiled with special reference to the experimental study of school children in the laboratory or classroom, by GUY MONTROSE WHIPPLE. Baltimore, Warwick & York, 1910. 534 p.

All psychologists will be grateful to the author for the compilation of this manual. The general groups of tests are anthropometric, those of physical, mental and sensory capacity, of attention and perception, of effort and description, of association, learning and memory, of suggestibility, of imagination and invention, of intellectual development, besides general tests.

A text-book of psychology, by EDWARD BRADFORD TITCHENER. N. Y., Macmillan, 1910. 565 p.

The present work has been written to take the place of the author's "Outlines of Psychology," which was stereotyped in 1896 and which, owing to the rapid progress of the science, has long since passed beyond the possibility of revision, despite the continued demand for the book. The author would have preferred to let it die a natural death, feeling that it would be impossible to recover the freshness and vigor of the first reading, but finally deciding to re-write, a first part, containing about half of this work, appeared in 1909, and we now have the remainder of it. The author and publisher intend to withdraw the "Outlines" from the market in the near future in the hope that this work, which follows the same general lines, will take its place.

Psyche: a concise and easily comprehensible treatise on the elements of psychiatry and psychology for students of medicine and law, by MAX TALMEY. N. Y., Medico-Legal Publishing Co., 1910. 282 p.

The writer divides the work into several parts, as follows: the psychology or physiology of the mental functions and their pathology, following under this latter section the rubrics of feelings, ideation, will and consciousness. Part three treats of the etiology of insanity; part four, its therapy; part five, special pathology.

The psychology and training of the horse, by COUNT EUGENIO MARTINENGO CESARESCO. London, Unwin, 1906. 334 p.

The chief sections of this book are headed, the mind of the horse, how the horse learns and how he must be taught, how he is taught obedience, fear and how to overcome it. The work is attractively written, bound and printed.

Ueber den Traum. Experimentel-psychologische Untersuchungen, von J. MOURLY VOLD. Herausgegeben von O. Klemm. Erster Band. Leipzig, Barth, 1910. 435 p.

This is a very interesting experimental study by a man who long practised upon himself and others binding limbs and otherwise restricting freedom of movement, noting the effect upon the dreams. The conclusion shows a very systematic relation and suggests the desirability of further experiments upon others.

Dogmatism and evolution. Studies in modern philosophy. By THEODORE DE LAGUNA and GRACE ANDRUS DE LAGUNA. N. Y., Macmillan, 1910. 259 p.

Dogmatism here denotes the body of logical assumptions which were generally made by the thinkers of all schools before the rise of theories of organic and social evolution. Its application is therefore very wide, including the empiricism of Berkeley and Hume as well as the rationalism of Descartes and Leibnitz. These studies do not claim systematic unity.

The science of poetry and the philosophy of language, by HUDSON MAXIM. N. Y., Funk & Wagnalls, 1910. 294.

After laying down some fundamental principles, the author proceeds to describe the evolution of analogical speech and discusses the question what poetry is and what not. Then follows an interesting chapter on profanity. Still others on the application of fundamental principles, the dynamics of human speech, philosophy of English verse, oratory, poetry, etc. The work is illustrated by a dozen or more quaint and mystic illustrations. The reviewer feels that it is difficult to do justice to this book.

The reasoning ability of children, by FREDERICK G. BONSER. N. Y., Columbia University, 1910. 133 p. (Columbia University Teachers College. Contributions to Education, No. 37.)

The author tested children chiefly of the middle and upper grades in problems of simple arithmetic, in supplying omitted words or completing sentences, in scoring out wrong words, in writing opposites, in selecting the best from ten reasons given for four different things, in selecting definitions, in giving in their own words the substance of poems. Returns were obtained from 757 children. The best test of general ability was that of opposites and the poorest of interpreting poems. The work is careful and painstaking in a high degree, but it tells us very little about children's power of reasoning and should have been designated a test of general ability among children.

Muscle-reading; a method of investigating involuntary movements and mental types, by JUNE E. DOWNEY. Reprinted from the Psychological Review, July, 1909. Vol. XVI, no. 4, pp. 257-301.

The central tendency of judgment, by H. L. HOLLINGWORTH. Reprinted from the journal of Philosophy, Psychology and Scientific methods, Aug. 18, 1910. Vol. VII, no. 17, pp. 461-469.

The perceptual basis for judgments of extent, by H. L. HOLLINGWORTH. Reprinted from the Journal of Philosophy, Psychology and Scientific Methods, Nov. 11, 1909. Vol. IV, no. 23, pp. 623-626.

The suggestive power of hypnotism, by L. FORBES WINSLOW. London, Rebman, Ltd., 1910. 90 p.

This work, which is dedicated to the officers and members of the Psychotherapeutic Society, regards its work as an old prophecy fulfilled, for Sir James Paget long ago said that some day some clever quack would disgrace physicians by curing by his will what they could not do by their remedies. After giving a general account of suggestion, duality of mind, the differences between subjective and objective, the author gives us the history of hypnotism, tells us of its schools and those of therapeutics, pays his respects to the pioneers of psychotherapy, discusses the rules governing its use, relations between crime and hypnotism, auto suggestion, hypnotism in the courts, power of suggestion in causing illness, the case of Elliotson, explains the secret of the pilgrimages to Lourdes, describes the effects of suggestion in dealing with inebriety and the drug habit, describes transference, fashion, which is purely suggestion, and concludes with a felicitation to the profession upon the fact that nearly all intelligent and progressive physicians have now accepted the main facts in this field.

Les lois morbides de l'association des idées, par M. PELLETIER. Paris, Rousset, 1904. 148 p.

After discussing the place of association of ideas in the psychic processes in general, the author characterizes them in the normal state and then discusses symptoms of mania and the incoherence of ideas that characterizes it and the causes of this incoherence. A final chapter treats of debility and how incomplete coherence affects them. The work is written essentially from the standpoint of a clinician.

Bulletin No. 2, Government Hospital for the Insane, Washington, D. C. Edited by WILLIAM A. WHITE. Washington, Gov't Printing Office, 1910. 135 p.

This work contains the following articles by Dr. S. I. Franz. The knee jerk in paresis, sensations following nerve division, touch sensations in different bodily segments, and Some considerations of the association word experiment. Dr. Achúcarro writes on some pathological findings in the neuroglia and in the ganglion cells of the cortex in senile conditions, on elongated and other cells in the Ammon's horn of the rabbit, plaque lesions in the ependyma of the lateral ventricles. The work ends with a final article by Dr. W. H. Hough on the comparative diagnostic value of the Noguchi butyric acid reaction and cytological examination of the cerebro-spinal fluid.

Die Frömmigkeit des Grafen Ludwig von Zinzendorf. Ein psychoanalytischer Beitrag zur Kenntnis der religiösen Sublimierungsprozesse und zur Erklärung des Pietismus, von OSKAR PFISTER. Leipzig, Deuticke, 1910. 122 p.

This work is dedicated to Professor Jung of Zurich and is an interesting new contribution to the rapidly growing number of Freudian interpretations of life.

Nature and man, by EDWIN RAY LANKESTER. The Romane's Lecture 1905. Oxford, Clarendon Press, 1905. 61 p.

Criticism and beauty; a lecture rewritten. Being the Romane's lecture for, 1909, by ARTHUR JAMES BALFOUR. Oxford, Clarendon Press, 1910. 48 p.

The judgment of difference with special reference to the doctrine of the threshold, in the case of lifted weights. By WARNER BROWN. University of California Publications in Psychology. Vol. I, No. I, pp. 1-71. Sept., 24, 1910. Berkeley, the University Press.

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- An experimental study of sleep.* (From the physiological laboratory of the Harvard Medical School and from Sidis' laboratory), by BORIS SIDIS. Boston, Richard G. Badger, 1909. 106 p.
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- Die psychologische Sehnhörung in psychoanalytischer Auffassung,* von SIGMUND FREUD. Sonderabdruck aus *Aerztliche Standeszeitung.* Jahrgang, 1910. No. 9, 7 p.
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THE ÆSTHETIC PRINCIPLE IN COMEDY¹

By HORACE M. KALLEN, PH. D., Harvard University

Although it is fashionable nowadays to praise the 'sense of humor,' there is a traditional rôle for critics of art which consists in deploring and cavilling at the human love of laughter. To pursue the laughable is almost invariably, according to this tradition, to sacrifice the high for the low, the excellent for the perverse. Supremacy, in art as in all walks of life, is taken to be isolated and sorrowful; beauty's majesty must wear the buskin. The marriage of æsthetic excellence with tragedy is indeed not only a legend of the elect, it is a commonplace of popular culture. The acclaimed art of our human inheritance has the power to awaken sadness; the acclaimed masters are masters of the mournful note,—Æschylus, Euripides, Michael Angelo, Dante, Shakespeare, Goethe, and who else you will,—their best is their most funereal. Nevertheless increase in humane quality may be fairly gauged by gain in the scope of laughter. While it is untrue that savages are without a sense of humor, it is true that their derision has a narrow range and fixes itself upon the more fleshly if profounder aspects of the common lot,—upon the pursuit and capture of food, upon the business of marriage and child-bearing, upon the enhancement and glory of the self. These great central interests are, no doubt, the piteous matter of amusement for civilization also, and our populace has hardly attained a wide vision of the comedy in the residual world; but it nevertheless has such a vision, and is appreciative of

¹This paper is part of the third chapter in a book—"Beauty and Use: Outlines of a Pragmatic Philosophy of Art"—now in preparation.

the range of the comic through institutions and ideas, through the sacred and the lofty, as well as through the natural and the instinctive. Civilized mankind has gained on the unsophisticated in so far as it can laugh and command where the savage trembles and is afraid, while the greatest master of life seems to be he who, like Democritus, understanding the world's nature, laughs at its manners.

A profound and vital reason exists for this human love of the comic, for this increasing power to find and to place it, for the fact that the majority pursue it, if not more eagerly, as eagerly as they pursue beauty; for the fact that the cult of the 'sense of humor' has perhaps more shrines and a greater body of worshippers than the institutional cult of beauty. The love of beauty is the love of happiness; its possession in the æsthetic experience is the joy of successful self-conservation. Beauty is the directly-felt goodness of the environment. The environment arrests you as you plod or scurry in your daily routine; it holds you, brings all the faculties of your organic self to play upon it instantaneously, integrates them, sums them, until you attain whatever enduring optimum of value the environment offers. Beauty is this optimum of value, this realized entelechy of harmonious and instant interplay in adaptation of your whole self with that particular environment. Now the behavior of the comic is much the same. It, too, comes upon you suddenly during the affair of living; it, too, arrests and deploys your life, compelling it to take hold of the comic essence it offers you, and to it also you are adapted in the instant, harmoniously, completely, directly. On the other hand, there are certain well-marked differences between the experience of the comic and the experience of the beautiful. The former seems more complex, both with regard to your own state and the condition of the object. Their elements are harder to grasp and more difficult to hold. For yourself,—you smile, at the very least; ordinarily, you laugh. For the object, there is something that corresponds to your own condition,—an uncertainty, a movement, in character and in form.

Consider these differences more closely, of course only so far as they are ordinary, healthy and normal; the trans-normal marvels of laughter are not our affair. In your own attitude the most striking point is the fact that it is an *action*; this smiling and laughing is something *doing*, and it is a doing which you love, which you prefer and persist in. To laugh is a privilege and a delight; and to be laughed at is, significantly, a degradation and a pain. It is not so with beauty; to be beautiful is even more agreeable than to enjoy beauty. This irreversible direction of laughter, well-exemplified in its

contagion, becomes still more significant when we observe its details. There is first the smile; the corners of the upper lip are drawn up, the canines and the incisors, the renders and the cutters, are laid bare, wrinkles form under the eyes, which narrow and brighten; there is a slight heightening of the respiration. There is also, perhaps, a barely perceptible outward movement of the hands. Very little is needed to pass from this smile, which may of course be reduced to mere upward twitch of the lips or a mere wrinkling of the eyes, to the quiet, audible laugh,—just a deep, not frequently noticed inspiration, then expiration in short, quick puffs, or chuckles or gurgles, accompanied by more noticeable wider expansive movements of hands and legs. If the laughter grows farther, is less restrained, then the head is thrown back as when swallowing a very agreeable morsel, the alternating inspiratory and expiratory processes grow more and more obvious and prolonged, the explosion of sounds louder, of varying pitch; the eyes are narrowed to a frown, tears come, the limbs are thrown far out, or the body sways back and forth rhythmically, in wider and wider arcs, the hands are extended and slapped together. If the occasion or witness of the laughter is a person, he may be slapped on the back, poked in the ribs, or even embraced. Withal the blood-vessels are dilated, the blood comes faster through the system, more oxygen reaches it. In a word, the general vitality is heightened, the basis of being extended. The whole phenomenon of laughter seems expansive, enlarging, vitalizing; all its movements appear as if intended to embrace and absorb their occasion.

And that occasion,—supplied by nature, created and modified by art? However it occurs, it must be given whole before it can evoke its laughing response. The maker of an unpremeditated joke does not laugh when he makes it, he cannot; he laughs like his auditors, after he has heard it, after he has taken in the comic substance for what it is. And the apparently frequent anticipatory laughter of the auditor, that is in no sense directed upon what is not yet but will be; it is directed upon a content already offered and found comic. The essential condition of laughter,—paradoxical, commonplace as it may be,—is the actual apprehension of the concretely present laughable.

This, both in nature and in art, has many forms, widely diverse, disparate and difficult of comprehension under a single rubric. In nature there is earliest of all, the eleemosynary 'laugh' of the well-fed, replete, resting child repeating in its contentment the pleasurable movements of sucking, so much like laughter, so essentially a smile. The object which especially evokes it is said to be the rise of the food in the gullet,

so that the action would be like chewing a vicarious cud. But this is the mere beginning of laughter, and its occasion is problematic. A far more certain occasion is tickling. Now tickling seems to be a pleasure both sought and dreaded. The child's responsive actions to the tickling stimulus are partly defensive, opposing, mainly expansive and embracing. It seems to contain two elements uncertainly mixed, alternating, undirected, carrying both menace and safety, with the element of safety predominating. Under favorable conditions the whole or any portion of the body responds to it. An expected contact of an unknown and thus far discomforting stimulus turns out to be a contact of pleasure and delight. There is an essential conflict and titillation between two diverse elements of which the personality-feeling, whatever that be, finally finds itself free and master.

The daily life offers many instances which are determinable as complications of the characteristic contents of tickling. The laughter which follows fear, emotional or intellectual tension, is such. So when a child laughs after having been frightened by a dog, a woman after having heard bad news or on the shock of some vision or encounter, the terrifying object has seized on the mind, disorganized it, upset its equilibrium, emotionally or otherwise, is a menace to its proper character. When for whatever reason, it lapses, when this process dies down, when the organism has, with temporary or permanent success, resisted and vanquished its enemy, the engaged energies are released, the disturbed equilibrium is restored, the organism is again in possession of itself, and in a single instant or a longer period, it does not matter, apprehends the whole of the lapsed situation with the failure of its enemy and laughs, spontaneously, instinctively. Literature affords many instances of the same thing,—the typical laughter of mad Ophelia, Hamlet's curious ironical play with the ghost:

“Well said, old mole. Canst work i' the ground so fast?
A worthy pioneer”

are instances. The preceding experience seems, so to speak, to break off and to constitute an object in which an element formerly a menace or a terror, exalted above the protagonist, has been thrown in the dust and made of low degree.

The laughter of sheer health might seem to be almost antithetical to this,—frequent, free, easy, evoked by the most trifling instances,—the sight of food, of friends, of strangers, the most ordinary events and actions. But it is not intrinsically different. Joyous though this laughter is, it is most prone to break out upon sudden stimuli, the overflowing energy of health seizes its unsuspecting object, is master of it *ab initio*,

and perverts its natural and proper relations to the world in which it belongs. The apparently meaningless laughter of sturdy children is such an action, the laughter of savages who are sufficiently familiar with strangers no longer to fear them, the very confident laughter of crowds, the careless laughter of people in power. Health, which is self-assured, stable, optimistic, finds everything grist for its mill of laughter, that is in the least different from it,—that is less stable than it. Health is literally wholeness, a self-sufficiency and completeness. The laughable, in so far as it is like tickling, is conversely not sufficient in itself, nor complete nor balanced nor stable. It seems less than health, and at its mercy.

This is perhaps nowhere so apparent as in play and make-believe. Those who have watched children at it must remember pleasantly how, wherever this play is collective, it is punctuated by continual bursts of laughter, sometimes accompanied by screams of it. Those who have questioned children about the persons and objects of their simulation, the characters they and their playthings assume, will not fail to recognize how deep a sense of the stability and reality of their customary environment children really have, and how rare are illusions on their part concerning the status of their fictions. For most of them, even the youngest, there is nothing magical or strange even in the most mechanical toys. Their sense of mechanism, indeed, seems stronger than their sense of mystery, of personality, of faerie. They do with their make-believes what suits their convenience; and what essentially suits their convenience is the domination and supremacy of the person they are. If they "play school" they insist either on being teacher, or on being victoriously troublesome pupils; if they personate characters, they insist on being the gloating all-vanquishing champion; Tom Sawyer as bold Robin Hood must kill the sheriff of Nottingham, but then Bill Harper, who was the dead sheriff of Nottingham, must also subdue Robin Hood. He cannot endure to be dead, even imaginatively. The laughter of play, then, apart from the physiological elements which like tickling depend upon titillation of expectancies, of physical contacts aimed and missed, of purposes crossed and frustrated, is a laughter directed upon an immediately apprehended difference between fiction and reality; and is the sense of vital power of control over both. In that more malicious form of play known as teasing, this becomes still more evident,—for teasing is play on the edge of earnest, pleasure on the edge of pain. Both the teaser and the teased laugh,—the teaser because he sees the contrast between the expectations of his victim and the character of his own intentions, because in that respect his

victim is at his mercy; the teased, because he recognizes the deceitful nature of his ostensible danger, because in his alarm at its on-coming he can still take it for what it is and so cause it to fall short of its intent. If he succeeds in doing so utterly, he turns the tables on his persecutor who thereby himself becomes the victim; if he fails in doing so, he becomes angered and the situation turns from fun to gravity. And with what ease, so often! A wink, a look, a word, may serve to turn a play of wit into a quarrel, a friendly game at cross purposes into a struggle for life.

Laughter, indeed, is intimately and often the clearest expression of victory in such vital struggles. The shouting laughter of partisans at great spectacular games in which their sides are successful, the wide, expansive, absorbing movements of throwing arms and limbs far out into the air, swinging hats and dancing attest this relation. It is evinced in the traditional report of the successful prize-fighter who toward the end of his combat 'comes up smiling.' Usage indicates it in 'the self-confident smile' attributed to any one who is master of an art or of a situation. Popular wisdom expresses it in the proverb 'He laughs best who laughs last.' Victory in combat of any sort whatsoever may be accompanied by laughter,—when the tension of the combat is relaxed, when the mind erects itself and surveys the event and the prostrate enemy. The laughter does not occur during the battle; during the battle there is silence, grim absorption in the business at hand. The occasion of laughter is not the combat, but the fallen in combat, the vanquished enemy, the mighty laid low, the peer reduced, the apparent strength unmasked and laid bare for the weakness it really is, while the victor remains firm, unshaken and laughing in his might.

The denudation or exposure of things, the inversion of appearance by reality before a witness whose own 'reality' remains firm, whose seeming and being are by contrast one, is indeed the basis, together with this envisagement of the defeated enemy, of the most universal matter of laughter nature supplies,—the laughter of sex. Fully nine-tenths of the witticisms of daily life, and more than half the wit of literature plays on sex. Sex is laughable because social life requires that it be hidden, set aside, submerged; while the natural endowment of man impels the instinct to raise its head out of the darkness, to peer into the light of day. This traditional throwing-off of linguistic, sartorial or customary convention causes laughter. The peasant and the boor, by use of language, do so directly,—the mere mention of matters allied to the reproductive function brings laughter; the more-trained, self-controlled, sophisticated individual is indirect.

He proceeds by innuendo, ambiguities, covert references. The submerged intent has farther to travel, more inhibitions to vanquish, in order to reach the open field of consciousness. But all classes of society laugh at suddenly discovered lovers, at amatory irregularities, directly and without thought. When they take thought they condemn them; and often, even in condemning, laugh.

Something like denudation or exposure is involved in the laughable character of novelties. The comedy of newness is almost universal. Even if the newness is circular and seasonal, it is still funny,—so the 'first straw hat' is every season an object of derision; a boy's first 'long trousers,' or first dress-coat. Savages are said to laugh continually at their first white visitor and his appurtenances; children and even adults will tease and persecute people with an unaccustomed beard, a different cut of clothes, another accent. The new is new just because it is distinctive, different, a variation from the habitual and customary. It is a little thing, isolate, against a massive tradition, a universal manner, a cumulative habit. It is a deviation from the type, a deformity like the traditionally laughable hunch-back, club-foot, magnified nose or hare-lip. At the moment of its appearance, it is at an evident disadvantage. It is an intruder, without the power to make its intrusion good. It is laughed at. To it may be assimilated the whole assemblage of little drolls which people and diversify the daily life—irruptions of irregularity, violations of the pervasive conventions which constitute the economy of social intercourse,—such are wearing the wrong clothes, using the wrong utensil, petty misfortunes, clumsiness of manner or of speech,—the whole host of disharmonies and incongruities at which we laugh. Of these the essence is the irruption of an unexpected, a new and discordant yet impotent factor into a harmonious and well-balanced situation.

The occasions of laughter, then, as they naturally arise in the events of the daily life are occasions which contain at least two elements, not in harmony with each other. In tickling we have given the dual nature of a stimulus; in terror the sudden fall or breaking-off and lapse of a dominating tension; in pure health, the weakness of other things; in play and teasing and battle and victory, the contrast between make-believe and actuality, apparent strength and real weakness; in sex and novelty, the conflict of the natural flux and the social order. In each case the occasion offers us a contrast or conflict between two elements in which the spectator does not participate. In the course of life they appear impure, adulterate with extraneous elements, not altogether detached from the residual flux. Their arrestive and vitalizing power

is restrained by other and ulterior conditions, by almost equally potent simultaneous impetus from interests looking in other directions, toward other ends. The art of comedy consists in abstracting these essentially comic complexes from their habitations in the flux, in freeing them of extraneities, and throwing them into relief. The comic of art, hence, has a rather different character from the comic of life,—it accumulates a certain desiderative value which is akin to beauty. In art, the comic might, indeed, be called the beauty of disintegration.

Although comedy has chiefly been associated with letters and the stage, there is no telling with what degree of adequacy it might not be expressed in the other arts. A limit is suggested in the fact that movement, action, invariably intensifies comic effect, but the least degree of movement required is perhaps impossible of determination. It is certain, however, that painted and carved objects are more laughable either when they are very simple, or when they occur in a progressive series. They appear either to tell stories, which need to be supplemented by verbal rubrics, or to present very obvious direct contrasts, exaggerations, novelties, whimsicalities, oddities. They involve an essential paradox which is, at one of its extremes, caricature, at another, grotesque symbolism. Animals with human expressions on their features; human beings with bestial characteristics; inanimate objects with some of the attributes of life; living beings with the appurtenances of the non-living; inverted natural proportions; and so on to no end,—these constitute the material of the plastic comic. Sculpture is one of the arts perhaps least amenable to the comic ideal. Most laughable sculpture is caricature, often caricature by accident, not by intention. The material of sculpture, in spite of modern practice and ambition, does not readily lend itself to the representation of that disintegrating essence which is the comic material. It is more adequate to the representation of repose than of action, and the movements it most successfully represents are the integrative and co-operative movements that enhance poise and stability, not those that express inner diversity and disintegration. Grotesque sculpture is not, by nature, comic; for the genuinely grotesque is the harmony of the extraordinary. Comic sculpture, when intentional, is caricature; when unintentional is maladroitness of the sculptor. That it has a larger capacity for comic expression than it has thus far exhibited must nevertheless be admitted. But such larger expression would need to be serial and cumulative, not instantaneous. It would require explanatory legend, and would approximate very closely to the comic of painting. Painting which shall

be intrinsically comic by virtue of its coloring or design is not ordinarily conceived. There is no inherent exclusion of such laughableness; the famous Schopenhauerian example of the comic,—the curve and its tangent,—indicate that in one instance, at any rate, pure geometrical form was apprehended as laughable. There is no reason why minds habituated to the apprehension of forms and colors as such should not discover an infinite deal of the laughable in them. There might be a pure comedy of design and of landscape, as well as of human feature and action. Hogarth, indeed, approximates some such thing in his ludicrous example of the consequences that follow on ignorance of the laws of perspective. But taken as a whole, comic pictures are mainly caricatures; they have a social subject-matter, and are most effective in series. Our 'humorous' literature is full of illustrations of this principle; the daily newspapers teem with them; they are the essence of the "comic supplement." They appear, significantly, to be studies of manners. The rich comedy of such series as Hogarth's 'Hudibras,' 'The Rake's Progress,' 'The Good and the Idle Apprentice' seems to lie in the cumulative integration of cross-intentions with caricature; and it is doubtful whether this integration would be so funny without the attached verbal legends, and the presence of laughing or smiling human faces. The latter constitute a very important element in the comic effect of pictures; and their presence is usually a drawback to the determination of intrinsic comic quality.

The enhancing effect of movement on comic quality indicates clearly why comedy is more frequently a matter for literature and the drama than for the plastic arts. Literature and the drama are intrinsically serial and climactic; while painting and sculpture are simultaneous and sudden. Music, the other temporal art, whose very essence is time, is not so often said to contain or to offer comic content. Nevertheless music has its distinctly comic material and method, and its characteristic comedy. This seems mainly to be provided by a combination of light, staccato instrumentation with deep-pitched notes, by the use of uncompleted phrases, and latterly by imitative natural noises like the crowing of cocks, the cries of children, the whistling of birds,—all in careful 'harmony' with the theme of the composition. That the first two devices are musically amusing may be granted. But whether the comedy of the last device springs from the nature of the art itself or from the more apparent intrusion of a foreign element into the musical complex is an open question, though barely so. But whatever the basis of the laughter, the laughter is indubitable.

In drama and literature, the nature of the mirth-provoking object is less open to question. The material of laughter is here purely human, purely relevant to complex or simple human interests. Indeed, according to one writer the human is the only material that laughter can have. This material may be internal or external; it may offer itself in the individual solely, or in the confrontation of individuals with each other or with their environments. The outer marks of the comic individual may be merely clumsiness or deformity; may be speech incompatible with gesture, gesture with speech, the merest physiological malapropism, the lisp, the stutter, the bare misuse of language. Any one of these may be amusing; all of them taken together constitute the representative comic figure, Mr. Punch. Falstaff is funny by his mere avoirdupois, Bardolph by his flaming nose, Pistol by his rhodomontade. Bring them into action, and these purely external traits may distort purpose, and throw the most excellent intention out of gear. A fat man makes a shadowy trooper; a ranting rascal cannot tell a straight story.

But this derailing of a swift-moving intention need not depend merely upon the external characteristics of the comic protagonist. Loosely interpreted, it is the essence of every comic situation, which is in Aristotle's excellent simile "in the nature of the missing of a mark." The situation is created by the fact that the characters do not hit it off. Its clearest type is perhaps Mr. Pickwick chasing his wind-blown hat. The situation has come upon him suddenly, out of the blue. The orderly march of his life has been broken up. His hat, which properly belongs on his head and should protect him from the wind and weather, has betrayed him to the wind and weather; and to add insult to injury, leads him a sorry dance away from his proper affairs, for the purpose of restoring the disturbed balance without which they do not easily go on. The hat must be back on the man's head before the man can return to his business. This is very laughable; but normally the laughter is killed if the man is compelled to return hatless to the routine of his life. Where hatlessness begins, tragedy begins; and this is a very significant feature in all comedy. The hat may not be utterly lost if the laughter is to be saved.

The hat-hunt runs over us from practically every cranny of the comic scene. Its principle is an inversion of the ordinary,—an inversion shocking, fresh and unexpected. Instead of a trick or perversity of things, it may be an encounter of limbs or persons. The runner who trips over his own feet is funny; but the clown whose running is brought to a sudden stop by the identically similar running of an identically

similar clown is funnier. The classic comedy, so well represented by the 'Comedy of Errors,' is based fundamentally upon this sort of inversion,—the kind of inversion that a person undergoes in a mirror. He is there, he is himself; yet he is not there, he is another, opposed and inimical. The alter-ego is the source of the deeds for which the ego suffers or is rewarded. The Syracusan and Ephesian Dromios are so related in practical life that the mere mirrored image of the one, having a different history, different antecedents, and a different status pays for the defects of the other. It is as if the image in the mirror were beaten for the impudence of the grimace it reflected. It is the "sudden glory" of the insignificant, the irruption and domination of the irrational.

Still another variant of it is the direct inversion of catastrophe, as the sudden and unprophesiable ups-and-downs of Face and his crew in the 'Alchemist,' the reversals of Epicoene, the inversions of the 'School for Scandal.' This is so obvious that more than to mention it is superfluous. The persistent repetition of such an inversion, always reconstituting the same situation, is another typical mode of the comic process. The battle between Punch and the devil is its key-form. Punch strikes the devil down with a blow that should deal him his eternal quietus; and the obstinate devil rises unharmed again and again and yet again to return to the attack as horrible as ever. Or perhaps the condition of the protagonist is that of the jumping-jack. Its limbs appear to move so spontaneously, so freely, so irresponsibly, while in reality they obey the inexorable leverage of strings and pulleys. I cannot think of a better instance of this type of inversion than Malvolio, so apparently pursuing his own freely-chosen purpose, so clearly the dupe and the toy of Maria and her fellow-conspirators. The comedies of Ben Jonson are full of such types, from the La Fooles, the Dappers the Druggers, to the Voltores and Moscas and Volpones.

Seek where you will in the comic of the stage or of letters, and invariably you will find something corresponding to one of these forms of inversion. If it is the comedy of mere incident, it will consist of the irruption of the unusual, an upset or reversal, of some sort, in nature essentially a disharmony like that of the man chasing his hat. In the comedy of manners, one finds private habit opposed to public usage, the mode to good sense, the individual preference to the social sanction: the comedy consists of the titillation, the see-sawing of the one with the other. In the comedy of character one finds no less the same thing, with another emphasis. The individual idiosyncrasies which are the deep-sunk well-springs of motive, pressing up action after action, with inexorable con-

sistency, are exhibited in conflict with social norms and conventional preferences. Here we are face to face with the comic object whose ludicrousness is internal first of all. It is the source of all else that is laughable, infecting with its distortions all that it touches. The comic of character is the internal homologue of the comic of person. It is founded on the internal disharmony of traits, on malproportion, moral deformity, as the other is based on physical deformity. The theory of humors, on which Ben Jonson has based all his comic pieces, fantastic and untrue though it be, has nevertheless grasped the secret of ludicrous character. It offers as the standard excellence the nature in which each of the four humors is present in right measure, just sufficiently choleric, phlegmatic, sanguine and melancholic to be of nice balance, poised for any flight you will. But change the proportion of any one of these humors, and you upset this excellent balance, and destroy the fine poise. The greater humor is at war with the others, perverts them to its own uses, interferes in their business, and ultimately breaks up the nature it distorts. The inner disharmony is expressed outwardly in a thousand ways, and this outer expression is comedy of character. Now multiply these humors a thousand fold, consider the relation of any one of the numberless preferences, habits, desires, intellections, tricks of speech, manner and gesture as well as of soul, to the remainder, and you cannot help seeing that this relation is identical with the relation between the weightier humor and the others. It is a combat, a distortion, a disintegrative maladjustment. The consuming passion for silence in Morose, the self-conceit of Malvolio, the didacticising stupidity of Polonius, the avarice of Harpagon, the magniloquent aimlessness of Mr. Micawber, the hypocrisy of Tartuffe, the subtly rigid self-worship of Willoughby Patterne, and I care not what other trait of what other person you will,—each is a trait which is comic only because disproportionate, and hence, wherever it appears, disorganizing. Harpagon loses his wealth because he loves it so; and, by the way, is made altogether ridiculous because his moral deformity intrudes and operates where it should not. Had Shylock loved revenge less, he would have suffered less; and Malvolio, certainly an efficient steward, had nothing but his cancerous self-love to thank for his degradation and misfortune. Hypertrophy of imagination over common sense in the Knight of La Mancha, the atrophy of imagination in Sancho, the fleshly weakness in Falstaff,—such are the fountains of comedy in these heroes of the sock. Whenever any one quality is called into play, this forestalls it, snatches its action from it, or spoils it by its influence. Perhaps all comic traits are no more

than the love of life, the instinct for self-preservation, no more than the spontaneous and natural egoism of mankind, taking a perverted direction, so eager to live well as to belie fantastically the most fundamental conditions as well as the most subtle of right living. The greatest of all ruinous misproportions is, of course, that of self-deception. Invariably by its means diverse social and natural antagonisms are exhibited and made explicit, whether in the adventure on Gadshill, the wind-mill tilt, the tantalizing dinner, or the cross-gartering. What "moves men merrily" is the far-spreading infectious disharmony.

This patent malproportion in character which is the prime source of comedy has led to an opinion, variously held, that the comic figure is an abstraction, that he is less individual and more 'universal' than the protagonist of tragedy; and that the function of comedy is that of social correction. There are some grounds for this inference. The practice of the Greeks in the use of types and type-names,—names like Phidippides, Dicæopolis, Mania of the Aristophanic comedy, or Glycerium, Palaestra, Bombomachides of the Plautan comedy, the Voltres and Corbaccios, the La Fooles and Moroses, the Mammons, Subtles, Faces of Jonson, the similar practice of his successors far into the eighteenth century, attest that dramatists seemed to be dramatizing moral qualities and types rather than persons. The very titles of the comedies: "Wasps," "Birds," "Volpone," "Epicœne," "L'Avare," "Les Precieuses" bespeak traits rather than persons. But moral tragedies like "Everyman" and "Ghosts" are no less typically and abstractly named; and there is scarcely a tragic character that cannot, as properly as any protagonist of comedy, be labelled by the peculiar trait which constitutes his tragic nature. In point of fact, comedy has no monopoly over these forms of art in the chastisement of the anti-social. And what, moreover, is anti-social? A convention, a mode or habit which has attained universality is as often the object of laughter as an isolated individual, a group as often as a habit. And these are as frequently condemned by tragedy as by comedy. Satire and irony, indeed, are correctives. But the corrective principle of these is not their comic quality, but their tragic earnestness. Satire is a battle, not a joke; comedy turns the battle into a joke. Where comedy becomes corrective it is no longer truly comic. For the subject of a joke there can be no sting if he is to laugh; and if it stings he cannot laugh. The laughter can have no portion in the ruin which moves him to mirth.

That it is a ruin which moves to mirth, and that the merry man must have no share in it, is most patent in the comic of

words. Civilized comedy is at its highest in words. These alone can render the very refinements of mal-adjustment, the delicate disharmonies of the spirit. They reveal the range of battle between mind and mind as nothing else can. Yet what target of a poisoned verbal dart ever responded to the impact with laughter or admired the accuracy of the aim or the sharpness of the missile? Invariably his first action is the aggression or withdrawal of defence. A return shot, scornful silence,—but no broadside of laughter. The play of wit has always imminent over it the play of the sword. The quip becomes the stab with a turn of the hand, and this just because the object of witty play is a ruin, or like to be one in that play. Recall by way of example that superb witticism of Heine's at a certain Parisian *salon*, where he, Soulié and an enormously wealthy *parvenu* were guests. The *parvenu* naturally received more attention than the two men of letters,—which moved Soulié to remark: "Even in the nineteenth century, they still worship the golden calf." "Yes," assented Heine, "but this one is much older." This characteristic Heinesque remark makes of its subject an ox; and 'ox' is the German *Schimpfwort* for stupidity, dullness, maladroitness. To call a man an ox is to insult and to degrade him; it is, by a stretch of meaning, to ruin his reputation for intelligence, to destroy his human dignity, and to make him like the beasts of the field. This Heine has done. Moreover, he has not done it by a direct aggression. He has ostensibly referred only to the age of the *parvenu*; he has ostensibly even defended him against attack. He has said nothing overtly insulting, yet he has called the man a calf of advanced years. In the phrase, "much older," therefore, there are two ideas not compatible, not belonging together, titillating the attention of the auditor. And this enhances the excellence of the witticism. Of wit which is impersonal, which is the play of ideas as such, and has no moral lilt against another person, the essence is this unstable union of thoughts, this conflict, incongruity, crush, and interference of two or more ideas, struggling for place in one word. The pun is, of course, the most obvious example of this fact; but it may be brought about in many ways,—by a slight difference in emphasis, metonymy, inversion, metaphor. Invariably there is an ambiguity between denotation and connotation, between figurative and literal meaning, which is the soul of the *double entendre*, as well as of the bald disharmony of ideas or of objects. Much of its quality is evident in the reply of one soldier to another who had called his attention to the bold escape and the immediate pursuit of a spy. "He is running for dear life" said the one, and the other replied "He'll

never buy it at that rate." It will be seen here that the literal and the figurative intention are jammed together in a strange and not incongruous contact. The pleasure and the taste of it are due to the jam. Still more potent does this become where there is no relevance whatever between objects, as in the wide perversion of the Twainesque humor, or in the attempt of the Scotchman to make his friend understand the meaning of 'miracle.' Tam had tried hard to teach him, but with ill-success. Finally he resorted to parable. "Look ye," he said, "when ye see a coo sittin' doon, that's no' a mirrecl. When ye see a thistle standin' up, that's no' a mirrecl neither. An' it's no' mirrecl when ye hear the throstle whistlin' in the tree. But when ye see the coo sittin' on the thistle, and singin' like the throstle, that's a mirrecl mon, that's a mirrecl." The incongruities are here obvious. Their violent refusal to hang harmoniously together is the strength whereby they "move men merrily." A most subtle form of it is the famous "'T was brillig . . ." in 'Through the Looking Glass.'

From the coarse and obvious comedy of the clown with his falls and tumbles, to subtle and recondite plays of wit the material of the laughable remains invariably a disharmony, a maladjustment ranging from the impact of bodies to the clash of souls. No less do the depth and scope of philosophy, where surely there should be little place for laughter, offer the great and eternal disharmony, a spectacle which, as poets have more than once sung, moves the gods merrily. But men are so moved no less than gods. The cosmic vision may stir the thinker to cosmic laughter. History offers us one strange and wonderful figure, isolate among his kind, whom tradition names "the laughing philosopher." Democritus of Abdera saw the great contrast between man's hopes and his condition, his conceit of himself, his belief in his own power, his headlong passion and pursuit of his petty ends as though they were the world's will and the world's purposes, as though his struggle were the cosmic joy and sorrow. But the cosmos is a void, and a hurly-burly of atoms. Against the volume of their inexorable tumult, man's cries are as utter silence; against the background of their fatal onward rush his willings and achievings, but the uncouth jerkings of the jumping-jack's limbs when the strings are pulled. Man is the ruined victim of his own illusions. His destiny is death because it is self-deception. Therefore Democritus laughed. Laughter, cheerfulness, *εὐθυμία*, is a restoration of the true proportions. It rests upon a recognition of the narrow limits and the eternal conditions of human well-being. It is a turning of destiny to scorn by accepting it, as one destroys the sting of rebuke and the violence of anger by offering them no

resistance. They are turned to derision because they are spent on a void, losing meaning and purpose. Thus the laughter of the sage is a double laughter. Its subject is the self-deception of man which combats the inexorable cosmos, but its subject is also the rage of the cosmos spent upon nothing at all. In both cases there is an inversion, a disintegrating disharmony, outside of which the sage stands, and the master of which he feels himself to be. He is upon the Lucretian rock, watching and enjoying the storm and the shipwreck below.

The range of the comic scene, we gather, is no less than the cosmos itself. The occasion of the laughter may be the compass of one small baby's toe, or the unbounded universe. It plays over the whole gamut of human relationship and cosmic disharmony. Nothing may escape it, from the attenuated malproportions of abstract mathematics to the terribly weighted deflections of the universe. But of laughter two things seem true. The first is the fact that it cannot endure. Custom kills comedy. What is habitual, what we are well-adapted to, what is for a long time a part of our own lives cannot move us merrily. To do so, it must exclude us, make us foreign to it. It must become something in which we no longer have a portion, and which for the time, has no portion in us. The traveller is likely to feel this most keenly; that is, if he is a laugher, rather than a creator of laughter. The creator of laughter, the professional humorist, can scarcely be a laugher. He is not a humorist because he sees the comedy in things, but because he twists things and distorts them so as to make them comical. He is invariably a preternaturally solemn person. Laughter must be free, but the cause of laughter is always bond. The maker of the laughable is the servant of his vocation; he cannot laugh and render service at the same time. The laugher is served, but serves not. Hence, then, the traveller who can laugh finds all things in a new country ludicrous at the beginning. Customs and modes, habits of life and manners, the very scenery move him to laughter. But as his stay is prolonged, the disharmonies seem to rub off; the articulation of life becomes smoother and less noisy. He himself has now become, to some degree, a part of the structure; speech, manners, dress,—his own have somehow become confluent with them, have set him at their centre, where he once was at the periphery. He can no longer laugh; nor can he understand his original laughter. This process is true no less of an oft-repeated game, a witticism, a relieved nervous tension or a philosophy. Familiarity breeds seriousness or indifference before it breeds contempt. The second characteristic of laughter is that it

enhances or preserves the laughter's implicit values, not always obviously or directly, but invariably. The outcome of the comic situation is an alterative outcome, not a destructive outcome. The disintegration which is the object of laughter leads to re-distribution, re-adjustment, harmony, not to real human loss. The upshot of any comedy shows a harmony attained by attrition and elimination of excrescences, by the reduction of the evil, by a restoration, even if only a momentary one, of things to their normal,—one may even say, to their normative,—relationships. The inversion of the natural order in which most comedy begins, proceeds in the course of the action, by the mere inertia of the comic disharmony, to right itself. Don Quixote is led by the effects of his madness to realize and see it truly. Harpagon is led by the operation of his avarice to comprehend its evil nature; Willoughby Patterne loses some of his self-love, Volpone passes from his dishonorable bandages to his more dishonorable chains. The new harmony may not be enduring, but it ends the comedy. And it is, of course, true that not always are the normal social standards re-asserted and the habitual conceptions of virtue victorious. In *Epicoene* the punishment of Morose is to our modern sense perhaps harder than his deformity of spirit deserves; the enrichment of Sir Dauphine by a swindler's trick, our contemporary moral sense will hardly stomach. But, notoriously, nothing is so variable as the actual social standard of mankind from period to period. Whenever we look more closely at the post-comedial harmony, we find that the standards of the age to which the comedy belongs have been vindicated. The standards of all time have little to do with comedy. It is sufficient that any prized thing shall be preserved or enhanced, that any distortion or evil shall be destroyed or decreased, even if for the moment only, not alone in the drama but wherever the comic occurs in sculpture, in painting, in the events and routine of daily life. The hat-chaser must recover his hat if he is to remain merely a comic figure.

Considering all of these facts together, what do they yield as the æsthetic principle in comedy? What is there identical between the tickled toe of a suckling infant and the philosophy of a Democritus? Students of the comic have given this question widely varying answers. There has been perhaps as much confusion in the definition of the comic, as in the definition of the beautiful. Theories may be roughly divided into three classes, yielding a certain minimum of unanimity.

The first group of theories may be called "degradation theories." They conceive the object of laughter as reduced in worth; and the laughter as enhanced therein. As Hobbes

has it: "Laughter is nothing else but sudden glory arising from some sudden conception of some eminency in ourselves by comparison with the infirmities our own formerly. . . . (It) proceedeth from a sudden conception of some ability in himself that laugheth. Men laugh at the infirmities of others, by comparison of which their own abilities are set off and illustrated." Laughter here is self-enhancement at the cost of one's fellow. The self-enhancement is as important as the degradation of the other. Other writers, however, take only the degradation to be significant. So Bain finds the "occasion of the ludicrous" to be "the degradation of some person or interest possessing dignity, in circumstances that arouse no other strong emotion." The dignities, moreover, must not "command serious homage;" and Groos finds the comic object to be one in a topsy-turvy condition, and hence regarded with a feeling of superiority. But for all three the object of laughter has in some way been reduced from its high estate. Something of the same sort may have been in Spencer's mind when he wrote that laughter naturally comes when there is "a descending incongruity," a turning from great things to small, a degradation.

The theory of degradation fails, however, to square with the obvious fact that degradation is a matter of geography, inclination, breeding and incidental affection. As one man's meat is another man's poison, so what may seem degradation to one may be exaltation to another. The mental state of the laugher is hardly one which feels the sentiment implied in degradation. It does not seem, in most cases, to possess what the Germans call *Tendenz* or *Schadenfreude*. As comic sense it carries detachment and freedom rather than malicious intention. The correct envisagement of fact which the theory offers is more simply because more freely offered in those explanations of the comic whose key-word is "contrast." The "contrast" theories emphasize differently the elements contrasted, but their intent is the same throughout. One author finds the contrast to consist in the complete exposure of weakness through the presence of a superior power. Schopenhauer sees it as the "unexpected subsumption of an object under a conception which in other respects is different from it." Hence he infers that "the phenomenon of laughter always means the sudden apprehension of an incongruity between such a conception, and the real object thought under it, thus between the abstract and the concrete object of perception." Bergson finds it in the opposition of the suppleness of life with the stiffness of mechanism, the substitution of one for the other; Freud in the release of repressed and submerged—chiefly sexual—complexes. And there are many other ways

of specifying contrasts. But they are, it will be seen, no more than specifications; their common element is the "contrast."

The contrast theory of the comic defines the comic by considering its objective nature. Aristotle's description of it as "in the nature of a missing of the target" stands between this objective description and the more directly psychological theory of Kant and his followers. This theory might be called the theory of "disappointed expectation." "Laughter," writes Kant, "is an affection arising from a sudden transformation of a strained expectation into nothing. . . . A jest must be capable of deceiving for a moment. Hence when the illusion is dissipated, the mind turns back to try it once again, and thus through a rapidly alternating tension and relaxation, it is jerked back and put into a state of oscillation." The first of these Kantian suggestions is hardly more than paraphrased by Lipps for whom "the comic arises, if in place of something expected to be important and striking, something else comes up (of course under the assumption of the ideas we were expecting) which is of lesser significance." The other half of the Kantian description has been more popular. We might call it the "oscillation theory" although it is essentially a form of contrast. It has received the endorsement of Hecker and of Wundt, and has been attached by them to the term "contrast."

The variations in these fundamental notions are innumerable. Writers have found the comic to be only that which violates social usage, or only that which conflicts with established moral, intellectual or æsthetic standards. The net result of a review of all of these theories is that they are all true, and in so far as they deal with unrelated facts, all exclusive of one-another. They are *specifications* of comedy under special conditions and in various fields. They contain the essence of the comic; but they have not really isolated it. Our journey through the field of laughter has shown us that this essence may reside anywhere in the universe. It is not confined to human beings or to social norms, as certain authors believe; nor is it limited to the merely living. Its habitat is as wide as experience. It ranges from the tangent which so stirred the jocund Schopenhauer, to the universe which amused Democritus. As anything may be beautiful, so anything may be comic. It becomes comic, as all the comic objects which we have examined have shown us, and as the theories of the comic which we have considered obviously affirm, when somehow it is at a disadvantage, out of proportion, mal-adjusted. It becomes comic when it constitutes a disharmony. This disharmony is the basis of contrast, the cause of oscillation, of

disappointed expectation, the essence of degradation. But by the mere fact of being a disharmony the object is not yet comic. The daily life and the arts offer the mind an infinity of disharmonies which are either tragic or indifferent. Intrinsicly, things are no more comic than they are beautiful. The comic, like the beautiful, is not a property which things possess, but a relation which they bear to the mind. We do not laugh at a thing because it is funny; it is funny because we laugh at it.

An examination of the nature of laughter itself will show us that which more specifically constitutes comedy. We have found laughter to be a wide-ranging action, corresponding to the active character of its object. But this action does not have the purposeful, rapt nature of other human activities. It seems to be a detached and free thing,—a thing which is leisurely and secure. Even when it ensues upon absorbing fear, upon the madness of anger, the anguish of passion, it seems to have this liberty and security, this leisure, as opposed to the precedent breathlessness and extreme intentness. It seems indeed often to be a cry of freedom, of relief, a roudade of triumph. When we seek the earliest semblance of an apprehension of the comic, we find it in the replete child, repeating the pleasurable act of sucking. Its normal expression in the smile requires the baring of the rending and cutting teeth, the assumption of an appearance which, when well-considered, bears a startling resemblance to an animal about to rend and devour its prey. In the hungry beast of the jungle, that has fought for its life in a double sense, and has triumphed in its struggle, may lie the ultimate parentage of laughter. The explosions of breath, the gurgitations, the throwing back of the head as if to swallow, the sprawling, expansive movements of the limbs,—those are actions that beasts still perform when they have their prey completely at their mercy. And this prey, up to the moment of possession, was a peer. The struggle to live matches not kind with kind, but every kind with all other kinds; its may be a contest of strength against swiftness, ear against eye, eye against nose. And the struggle invariably carries its essential hazard which makes even the weakling his enemy's peer. There is therefore the inevitable absorption and tension and breathlessness. In no matter how unequal a combat, there is even for the victor one moment of dread and menace, and there is the final triumph and relief in laughter. The primeval laughter is the triumphant beast, with its paw upon its defeated enemy, and its jaws set for the act of devouring. The first laughter is life's earliest cry of victory over the elemental world-wide enemy that wages the titanic battle with it. Laughter is perhaps a mutation

from feeding, and it serves the same result: it strengthens life by heightening its vitality. Its scope has expanded as the world has expanded. The laughter of man has all things for its object,—all things that may enthrall him or do him hurt, in whatever sense. It 'degrades' them, makes them man's proper food; it contrasts them with what they were; it destroys their power over him. He stands outside and beyond them; they cannot touch him. The object of laughter is ridiculous, not in so far as it is good, but in so far as it is dangerous. It is the frustrated menace in things, personal, social or cosmic,—that moves men merrily, when their power for evil is turned to emptiness. The novel, the dark, the cancerous in the life of the spirit and in the life of the body becomes ridiculous when we recognize that it is ineffectual. And conversely, to turn a thing to ridicule is to make it ineffectual, to throw it out of gear, to rob it of its place, to compel it to spend its energy in a vacuum. This is true degradation, and the laughter in it is not appreciation but malice. It is for this reason that even to so intelligent and sympathetic a student of the comedy as Bergson or Meredith, comedy seems to be a social corrective. But they fail to see that the comic force lies not in the correction, but in the joy of the corrector. There is always the possibility of a certain cruelty in comedy, an utter brutish joy in victory which is ethically more outrageous than the thing it destroys, until one remembers that what laughter consumes, laughter first finds evil.

This observation yields the key to the right definition of comedy. Beauty, it has been noted, is the relation between the mind and the environment when the two are adapted to each other harmoniously, perfectly and immediately. And the environment which beauty presents to the mind is good in itself, an intrinsic and direct excellence. Now the environment which comedy presents to the mind is primarily an evil, full of discord and unrest. This evil comes to us, however, not as our peer, but as our slave, bankrupt and stripped of its power to harm. And to it, as to the thing of beauty, we are adapted directly and instantly. Comedy, then, like beauty, is a relation, but it is a relation in which we are harmoniously and completely adapted to what is in itself a disharmony, a mal-adjustment. It is a relation which converts evil into goodness. It adapts us adequately to disharmony and mal-adjustment, snatching as it were, life's victory from the jaws of death itself.

CONSCIOUSNESS IN RELATION TO LEARNING

By LOUISE ELLISON ORDAHL

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I. INTRODUCTION

The question of the relation of consciousness to learning, in the case of man, was suggested by the disagreement among psychologists as to the value of "ability to learn" as a criterion of consciousness in animals. From a purely metaphysical standpoint, those who accept the doctrine of psychophysical parallelism would admit consciousness of some sort as an attendant of the activities of all animals. Those who attack the problem from a purely empirical standpoint¹

¹Bethe, A.: Die anatomischen Elemente des Nervensystems, und ihre physiologische Bedeutung, *Biol. Cent.* 1898, XVIII. pp. 863 ff.

Nuel, J. P.: La psychologie comparée, est-elle légitime? *Arch. de psy.*, 1904. V. p. 320.

Ziegler, H. E.: Theoretisches zur Tierpsychologie und vergleichenden Neurophysiologie, *Biol. Cent.*, 1900, XX. p. 1.

either attempt a complete explanation of animal behavior from the physiological side without the assumption of mental qualities, or else they argue that consciousness is present only when the animal is able to profit from experience, or to profit from it rapidly enough to argue the presence of a psychic resultant of former experience.¹ Still others assume that consciousness may be present in all animal forms; but the power of associative memory is a measure of its grade, or a proof of its existence.² Those³ who deny the possibility of a comparative psychology are met by the answer that the ascription of consciousness even to human beings rests upon inference and assumption, no mental states being capable of proof but our own.⁴ Before considering the question of the possibility of learning without consciousness, and the relation of learning to consciousness,—which are the main themes of this study,—we shall consider briefly a few definitions of the term “consciousness,” and the related terms “unconsciousness,” and “subconsciousness.”

II. THE CONCEPTS OF CONSCIOUSNESS, UNCONSCIOUSNESS AND SUBCONSCIOUSNESS

Practically all admit that “consciousness,” being an ultimate, is incapable of definition; yet it has been variously described and explained.⁵ For Descartes it was equivalent to self-consciousness; Wolff was the first to give it the meaning of “ultimate property of the soul;” while others consider self-consciousness to be only a particular form of consciousness. For Lipps it is identical with the ego.⁶ Usually it is broadly an equivalent for awareness or experience, and an opposite to the unconsciousness of coma, fainting, dreamless sleep, etc. Some writers make it synonymous with attention, or a general term for that experience of which attention

¹Loeb, J.: *Comparative Physiology of the Brain and Comparative Psychology*, New York, 1903. p. 12; p. 118.

Washburn, M. F.: *The Animal Mind*. New York, 1908. p. 33.

Romanes, G. J.: *Animal Intelligence*, New York, 1883. p. 4.

²Wundt, W.: *Grundzüge der physiologischen Psychologie*. (Fifth Edition.) Leipzig, 1902. III. pp. 324 ff.

Romanes, G. J.: *Op. cit.*, p. 4.

³Ziegler: *Op. cit.*, p. 2.

Nuel: *Op. cit.*, p. 343.

⁴Yerkes, R. M.: *Objective Nomenclature, Comparative Psychology and Animal Behavior*. *Jour. Comp. Neurol. and Psy.* 1906. XVI. p. 383.

Forel, A.: *Ants and some other Insects*. (Trans.) Chicago, 1904. p. 2.

⁵See Horwicz, A.: *Psychologische Analysen*, Halle, 1872-1875, for review.

⁶Lipps, T.: *Leitfaden der Psychologie*, Leipzig, 1909. p. 6.

is only a high degree, or with selective consciousness. Defined as "meaning" it is almost equivalent with apperception. Memory and consciousness are sometimes identified, but on the other hand, memory and unconsciousness are also identified, memory being regarded as a function of all matter, and perfect memory a characteristic of automatized acts unconsciously performed. Consciousness is frequently an equal term with "experience" and "psychic." It describes the "sum-total of our mental experience."¹ It is the interconnection of our psychic processes,² or a series of ideas connected with each other.³ It is "co-ordinated psychic activity,"⁴ synthesis, change, or an "orderly succession of changes."⁴ It is characterized by the pursuance of future ends, and is a synthetic unity.⁵

Consciousness is supposed to accompany only afferent impulses sent in from a moving organ,⁶ or "neural processes of peculiar organization," or complex constellations of neurones.⁷ It is supposed to arise "only when the motor cells are ready to discharge toward the periphery,"⁸ when the sensory impression is being followed by the motor reaction;⁹ "it involves not only the sensory side, but the motor discharge."¹⁰ "It varies with the novelty of the neural processes concerned, and accompanies new connections;"¹¹ it "attends new complex functions."¹²

Since any definition of consciousness touches either directly or indirectly upon the disputed question of "uncon-

¹Wundt: Grundzüge, III, p. 321.

²Wundt: *Ibid.*

Titchener, E. B.: An Outline of Psychology. New York, 1908. p. 13.

³Calkins, M. W.: An Introduction to Psychology, New York, 1901, p. 150.

⁴Marshall, H. R.: Instinct and Reason, New York, 1898, p. 43.

⁵James, W.: Principles of Psychology, New York, 1890, I, p. 8; p. 139.

⁶Morgan, C. L.: Introduction to Comparative Psychology, New York, 1906. (Second Edition.) p. 154.

⁷Morgan, C. L.: Animal Behaviour, London, 1900. p. 45.

⁸Sidis, B. and Goodhart, S. P.: Multiple Personality, New York, 1905. pp. 3 ff.

⁹Kirkpatrick, E. A.: The Part Played by Consciousness in Mental Operations. *Jour. of Phil., Psy. & Sci. Methods*, 1908. V. pp. 421-429.

¹⁰Breese, B. B.: On Inhibition. *Psy. Rev. Mono. Supp.*, 1899, Vol. 3, No. 1, pp. 1-65.

¹¹Maudsley, H.: The Physical Basis of Consciousness, *Jour. of Mental Science*, L, 1909. p. 12.

¹²Münsterberg, H.: Grundzüge der Psychologie, Leipzig, 1900. I. 531. ff.

¹³McDougall, W.: A Contribution towards an Improvement in Psychological Method, *Mind*, 1898. N. S., VII, p. 366.

¹⁴Royce, J.: Outlines of Psychology, New York, 1903. pp. 81-2.

scious'' or ''subconscious'' mental processes, a brief discussion of the two terms is expedient.

UNCONSCIOUSNESS

''Unconsciousness'' is a negative term denoting the opposite of consciousness. It is employed in describing states like fainting, epilepsy, or dreamless sleep where every mental quality is wanting. It is also applied to psychophysical processes which lack their normal conscious accompaniment, as pain or deep emotion temporarily forgotten during great excitement, and to perceptual processes such as the unconscious inference of depth from the fusion of the two retinal images, and the perception of a clang of definite quality from the fusion of partial tones which analysis alone discloses. Automatic acts are ''unconscious.'' The word is also loosely used for ''unreflective,'' ''unintentional,'' or ''inattentive.'' ''Unconscious mental process'' may have various meanings. It may indicate the physiological process correlated with a conscious process, or a physiological process with no psychic accompaniment, but determining consciousness later, or a neural process with psychic accompaniment of which for some reason the individual possesses no awareness. These uses will be discussed later. Whatever the metaphysical implications, the term when carefully used is a limiting concept opposed in its significance to ''consciousness.''

SUBCONSCIOUSNESS

''Subconscious'' is more ambiguous in meaning than either of the two preceding terms, and though it is frequently interchangeable with ''unconscious,'' it usually implies something more definitely psychic. It denotes (1) the forgotten, (2) the purposeless, (3) the unnoticed, (4) the mechanized, (5) the reproducible, (6) the productive, (7) the psychic real.¹ It is also used to describe (8) simultaneously existing secondary streams of consciousness thought to appear in the pathological phenomena of divided personality, (9) and dissociated states which some writers believe to be synthesized into a subliminal, submerged self and to constitute a large part of mind.² The first three uses of the term subconscious (or unconscious) are descriptive of facts of experience, the 5th, 6th and 7th are metaphysical interpretations of results whose causes are unknown, and the fourth and fifth are a mixture of facts and

¹Hellpach: Das Unbewusste, *Centralblatt f. Nervenheilkunde u. Psychiatrie*, 1908, XXXI, pp. 65-66.

²Summary by Prince. A Symposium on the Subconscious. *Jour. of Abnormal Psy.*, 1907-8, II, pp. 69 ff.

interpretation; the last two uses are descriptive and explanatory of anomalies of consciousness and will be considered later.

Used in the sense of unnoticed or unattended, "subconscious" (or "unconscious") belongs to the realm of experience and is used in the sense of "least consciousness." As such no metaphysical implications are involved. But it is an easy step from dim consciousness to complete loss of consciousness, and many authors make the transition, including, as psychic, processes beyond the point where we are even dimly aware of them. Discussion as to the psychic nature of processes of which we are not introspectively aware began with Leibnitz¹ and the later treatment of Hamilton, Mill, Brentano and Carpenter. The use of the conception in explanation of the pathological phenomena of multiple personality, hysterical manifestations, hypnotism and other anomalies of consciousness has more recently brought the question into prominence again. The following are most of the representative arguments for the hypothesis that the mental life is wider than experience, with the opposing views:

(1) Total perceptions must be composed of an infinite number of infra-conscious sensations. The roar of the ocean is made up of the imperceptible sound of each wave, the greenness of the forest of the color of each separate leaf.² "But," it is answered, "this is not necessarily so, for a sum of magnitudes differs from its parts, not merely quantitatively, but qualitatively." A lesser degree than zero changes water not partially, but completely to ice.³ A sufficient quantity of the cause may be necessary to produce any of the effect.⁴ The infra-sensible stimuli affect the nerve and help the birth of the sensation when other stimuli come, but it is a matter till then of the nerve-cell only.⁵

(2) By far the larger part of our spiritual possessions are not in consciousness, but are the forgotten memories, unconscious habits and the results of early experiences. That these are really concerned is shown by cases where delirious patients speak the forgotten language of early childhood,

¹Leibnitz: *New Essays Concerning Human Understanding* (trans.), N. Y. 1896. pp. 47-52.

Hamilton, Sir Wm.: *Lectures on Metaphysics*, Edin., 1877, Vol. 1, p. 339 ff.

Mill, J. S.: *An Examination of Sir Wm. Hamilton's Philosophy*, London, 1878, Vol. 1, ch. 8.

Brentano, F.: *Psychologie*, Leipzig, 1874, Ch. 4.

²Leibnitz: *op. cit.*, pp. 47-52.

Hamilton: *op. cit.* p. 339 f.

³Brentano: *loc. cit.*

⁴Mill: *loc. cit.*

⁵James, Wm.: *Principles of Psychology*, N. Y. 1902. Vol. 1, p. 159.

and idiots remember things they have heard but not understood, also the oft cited case of the ignorant servant who quoted in delirium passages of Hebrew she had heard her master read. Contrary opinion contends that not the ideas, but the power of reproducing them remains latent, physiological modifications only being concerned. Understanding what one hears is different from consciousness, and probably the idiots and the servant were aware of the sounds, but not of their import. In memories, not the concept is conserved, as Herbart believed, but "molecular habits of the brain."¹ "Unconscious psychic dispositions" would have to be assumed just as much for newly produced as reproduced ideas.

(3) Habitual actions which are at first entirely conscious become mechanized and unconscious. Complete activities formerly requiring a voluntary initiation of every act in the series, now run on of themselves when consciously started. One party holds this fact to be evidence that the psychic side exists unconsciously; the other, that the process has become merely a matter of physiology.

(4) Unconscious psychic processes are manifested by results existing in consciousness—results whose underlying processes are entirely unknown to us. Examples are the perception of depth from the fusion of simultaneous double retinal images, the perception of direction of sound by its relative intensity in the two ears, and other judgments or inferences spontaneously made without consciousness of the reasons or of the underlying principles. Also, there are sometimes present in the after-image details which were not seen in the original image.² Against the last example Brentano urges that the after-image is really due to a prolongation of the physiological excitation. In the original perception consciousness was occupied with something else, but is free later so that the unnoticed phase makes itself evident in the after-image. A similar argument, *i. e.*, that details of which we were unconscious in the original experience are frequently observed later in the memory image, might be met by the explanation that such details were actually present, but were not in the focus of attention.

A physiological explanation is thought to meet the next three arguments.

(5) By turning our attention to something entirely different we are frequently enabled to recall a forgotten name, etc., which has been supposed to prove that an unconscious pro-

¹Münsterberg, H.: Grundzüge, d. Psychologie, Leipzig, 1900, pp. 223-224.

²Helmholtz: Handbuch d. physiologischen Optik. Hamburg, 1896. pp. 602, 962.

cess has gone on to call it up. Profound sleep may recover lost ideas, because, it is held, the process goes on undisturbed by our fruitless efforts. (A refreshed and rested brain may be responsible for quicker association.)

(6) The same thing is seen in the recall of forgotten facts, and even unnoticed details of a former experience, while a different stream of conscious thought flows along beside it. Automatic writing, "crystal gazing" or "shell hearing" may make known to us facts which it has been impossible for us to recall or even of which we were "unconscious" before. (The automatic writing is explained as a purely physiological process, with no "detached consciousness" and the "unconscious" perceptions as conscious but rapidly forgotten.)

(7) Activities similar to conscious activities except that they are carried on during abstraction and so lack the conscious quality are supposed to show unconscious psychic processes. For example, one may take one's way along the street, choosing one of many possible directions, while one is so absorbed in deep thought that he is unconscious of what he is doing. Or one may hear a sound, detect an odor or feel a pressure while absorbed without being conscious of it, but when the engrossing thought is past it may come to full consciousness. Bleuler, who thinks consciousness occurs only with the association of a complex with the ego-complex,¹ explains such cases by saying that the object was perceived by the psyche, but not so associated. The explanation by those opposed is that what we have is perception with rapid oblivescence.

(8) "Mediate associations where ideas arise which have no causal connection in consciousness show the efficiency of unconscious links." Jerusalem reports such a case, as follows: A man was busy at his work when suddenly there flashed before him a scene witnessed many years before, which was totally out of keeping with his present occupation. Search for the connecting link finally resulted in finding a tiny hidden flower, which he had not known was present, and which had been directly associated with the earlier experience. The odor, of which he was "unconscious" had probably been responsible for the association. Wundt answers to this and like cases that the odor was conscious but unnoticed,² and other opponents take the same attitude. Scripture performed a series of experiments which seemed to show that associations were formed between nonsense syllables by iden-

¹Bleuler, E.: Bewusstsein und Association, *Journal f. Psy. u. Neurol.* VI, 1906, 154. Re-published in C. G. Jung's *Diagnostische Assoziationsstudien*, Leipzig, 1906. I. 257.

²Wundt's *Philosophische Studien*, X, 1894. S. 326-8.

tical characters written on the outer part of the cards used so that they were not noticed.¹ Cordes and Messer think they find cases of mediate association, but Münsterberg,² W. G. Smith and Howe³ get only negative results. Pierce⁴ suggests as an explanation for cases like finding one's self saying a word which later we discover to be written somewhere but which we have not "seen," as the 'translation' of the image into another field than that in which it was received. Out of 892 observations of "free rising" ideas Kiesow reports that 41% could be accounted for, and believes that his experiment proves all such links to be conscious and either ideational or emotional in character.⁵ By some, "free rising ideas" receive a physiological explanation.

(9) Closely connected with mediate associations are the sudden flashes and insights—ideas which apparently come from nowhere—and the exaggerated and more brilliant form of the same thing seen in the inspirations of the genius and the fancies of the poet. Not an unconscious ideational connection, but a purely physiological basis of the association satisfies the opponents of the "unconscious."

(10) Decisions have sometimes been reached or problems worked out in sleep. In dreams the result may be arrived at, but the setting may be fantastic or absurd. Here, too, the physiological mechanism may have started and carried the whole thing out by itself, or if the process is accompanied by dreaming it is none the less conscious for the fact that associations are lacking to make the setting normal.

(11) Development of emotional states is often unconscious. Prejudices are formed for no conscious reason; appreciation of art rests on unconscious factors; a man may be in love without being conscious of it. Here the usage of the term rests on the identification of consciousness and self-consciousness. Because an attitude is "unreasoned" it is not necessarily unconscious. Experiencing an emotion or idea and having it as an object of consciousness are two different

¹Scripture, E. W.: Über den associativen Verlauf der Vorstellungen, Leipzig, 1891. Diss. pp. 76-87.

Cordes, G.: Experimentelle Untersuchungen über Association. *Philos. Studien*, XVII. 1901. S. 73.

Messer, A.: Experimentell-psychologische Untersuchungen über das Denken, *Arch. f. d. gesam. Psy.*, VIII. 1906, 63 ff.

²Münsterberg, H.: *Beiträge*, Bd. 4 1892 S. 2-8.

Smith, W. G.: *Mind*, N. S. III. 1894. p. 301.

³*Am. Jour. Psy.*, VI, 1894. pp. 239-41.

⁴*Jour. Phil. Psy. & Sc. Meth.*, I, 1904. pp. 400-3.

⁵Kiesow, F.: Ueber sogenannte "freisteigende" Vorstellungen, usw. *Arch. f. d. Gesam. Psy.*, VI, 1906. 357-90.

matters. (James.) The man in love is conscious of each feeling and sensation, but not of the fact itself (Brentano).

UNCONSCIOUS AND SUBCONSCIOUS AS USED BY PSYCHOPATHOLOGISTS

The use of the terms "unconscious" or "subconscious" by the psychopathologists to describe anomalies of consciousness deserves special attention because the sense is somewhat different from any of the preceding cases. Consciousness is conceived as split into two consciousnesses, one usually more firmly knit together and predominant than the other, the secondary consciousness, "co-consciousness," or "subconsciousness." These two (or sometimes more) consciousnesses may exist simultaneously or alternate with each other. Prince explains the phenomena thus. Ideas making up an experience tend to become organized into a complex which may be a subject, time or mood complex. Dissociation of personality may take its line of cleavage along any one of these three complexes and in abnormal conditions a complex which is only one side of a character may become the main or sole complex of the new personality. Complexes may be artificially organized in hypnosis, trances, etc. The formation of complexes has its basis in the organization of the neurones into complexes, which retain their organization so that stimulation of one element starts the whole process. Physiological complexes can be conserved despite absence of awareness in the original experience. Strong organization of physiological complexes together with lowered physiological threshold and decreased inhibition, might render them accessible to minimal stimulation, whether peripheral or central, and cause them to function automatically as different groups of ideas. If the threshold were sufficiently low, it might become "co-conscious," without entering the field of personal consciousness. This co-consciousness is really conscious because it behaves so, being able to solve problems, and because it says it is conscious. For Prince there is no normal "subconscious or subliminal self or hidden self."

Janet¹ who likewise limits "subconscious" to the pathological co-activity of divided personality, thinks there may be a group of co-existing ideas in the normal individual because "pathological phenomena have their germ in normal physiology." This aggregation is due to weakness in power of synthesis. In hysteria the power of psychic synthesis is so weakened and consciousness so narrow that when one per-

¹Janet, P.: A Symposium on the Subconscious. *Jour. Abnormal Psy.*, 1907-8, II, p. 62.

ceives an impression he is inaccessible to others. Sometimes he is unable to receive impressions from more than one sense realm, or he may even be able to obtain data from impressions from one side of the body only. Ideas are not associated with one another as with normal people, but every idea takes up the whole narrow activity of consciousness.

Sidis, on the other hand, considers a sub-conscious self the possession of every individual normal and abnormal. The subconscious is not an unconscious physiological mechanism, it is a secondary consciousness, a secondary self.¹ It may possess some degree of self-consciousness, may grow and develop. "As a rule the stream of sub-waking consciousness is broader than that of waking consciousness, so that the submerged, sub-waking self knows the life of the upper, primary, waking self, but the latter does not know² the former. This self is manifested by all the facts of "crystal gazing," "shell hearing," automatic writing and the like. They "reveal the presence of a secondary, submerged, hyperæsthetic consciousness that sees, hears and perceives what is outside the range of perception of the primary personal self." This sub-awaking³ self shows itself present in post-hypnotic suggestion, "shell-hearing," "crystal vision," etc. It is extraordinarily plastic and devoid of all personal character. The subconscious is by no means identical with states of low intensity, but includes psychic states ranging from the lowest to the highest tension and vividness of mental activity.⁴ In the functional relation of nervous elements he finds the physiological basis for the disaggregation of consciousness. The neurons form combinations of ever increasing complexity, and the more complex their organization the greater the organization of psychic units into systems. The individual mind is therefore a complex system of many minds. "There may be as many different personalities, parasitic or secondary, as there are possible combinations and disaggregations of psychophysiological aggregates." A neuron aggregate, entering into association with other aggregates and being called into activity from as many different directions as there are aggregates in the associated cluster, has its neuron energy kept within the limits of the physiological level. A dissociated neuron aggregate, on the contrary, is not affected by the activity of the other aggregates; it is rarely called upon to function and stores up a great amount of neuron energy,—

¹Sidis, B. and Goodhart, S. P.: *Multiple Personality*, N. Y., 1905, p. 128.

²*Ibid.*, p. 138.

³*Ibid.*, p. 45 and 184.

⁴*Ibid.*, p. 45 and 184.

with the equilibrium of the neuron aggregates, with the synthesis of the dissociated systems, the subconscious eruptions, attacks or 'seizures' vanish never to return.¹

The explanation of Breuer and Freud for like phenomena is similar to that of Sidis. In some cases we find that "great complexes of ideas and complex psychic processes, rich in consequences, remain completely unconscious in many patients and coexist with the conscious psychic life."²

Cleavage is usually caused by the suppression from consciousness of a painful experience. Ideas producing the hysterical phenomena, though of long standing, are lively and actually present, their continued liveliness being due to a dearth of associations and external impressions. Cure consists in associating the suppressed experience with the rest of consciousness, for when an emotion is denied expression in reaction the intra-cerebral excitation is greatly increased but used neither in motor nor associative activity, and in some cases abnormal reactions enter and there is an "anomalous expression of the emotional life." When the complex is associated with other neural complexes, its excess energy discharges itself. Phenomena of daily life show the repression of painful memories and evidences of the effect of unconscious ideas, such as forgetting good resolutions or the return of a desired but borrowed book, and many symptomatic and accidental acts. Unconscious motives determine many of our actions. Dream work is a complex thought structure formed in the daytime and not discharged, leaving a remnant which persists and would disturb sleep were it not converted into dreams.

Jung³ finds longer reaction times when the stimulus word is associated with an idea complex possessing a strong feeling tone. This complex, momentarily separated from consciousness, exercises an effect which concurs with the ego-complex. The "constellating" of an association is mostly unconscious, the complex playing the rôle of a quasi-independent existence, a "second consciousness."

GENERAL DISCUSSION OF THE ABOVE MENTIONED CONCEPTS

The above are the representative uses of the terms "conscious," "subconscious" and "unconscious," with the disputes therein involved. Objections to speaking of "unconscious psychic" processes are mainly on the logical ground that un-

¹Sidis, B.: *Psychopathological Researches*, N. Y., 1902, p. 212.

²Breuer and Freud: *Studien über Hysterie*, Leipzig, 1895, p. 194.

³Jung: Ueber das Verhalten der Reactionszeit beim Assoziationsexperimente. *Journal f. Psy. u. Neurol.*, VI. 1905. 29. (Republished in C. G. Jung's *Diagnostische Assoziationsstudien*, Leipzig, 1906, I. 221 ff.)

conscious and psychic are contradictory terms. Metaphysical considerations on the other hand are responsible for the contrary opinion, for it is held that the law of universal causation must hold for the mental as well as the physical world. Changes often occur in consciousness with no consciousness, *i. e.*, with no consciously observable cause; therefore they must have an unconscious mental cause. The problem reduces therefore to one of epistemology;¹ the identification of consciousness and the psychic or the extension of "psychic" to cover the accompaniment of all material changes. As Hellpach points out, the unconscious can never be discovered by investigation, but only by hypothesis, by analogy, or metaphysics. He who denies the unconscious retreats to the empirical and has to explain all from the conscious side in which there are vast gaps. If he makes any assumptions he must say that consciousness causes physical changes and these again conscious ones, which is the interactionist's position, or that physical and mental changes are parallel, which is the position of either monistic or dualistic parallelism.² The attitude one takes reduces, therefore, ultimately, to a question of his temperament. Practically, it makes little difference whether one assumes the changes going on without consciousness but later affecting it to be complex neural changes only,³ or changes possessing the conscious character in structure but lacking the conscious quality, or a psychic reality accompanying all existence, or a psychic accompaniment of molecular changes different in degree from those underlying conscious experiences.

Let us consider somewhat more fully the use of the terms "unconscious psychic process" in this metaphysical sense of the psychic accompaniment of physical processes lacking the conscious quality. By consciousness we mean that indefinable ultimate best described as experience or awareness. It is not identical with self-consciousness, which is only consciousness of one's past states of consciousness, immediate or remote. Consciousness is the broader term. Conscious states, however, are those which can become self-conscious later. Consciousness is always more or less complex, the elements entering therein contributing to the character of the whole, which is qualitatively different from these elements. It is the *interconnection* of the psychic processes, *i. e.*, it is the *association* of the elements constituting it. Where association is

¹Münsterberg, H.: Symposium on the Subconscious, *Jour. Abnormal Psy.* II. 1907-08, p. 28.

²Hellpach: Das Unbewusste, *Central. f. Nervenheilkunde und Psychiatrie*, 1908, Bd. 31, S. 65-6.

³Münsterberg; *Psychotherapy*, N. Y., 1909, p. 140; p. 147.

strongest consciousness is most intense. We assume the existence of a psychic side concomitant with all material changes, both conscious and unconscious. "Unconscious" we use as diametrically opposed to "conscious," "psychic process" in the metaphysical sense of the psychic side of a process possessing a concomitant physical side. Such a psychic side we conceive to be present in all organic matter, if not in all matter. With the activity of each neuron, therefore, there is psychic process, but consciousness probably does not occur until there is a complex functioning of neurons in one system or pattern. The pattern may change and shift in its organization, now dropping out some elements now taking up others, but the whole is usually in a more or less close functional connection. It is however conceivable that two or more different complexes may be functioning with sufficient intensity to give different alternating or simultaneously existing streams of consciousness. Association of an aggregate with the personal aggregate is probably necessary for consciousness—in other words, association of elements with the general bodily sensations and feelings which constitute the fundamental part of our personality—what Bleuler probably means by "the association with the ego-complex."

According to this view ideas out of consciousness do not become physiological processes any more than they were such before. We must assume some disintegration of the neuron aggregates underlying the idea and with this disintegration some change in the idea itself, according to which it no longer possesses its former character, but is the psychic accompaniment of the physiological process.

As for the objection to the term "unconscious psychic process," we agree with Lipps¹ that every psychic *process* is unconscious. All that is given in experience is each separate state of consciousness, the process underlying the sequence of ideas or feelings never being a matter of consciousness, but something which is merely inferred. The inspirations of the poet and the associations of the genius are not more spontaneous than those of the ordinary man, only richer and more varied. The ordinary man may be able to trace and explain the sequences better than his more fortunate brother, but the processes underlying them are none the less unconscious.

As for "free-rising" ideas, or "mediate" associations, conscious connective links, rapidly forgotten, without doubt exist in many cases. When the idea can be traced to an association started from some external cause, of which the subject was absolutely unconscious, a peripheral physiological

¹Lipps, T.: Leitfaden d. Psychologie, Leipzig, 1909, p. 83 f.

excitation has probably set into action a neuron complex out of connection with the main system, which is concerned in some other different state of consciousness. When this widens and the emphasis of activity shifts, the aggregate stimulated peripherally, without conscious, but with psychic, accompaniment may be taken up in the new arrangement of neuron complexes and become conscious. But sometimes an idea enters with no traceable connection. Here aggregates concerned in the "free rising" idea may be in a state of functional activity of a greater or less degree but separated from the centre of activity. Cells concerned but slightly in the conscious processes of the moment may set in activity others involved in the detached aggregate, which functions then with such completeness as to shift the centre of activity from the original to the new position, the new idea becoming the centre of consciousness.

The divided consciousness of multiple personality, and similar states sometimes experienced by normal individuals in dreams or abstracted conditions where consciousness seems to be divided into two alternating or coincident streams, is of the same general character as consciousness. The term "co-consciousness" is a better name for this experience than "subconsciousness," and is more definite in its meaning. Used in such a sense "subconscious" is too easily extended, even by the writer who so uses it, to cover something which is literally under consciousness, out of which consciousness arises and into which it descends. Used in such a way, it takes the place of the older concept of the "soul" as an independent creative entity. Just as the soul was responsible for our actions or looked out upon our thoughts, so its successor, the "subconscious self" is supposed to do.

Consciousness does not fade off from distinctness by ever fainter degrees into unconsciousness. Facts in the margin of consciousness are qualitatively the same as those in the focus, but the difference between the outer limits of the margin and the region beyond is absolute. "Subconscious," when used to denote the periphery of the conscious field, is a term descriptive of a condition of actual consciousness, different in degree; but as it is too easily extended to describe processes outside of consciousness, "perceptual" is a better term.

Analysis shows that expressions like "resting back on the subconscious" in prayer and meditation mean the relieving of mental tension by widening attention, so that activity can shift from newer, less firmly established association complexes to older, well developed complexes which have had survival value. The individual is "larger than his conscious-

ness,' in that consciousness at best is so narrow as to embrace but a small part of the results of his own past habits and experiences, and those of the race seen in tendencies, appetencies or instincts. In such a sense any present experience is only to a small degree determined by conscious factors. One's motives for action are seldom clearly analyzed or made focal in consciousness. Oftener they are entirely without consciousness, being the results of past experiences and training which have developed characteristic modes of spontaneous response.

CONSCIOUSNESS IN ANIMALS

No objective proof of consciousness in animals is possible,¹ but the assumption of consciousness in them rests on inference, just as it does in our fellow beings, for the only place it can be positively known is in the individual himself. Denying the possibility of comparative psychology would therefore logically result in a like attitude toward human psychology.² Any objective criterion of consciousness must be arbitrary. "Learning" or "modifiability of behavior" as an indication of its presence is not good, for there is evidence that plants learn,³ and even material⁴ objects adapt themselves to repeated stimuli or changed conditions as the seasoning of a violin to strains of the master. There are also some indications that human learning goes on unconsciously.

III. THE RELATION OF CONSCIOUSNESS TO LEARNING

The relation of consciousness to learning has received some discussion as well as experimental testing. The problem of learning in general I have reviewed elsewhere⁵ and shall consider here only the results bearing directly on the subject in hand.

THE LEARNING PROCESS

We may define learning as the formation of associations between certain stimuli and definite modes of reaction. The simpler and less varied the stimuli the simpler the learning process will necessarily be, and the more permanent the value of

¹Yerkes, R. M.: Objective Nomenclature, Comparative Psychology and Animal Behavior, *Jour Comp. Neur. and Psy.*, 1906, XVI, p. 388.

²Claparède, E.: La psychologie comparée, est-elle légitime? *Arch. de psy.*, 1905-6, V, p. 34.

³Darwin and Pertz: On the Artificial Production of Rhythm in Plants. *Annals of Bot.*, 1903, XVII, pp. 93-106.

⁴Washburn, M. F.: Animal Mind, N. Y., 1908, p. 33.

Claparède, E.: The Consciousness of Animals. *Internat. Quart.*, 1903-4, VIII, pp. 296-315.

⁵Ellison, L.: The Acquisition of Technical Skill, *Ped. Sem.*, 1909, XVI, pp. 49-63.

the associations formed, the simplest form probably being that in the case of lower animals where the association is between a single stimulus and a simple movement, the highest and most complex form in man, where complicated movements must often be preceded by a train of thought possessing little motor accompaniment. The simplest form of learning involves, then, direct motor response to a simple stimulus, the highest, however, lacks much of this motor element, being for the most part an association of symbols, such as the growth in meaning of words, and the power of generalization from previous experience. Most learning involves both these factors.

Observation and experiment goes to show that learning to meet any new situation involves a specialization and perfection of some part of an already existing habit or mental possession. As Morgan points out, effective consciousness¹ finds itself a partner in a "going concern." The performance of the instinctive act whose co-ordinations are hereditary, and the consciousness such a performance evokes, are simultaneous.² The behavior and the conditions producing it occupy consciousness, but "the effects of the behavior, as the animal becomes conscious of the acts concerned, serve to complete and render definite the conscious situation. Consciousness, however, probably receives information of the net results of the progress of behavior and not of the minute and separate details of muscular contraction."³ As Sherrington puts it, "the controlling centres can pick out from some ancestrally given motor reaction some part of it so as to isolate that as a separate movement, and by enhancement this can become a skilled adapted act added to the powers of the individual."⁴ When a new movement is initiated an excess of energy is expended and with it occur many more or less random movements; of these, as the effort is repeated, a special movement, or a special series, finally stands out from the scattered mass. The clearer its separateness from the rest, the more vivid its conscious accompaniment and the power of conscious control. Consciousness of the way a movement feels is necessary for its voluntary performance, hence, as Judd's⁵ experiments show, an abstract idea cannot

¹Morgan, C. L.: Introduction to Comparative Psychology, N. Y., 1906, p. 51.

²*Ibid.*, pp. 99, 101.

³*Ibid.*, p. 105.

⁴Sherrington, C. S.: Integrative Action of the Nervous System, N. Y., 1906, p. 389.

⁵Judd, C. H.: Practice and its Effects on the Perception of Illusions, *Psy. Rev.*, 1902, IX, pp. 27-59.

take the place of direct perceptual experience. The way to get control of a movement as the experiments of Bair¹ and of Swift² show, is by working outward from some general movement over which we already have control. In Bair's experiment on learning to move the ears, the subjects began with the muscles over which they had conscious control, such as raising the brow, clinching the teeth, making more and more strenuous effort to get closer to the ear, an excess of motor energy being discharged with proximate muscles. "As soon as the sensation arising from the movement of the ear was associated with the concomitant sensations of muscles close to it, over which there was already voluntary control, there was a basis for learning the voluntary control of the ear." The definite idea of the movement given by electrical stimulation of the *retrahens* muscle was not sufficient to produce the movement, but it gave a general idea as to the direction the innervation was to take. As control developed attention was narrowed down from the general sensation of the adjacent muscles to that of the specific movement sought for. Likewise, in the control of the reflex wink, Swift found it necessary to begin with the muscles around the eyes over which there was conscious control. What Bair³ says in regard to the general ability given by special training, *e. g.*, "to a new situation we react by a general discriminative reaction and are more likely to hit on a favorable response than without this special training," is true of all learning. For no matter what new acquisition is undertaken, if it is possible to master it, some previous general training has either been developed by the individual or through the inherited co-ordinations of his ancestors. Experiments on acquisitions of a more complex kind show the same fact—attentive consciousness cannot be directly and advantageously applied at first, because of the multiplicity of details which overwhelm it. The new experience calls up too many old associations which are not pertinent. Such facts account for the rapid rise of the learning curve at first, when responses are selected from a mass of older habitual reactions, and its slower ascent later, when associations really new are being formed.

CLEAR CONSCIOUSNESS AND LEARNING

The importance of clear consciousness in learning is shown by the following facts. Experiences causing greatest atten-

¹Bair, J. H.: Development of Voluntary Control, *Psy. Rev.*, VIII, p. 499.

²Swift, E. J.: Studies in the Physiology and Psychology of Learning, *Am. Jour. Psy.*, 1903, XIV, pp. 200-251.

³Bair, J. H.: The Practice Curve, *Psy. Rev.*, *Mon. Sup.*, 1902 V.

tion are best remembered. Desire to succeed and intense effort are necessary for progress, which means that one must attend closely to the matter at hand. Even in learning of a purely muscular sort, where attention to the movement itself has been found to be a hindrance, attention to the objective features of the task is required for the perfection of the unconscious or dimly conscious part of the reaction. The fact that subjects of a given mental type are most interfered with in their learning, by distractions appealing to that type of imagery, shows that undisturbed consciousness is essential. Trying to recite a syllable series is more effective in establishing the syllables than merely reading them,¹ because of the narrower attention required. Figures drawn with the left hand are better remembered than those drawn with the right, for the same reason;² the greater ease of remembering sense material as compared with nonsense is also probably due in part to the easier application of attention.

Results from experiments on cross education point to like conditions, for the more similar the training and the test material the greater the transference.³ Improvement consists in more economic methods of work,⁴ and is essentially "attention training." Transference consists in the carrying over of right hand "methods" to the left hand. Perhaps the most adequate study of this problem is a recent one by Fracker,⁵ who finds that the most essential element in transference is imagery and that improvement occurs if imagery is developed in the training series which can be transferred and advantageously used in the test series. It may be subconsciously developed, but if it comes to be consciously recognized, the improvement is more rapid. "The rate of improvement seems to depend directly upon the conscious recognition of the imagery and upon attention to its use. The transference of elements is a conscious transference." Improvement during intervals of no practice seems to be due in part to freshness and better attention, in part to the fact that interfering habits are forgotten, so that better and more

¹Witasek, S.: Ueber Lesen und Rezitieren. . *Zeits. f. Psy.*, 1907 Bd. 44; pp. 161-185; 246-282

Katzaroff, D.: Expériences sur le rôle de la récitation, *Arch. de Psy.*, 1908, VII, pp. 255-8.

²Rowe and Washburn: The Motor Memory of the Left Hand, *Am. Jour. Psy.*, 1908, XIX, p. 243.

³Ebert u. Meumann: Ueber einige Grundfragen der Psychologie der Uebungsphänomene, *Arch. f. d. ges. Psy.* 1905, IV, S 1—232.

⁴Swift: *Op. cit.*

⁵Fracker, G. C.: On the Transference of Training in Memory, *Psy. Rev.*, Mon. Sup., IX, 1908, 56-102.

practiced ones may be free to assert themselves.¹ Then, too, attention is not distracted by the new elements of the situation, but can be more economically applied. The value of clear consciousness in learning is that it assists the selecting of good elements from the complex reaction and the eliminating of disadvantageous factors. When the subject is weary he is apt to fall into bad habits which are more difficult to modify because unconscious.

Consciousness of details and elements of a process gradually gives place to consciousness of larger and more complex difficulties. These elements gradually form themselves into larger wholes and consciousness works with greater units. This is true not only of muscular learning, but of more intellectual activities such as typewriting, the telegraph language and chess. As Cleveland² puts it, with reference to chess, "learning requires the perfection of the elements and their organization into ever larger groups, so that attention is not bound to details, but left free to forge ahead and anticipate difficulties." In the writing processes, first letters, then words, then sentences are grasped. In chess one grasps the situation by larger and larger wholes. Cleveland puts it thus, "Progress in chess consists in the formation of an increasing symbolism which permits the manipulation of larger and larger complexes. . . . There is something in the purely intellectual life corresponding to motor automatism, which is shown in the ability to think symbolically or abstractly, and thus to handle large masses of detail with a minimum of conscious effort. It involves the increasing ability to take in during a single pulse of attention a larger and larger group of details which means, of course, that the attention is no longer needed for each one."

The importance of the motor element in learning verbal material is without doubt due to clearer consciousness of the task, the material appealing not only to vision, but to hearing and the kinæsthetic senses.

SUBCONSCIOUS AND UNCONSCIOUS LEARNING

Such are some of the observations as to the rôle of consciousness in learning, but much of our learning goes on below consciousness. As Kuhlmann³ points out, much of our most important learning—the use and functional activity of our own

¹Book, W. F.: *The Psychology of Skill*, U. of Montana Bull., No. 53, 1908, p. 6 ff.

²Cleveland, A. A.: *The Psychology of Chess and Learning to Play it*. *Am. Jour. Psy.*, 1907, XVIII, pp. 269-308.

³Kuhlmann, F.: *The Place of Mental Imagery and Memory among Mental Functions*, *Am. Jour. Psy.*, 1905, XVI, p. 337-356.

bodies—goes on with no conscious direction. The digestive apparatus must learn to do its work, and so must other internal organs. Early reflexes such as the reflex wink and mimetic expressions, binocular vision, the co-ordination of voluntary muscles, develop without *voluntary* use of incoming stimuli for their guidance.

The very possibility of learning rests on an unconscious basis of physiological endowment. The dullard and the genius are alike dependent upon their physiological inheritance, and the quick wits of the healthy child are as much beyond his conscious control as are those of the feeble defective.

We are not conscious of the "process" underlying our associations, but merely of the results as they present themselves to consciousness. In fact learning may progress without our knowledge of the fact, as is seen in the development of unconscious automatisms. For example, one may develop peculiar manners of gait or expression without knowledge, having unconsciously imitated some one possessing a like peculiarity. Through the ever present suggestions of a new environment we may develop new ideals and new apperceptive attitudes of which we are unconscious until we are taken back to our old surroundings. In learning of a muscular sort we may have been conscious of every sensation leading up to the subsequent habit, without consciousness of the method in which we work, or of the existence of the habit itself. An example is given by Pfunst.¹ Having directed his subjects to think of one of two similarly sounding words of a series to which he would respond with certain arm movements, he was able to tell by the direction of the head or eye movements, of which word they were thinking and to which they expected him to react. By changing his manner of responding he obtained a similar change in their movements. He was also able to tell by head movements of which direction, left or right, his subjects were thinking. He concludes that "the changing of natural movements of expression and the acquisition of new ones are both possible without knowledge of the person."

Our environment is one of the strongest factors in training us, whether it operates consciously or unconsciously. One hears good language continuously, and easily forms the habit of using it himself. The development of our ethical ideals and æsthetic feelings and our very forms of thinking unfold before we are conscious of their existence. Such habit formation rests on instinctive imitation and forms one of the most important classes of learning.

¹Pfunst, O.: Das Pferd des Herrn von Osten, Leipzig, 1907, p. 77 ff.

Learning may also progress without consciousness of the end or purpose. Such a fact has its best illustration in the instinctive activities of animals and the play of children and animals. Play is a training process for life where most of the activities required in later life have their initial, though unwitting, development.

The formation and strengthening of associations below consciousness is indicated by some of the laboratory experiments on learning. This must be the explanation of the fact that scattered repetitions, which deal with older associations, give best results. Müller and Pilzecker think the greater strength of the older associations is due to the tendency of an excitation to outlast the stimulus—to a “perseveration tendency”—as a result of which an idea rises of its own accord into consciousness without associative connections. Illustrations other than those found in learning nonsense syllables are the following: The histologist’s illusion which occurs after working long and intently with the microscope; images seen with closed eyes often have then the character of the microscopic forms. Similarly when one studies or thinks intently of any subject, carelessly perceived objects tend to take on its character. While studying the anatomy of the internal ear every gas jet or twisted twig was a cochlea, for the writer. It is not uncommon to see, before sleeping, scenes which have passed before the eyes while travelling, or on a tramp. “Crystal vision” may illustrate the same phenomenon, freeing the mind for the appearance of ideas underlying which is this perseveration tendency. This fixing of the association probably goes on physiologically whether the ideas crop out into consciousness or not, for Müller and Pilzecker found that attention to any other engrossing matter prevented their recall, although free reproduction was not a tendency with all their subjects; nor was the hindrance due to preventing the subject from thinking over the series. They conclude that “after the reading of a syllable series, certain physiological processes which serve for the strengthening of the associations formed by the reading of a series continue for a certain time with gradually diminishing intensity.”¹

“Retroactive amnesia” or the forgetting, after a shock, of incidents extending backward from the shock to several hours or more, likewise points to the probability of the physiological fixing of associations.²

Experiments on the acquisition of skill show that unconscious habits are developed which consciousness either selects

¹Müller u. Pilzecker: *Zeit. f. Psych.*, Ergänzungsband I, 1900. p. 196.

²Burnham, W. H.: Retroactive Amnesia. *Am. Jour. Psy.*, 1903, XIV, p. 386-7.

or represses. As Swift says, "Consciousness discovers certain methods in operation and approves or disapproves them."¹ Subjects improve by "hitting upon" better ways of working, without any further conscious selection, at first, than the general effort to succeed. Book's experiment shows the same thing. "A mass of old associations are called up, only a few of which are directly serviceable for the work. From these there are unconsciously built up, by the double process of elimination and selection and reorganization, the first elementary associations (letter associations) used, and from these in turn the later, higher order habits. There comes to be . . . a sort of unconscious struggle for existence among the many modes of action, ending in the survival of the one direct and economic way of reaching the goal desired."²

Automatization of elements previously conscious occurs in the perfection of all activities, leaving consciousness free to undertake more difficult features.

IV. AN EXPERIMENTAL STUDY OF THE RELATION OF CONSCIOUSNESS TO LEARNING

Our own investigations undertook to discover by experiment (1) whether learning is helped by factors which never come into consciousness, or are present only to a minimal degree, (2) whether the formation of a habit of whose existence and development one is unconscious can progress as well under distraction, when consciousness is removed as completely as possible from all the elements which go to make up the habit formation; and finally, to find the rôle of consciousness in learning simple tasks involving, (3) almost no intellectual factor, (4) a complex co-ordination of muscular impulses, and (5) learning of purely intellectual character.

1. *Do Unnoticed Items Assist in the Formation of Associative Links?*

EXPERIMENT I was suggested by Scripture's experiments on the associative course of ideas,³ and work of a similar sort,⁴ which seemed to show that unnoticed features of a total impression (like an inconspicuous Japanese symbol or a numeral placed beside a word or a picture) could serve as a bond to connect the given word or picture with another word or picture which had been elsewhere accompanied by the same symbol or numeral. The theoretic-

¹Swift, E. J.: Studies in the Psychology and Physiology of Learning. *Am. Jour. Psy.*, 1903, XIV, p. 201-251.

²Book, W. F.: Univ. Montana Bull., No. 53, 1908.

³Scripture: *Op. cit.*

⁴Sidis: The Psychology of Suggestion, N. Y., 1898, p. 171.

cal importance of the question and the fact that most attempts to repeat Scripture's work had led to negative results invited a new attack. The plan which we undertook may be illustrated by the following scheme, though the actual execution of the experiment was carried out with greater refinement and in a different way as to details.

The observer is presented with a triple series of meaningless syllables, as in Group I below, and is required to read series *b* a certain number of times and if possible learn it. Series *a* and *c* are of course all the time before his eyes though not involved in his task. After reading *b* the required number of times, his knowledge of it is tested by the "Treffer method," and his success in giving the required syllables recorded. Then after a brief interval he is presented with Group II of which the middle series is the same as one of the side series in Group I, *e. g.*, series *a*, and he is required to read (and learn) series *a* in the same manner in which he has just read (and learned) series *b*.

Group I			Group II		
<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>a</i>	<i>e</i>
jex	mil	peb	miv	jex	hal
yal	hud	yom	sem	yal	juj
bup	gib	lur	coj	bup	pom
dof	dep	zal	fet	dof	das
tem	voz	bic	dib	tem	lef
fuj	pog	vop	vil	fuj	roj
nen	lek	loh	buj	nen	zup
gop	gaj	nat	hix	gop	fab
riz	fiv	jof	kug	riz	uls
mod	yem	wam	len	mod	veb

If *a* is on the average learned with greater ease or completeness than *b*, the inference is that the previous presentation of *a* in indirect vision has somehow been helpful—directly by rendering the syllables individually more familiar, or indirectly through their association with the syllables of series *b* which have in the learning been associated with each other. If *a* is not learned on the average more easily or perfectly than *b* the inference is either that no assistance is gained by the "unconscious" perception of *a* or that the gain is not of sufficient amount to be determined by this method of experimentation.

Such experiments were carried out on two trained observers through a considerable number of days, but it may be said at once that the results were on the whole negative. There was no clear evidence of any advantage. The presumption is that the assistance gained is small in amount—too small to be deter-

mined by this method. Later experiments undertaken expressly to determine the delicacy of the method showed that one reading of the *a* series with full attention had no beneficial effect upon the learning of the *a* series after an interval of ten minutes during which the *b* series had been learned.

Though the results of this series of experiments must therefore be set down as inconclusive, they may have, perhaps, a certain value in other connections and are therefore given in the Appendix of this paper.

2. *The Effect of Attention and Distraction on the Formation of the Motor "Set" (Motorische Einstellung)*

EXPERIMENT 2. The purpose of the second series of experiments was to find the effect on the "*Motorische Einstellung*" of attention and distraction. The term "*Motorische Einstellung*" indicates the effect which repeated lifting of a heavy weight has in making subsequent lighter weights seem too light. It is probably due to a temporary habit of the nervous system. The problem in our case was to discover whether a neural habit of this sort, of whose existence the subject was unaware, would be more readily formed when he was attending to the lifting of the heavy weight than when he was inattentive to it.

The phenomenon of "*Motorische Einstellung*" was first reported by Müller and Schumann.¹ They lifted a moderate weight of, say, 600 grams and, after it, lifted a heavier weight of 2,400 grams to an equal height a certain number of times, in a definite rhythm. Then a weight of 800 grams was lifted and found to seem lighter than the 600 grams, lifted before the training with the weight of 2,400 grams. They explain the illusion by saying that the 800 grams, which is lifted with an unusually powerful impulse after the work with the weight of 2,400 grams, rises with unusual speed and therefore seems lighter than the first weight, because we are apt to judge as lighter a weight which raises more quickly. The repetition of the lifting of the heavy weight has set up a tendency in certain sub-cortical centres to discharge automatically with a somewhat extra intensity. Experimentation of this kind was carried further by Steffens.²

The apparatus used is pictured in the accompanying cut. Two boards measuring about eighteen inches long were clamped to the sides of the bottom of a chair so that the ends

¹Müller u. Schumann: Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte, *Pflüger's Archiv*, XLV, 1889, 37-122.

²Steffens: Ueber die motorische Einstellung. *Zeits. f. Psy.*, Bd. 23, S. 240-308.

extended about seven inches beyond the front edge. Holes were bored near the forward ends of the boards and through these were passed the ends of two handles by which the weights were lifted. The upper parts of the handles were made of wood and were provided with grooves into which fitted the fingers of the observer, enabling him to hold the handles firmly and in the same way each time he lifted. The handles below the board consisted of brass rods having at their lower ends disks of wood, on which the weights rested. An iron needle was passed through each brass rod in the middle, making it possible to raise the handles only a given distance. To prevent the needles hitting against the boards with a jar, a string was fastened in front of the chair, by means of two iron standards clamped to the table, at such a height that the observer's hands would touch the string before the needles came in contact with the boards; as soon as the hand touched the string the weight was lowered. A disk of cork was used on each handle to prevent the clinking of the weights against each other. The entire weight of each handle with the cork disk was 100 grams.

The chair stood on one of the large laboratory tables. As far as possible from the observer a metronome was placed, its noise being deadened by a cloth pad between it and the table. The experimenter sat at the side of the table to the observer's left, and changed the weights as the experiment required. These were flat and circular in form with a rather large slit so that they would slip on and off the handles easily.

The method of the experiment was this: The right-hand weight was always the standard, and was always kept at 300 grams, *i. e.*, a 200 gr. weight plus the weight of the handles. By trying different weights a weight was found for the left hand which usually seemed equal to the right-hand weight. Owing to the difference in strength between the right and left hands this was actually a weight much smaller than the standard. Since practice was apt to increase the strength of the left hand, it was necessary to determine what this weight was before every experiment; and doing this counteracted also any influence which might have been carried over from lifting heavy weights in the experiment of a previous day. After determining the apparently equal weight, twenty judgments were made, upon weights offered for comparison with the standard (300 grams in the right hand), four with the weight which had been judged equal, and four each with weights ten and twenty grams above and ten and twenty grams below the "equal" weight. If the judgments were perfect the results would of course show four judgments "equal," eight "heavier" and eight "lighter." As a matter of fact they

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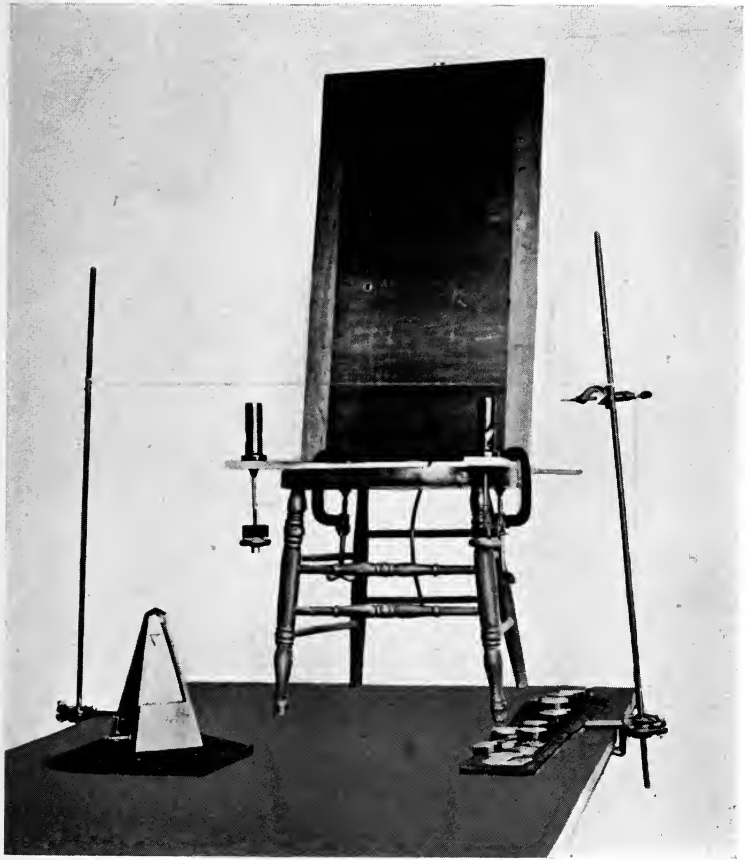
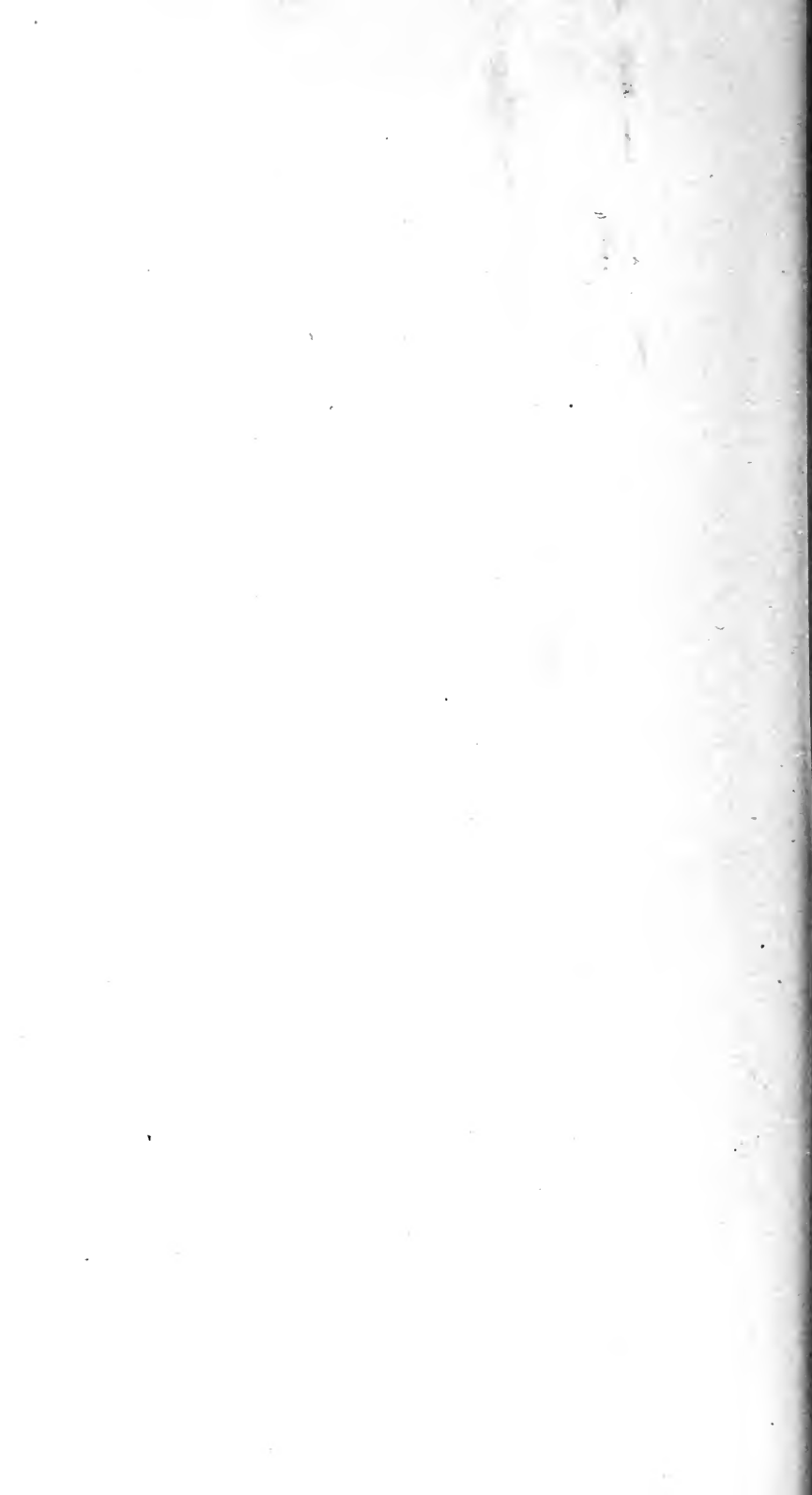


Fig. I.



varied a little, this way and that, as is common in such experiments. The lifting was done rhythmically to the stroke of the metronome, which beat at about four per second for the different observers, some requiring a slower rhythm than others. The rhythm was constant, however, for each subject. At "one" the standard was raised, at "two" lowered, on "three" the comparison weight was raised and on "four" lowered.

The results of the twenty lifts just described served as a basis of comparison for similar lifts after a period of lifting a heavier weight to establish the *Einstellung*. This heavy weight, which was called the "training" weight, was for all subjects a weight twice as great as the one which had seemed equal. The experiment here differed a little in its method for the lifting "with attention" and "with distraction," though the actual weight lifted and the rhythm for lifting were the same for each. In the experiment "with attention," the subject was told that he must determine, by estimation, a weight twice that of the standard, and that he would be given two weights above and two weights below this double weight, and sometimes the double weight itself. He was to raise the standard just as he had done in the previous judgments of equals, which would aid him in his judgment, and he was to judge as quickly as possible. Here the standard was raised on "one," lowered on "two," a pause on "three," the variable raised on "four," lowered on "five," and the judgment given immediately. While the judgment was being given the experimenter shifted the weights, and the subject began to raise the standard in nearly all cases after the sixth count. The method of lifting was at first not carried out in this three-six rhythm, but in a two-four rhythm; but was changed after three days of experimentation to make the rhythm the same for the experiments "with attention" and "with distraction." All the results are incorporated in the tables which follow, as the change of rhythm seemed to have no disturbing effect. Ten judgments were made, four of the weights being greater than the "double" weight, four less, and two the "double weight" itself, so that the actual weight lifted amounted to lifting the double weight ten times. Immediately after giving the tenth judgment the five original (*i. e.*, nearly "equal") weights were compared with the standard, to test the *Einstellung*, and the results recorded. After a rest of from one to two minutes the double weight was raised five times more, in the manner just described, to refresh the *Einstellung*; and immediately after, the original (nearly "equal") weights were again compared. This was done until the training weight had been raised twenty times in all and the original weights had been compared twenty times. In schematic form the ex-

periment was: (1) to determine a weight which when lifted by the left hand should seem equal to the standard weight, when lifted by the right hand. This was done by offering the original weights in such a way that the "equal" weight was raised four times, the ten and twenty grams "heavier," four times each, and the ten and twenty grams lighter, four times each. (2) Ten lifts of the training weight, followed by five comparisons of the original weights arranged according to a regular permutation. (3) Five lifts of the training weight, with five judgments of the original weights. (4) Repetition of (3). (5) Repetition of (3).

The experiment "with distraction" differed from that with full attention only in so far as lifting the training weight was concerned. Here the "double" weight only was lifted ten times, with a pause, then five times with a pause, and so on, until twenty lifts had been made; the actual weight lifted, however, amounted to the same for both forms of the experiment. While the observer was lifting the training weight in the "distraction" experiment, the experimenter read as distinctly as possible from some interesting reading matter. After the lifting of the original weights, which immediately followed that of the training weight, the subject was asked to give the content of what had been read, and a record was made of his success.

Four university students of psychology, two men and two women, served as observers. The number of experiments per observer varied from 14 for observer I, to 28 for observer IV, owing to modifications made necessary by the differences of the individual observers. Each experiment represents twenty lifts for the original weights, twenty for the training weight, and twenty again for the original weights.

The *Einstellung* was present unmistakably in the case of each observer. Observer I showed a clear and decided difference in the intensity of the *Einstellung* under the two conditions of the experiment, the effect being much greater when the training weight was lifted with attention than where distraction was used. This is true for the total and also for all except one of the single pairs of days on which experiments with attention and with distraction were made. The other observers, however, showed this difference but slightly or not at all. The difference was so clear for observer I that an explanation was sought for the indefiniteness of the records of the other three observers. As none could be found at first for observer IV, his work was continued until 18 complete experiments had been performed. The difficulty was discovered for observers II and III and the experiment modified after 12 complete experiments. The following table gives the

results obtained from the work just described. The first column represents the observer, the second the number of

TABLE I

No. of lifts	Ra		Ta		Rd		Td		Dif. a		Dif. d		a-d								
	L	E H	L	E	L	E H	L	E	L	E	L	E	L	E H							
Obs. I	140	49	50	41	6	38	56	46	79	45	16	+64	-29	-35	+41	-11	-30	+23	-18	-5	
	140	50	36	54	108	22	10	49	40	51	103	20	7	+58	-14	+54	-20	-34	+4	+6	-10
III	120	42	36	42	99	13	8	41	40	39	96	22	2	+57	-23	+55	-8	-37	+2	+3	-5
	180	48	85	47	105	67	8	43	88	49	107	64	9	+57	-8	+64	-24	-40	-7	+6	+1

lifts, the third (*Ra*) the results before training on days when the training weight was lifted with attention, the fourth (*Ta*) the results after training on days when the training weight

was lifted with attention. Column *Rd* contains the results from the lifting of the original weights before training on the days of the "distraction experiment," and *Td*, those obtained after lifting the training weight on the same days. *Dif. a* shows the difference between the results before and after the training weight, in other words the amount of the *Einstellung*, for the experiments with attention. *Dif. d* gives amount of the *Einstellung* for the distraction experiments. Column *a—d* gives the difference between the *Einstellungen* in the two cases. L, E, H, stand for "lighter," "equal," and "heavier" cases respectively. It will be noticed that some superiority of the *Einstellung* with attention exists over that with distraction except for observer IV, in whose case the opposite is true.

The *Einstellung* came out so clearly in both forms of the experiment with observers II and III, that it was thought that the training weight was so heavy as to give a tolerably intense *Einstellung*, irrespective of attention and distraction. It was therefore reduced a hundred grams to make the *Einstellung* more moderate, and the greater effect "with attention" immediately showed clearly against that "with distraction." The explanation for observer IV was different, and was obtained by an examination of the columns giving the results before the training weight had been lifted (columns *Ra* and *Rd*). The equal cases should number something near one-fifth of the total number and the heavier and lighter about two-fifths each; but the proportion is almost reversed, showing that weights just above and just below the "equal" weights were not discriminated from the "equal." To remedy this difficulty the training weight was kept as before, but the original weights were decreased (and increased) from 10 and 20 grams below and above the "equal" to 20 and 40 grams below and above, in order to make the possibility of discriminating greater. His results then showed the same tendencies as the other observers. Table II gives the results. The lettering of the columns has the same significance as for the preceding table. The first line of figures for each observer reading across the table gives the results of the modified experiment. The second gives the combined results of the first and second form. Observer I, performing the experiment only in its original form, is represented by but one line of figures.

After the experiment was closed a final test was made with each observer both with distraction and with attention, but the observer was asked to notice his manner of lifting, and to see if it differed subjectively in either case. Observers I and II reported them the same. Observer III held the weights a little looser in the lifting with attention but the lifting itself was the same. Observer IV raised the weights a little more

TABLE II

	No. of lifts	Ra			Ta			Rd			Td			Dif. a			Dif. d			a-d		
		L	E	H	L	E	H	L	E	H	L	E	H	L	E	H	L	E	H	L	E	H
Obs. I	140	49	50	41	113	21	6	38	56	46	79	45	16	+64	-29	-35	+41	-11	-30	+23	-18	-5
II	120	61	18	41	79	25	16	58	29	33	73	31	16	+18	+7	-25	+15	+2	-17	+3	+5	-8
	260	111	54	95	187	47	26	107	69	84	176	51	23	+76	-7	-69	+69	-18	-51	+7	+11	-18
III	100	57	26	23	72	15	13	57	18	25	64	19	17	+21	-11	-10	+7	-8	-1	+14	-12	-2
	220	93	62	65	171	28	21	98	58	64	160	41	19	+78	-34	-44	+62	-9	-45	+16	-17	+1
IV	100	21	33	46	49	32	19	36	28	36	52	32	16	+28	-1	-27	+16	+4	-28	+12	-5	-7
	280	69	118	93	154	99	27	79	116	85	159	96	25	+85	-19	-66	+80	-20	-60	+5	+1	-6

sharply and slightly higher "with distraction." This difference was too slight to be detected by the experimenter. Ob-

server IV was the only one who had any idea as to the purpose of the experiment, and he had surmised it.

Taking all facts into consideration it seems certain that with a training weight which gives a moderate *Einstellung* and original weights which are different enough in value to render discrimination easy, lifting the training weight with full attention produces a more intense *Einstellung* than lifting the same weight in the same manner, but with distraction.

3. *The Rôle of Consciousness in the Acquirement of Muscular Skill*

The third series of experiments was of a very simple character and useful chiefly in furnishing opportunity for introspection. It consisted in learning to throw balls at a target about eight feet in diameter, from a distance of 14 to 18 feet. Two university students of the psychological department served as observers. One, the writer, had almost no previous experience of the sort; the other, a gentleman, had thrown some, but "not enough to amount to anything." Ten throws were made in close succession, then a pause until the observer was rested, then ten more throws until fifty had been made. The experiment covered 16 days of 50 throws each, these days occurring, with few exceptions, in uninterrupted succession.

The experiment was not prolonged far enough to give a satisfactory learning-curve, such as has been found for similar work by other experimenters, *e. g.*, Bair, Book, Swift, and others, since general introspective results were the main object of our work. It may be said, however, that the greatest gain, both in uniformity and amount of score, came for each subject in the first few days.

In learning to throw at a target one must specialize and perfect certain elements of the complex mass of neuro-muscular co-ordinations of which he is in possession through inheritance and his own practice in general activities. His consciousness is taken up primarily with the target and the ball in his hand and vaguely with those particular and general bodily sensations which enter in to make up the "set" of the situation. Only gross errors, such as standing too far to the right or the left, or throwing with too great or too little force, are consciously corrected. The minuter, more skilful adjustments developed of themselves out of the larger, less perfect ones already existing, and were then perhaps consciously continued or avoided. Attention to the mechanical side of the throwing only resulted in inferior work; yet clear consciousness was necessary for good results, but it was consciousness of objective elements—the target and to a certain extent of the ball—rather than of

one's arm or its movements. After a little practice both observers mentioned the fact that greater concentration and fixation of attention on the target resulted in better throws. When greater effort consisted in deliberate attention to the *mark*, good throws resulted; but when, as occasionally happened, the observer tried to regulate the *process* and attended to the hand, arm, or ball, random shots were sure to occur. One observer remarked: "I don't believe thinking of the thing will do any good. All I can do is to stand before the target and *wish* to throw well;" the other said that he was simply trying to make good throws but did not know how he did it.

Good physiological condition, interest in the work and a tonic muscular condition seem to be concomitants of success, for when observers are ill they lack energy and interest, throw almost listlessly, and with poor results. On good days they stand erect with muscles tense and eyes fixed on the target and do their best work. One observer at such a time even found himself forcibly squeezing the ball. The amount of energy put forth grew more regular with practice, *i. e.*, there was better co-ordination.

The introspections show that, in such an almost purely sensory-muscular process, skill develops without consciousness of the details. The peripheral sensations accompanying or preceding the reaction contribute to form the background of consciousness and to produce feelings of satisfaction or dissatisfaction according as the movements are rightly or wrongly made. Consciousness has little place "as guide," save in the grosser features of the task, but attentive consciousness of the end was necessary for the development of these peripheral adjustments. Clear consciousness seemed to be accompanied by a general neuro-muscular tonicity favorable to the best work. Probably with clear consciousness the organism is acting more as a unit of closely knit parts, each of which is then more effective on every other part while it is active, than in a state of disintegration where association is loose.

4. *Learning to Write in Unaccustomed Ways*

The experiments of this series were, like the last, of a simple sort, though they involved skill of a somewhat greater complexity. They consisted in learning to write ordinary script with the left hand, and mirror script with both the right and left hands.

Left-hand Writing. In the normal script experiments eight observers assisted, four of whom were men and four women. All were trained psychologists, except one woman who, nevertheless, had had much practice as an observer and was

excellent at introspecting. One observer was almost ambidextrous, two were left-handed. No definite tests of mental type were made, but the indications are that two observers were, in this sort of work, predominantly motor.

The conditions of the experiment were kept as nearly constant as possible for each individual during all his work. The experiment covered a period of about fourteen days. The standard sentence, written by all, was, "Motives are like chemicals. The more you analyze them the worse they smell." This the observer repeated several times before beginning to write in order to learn it. The sentence was written three times with the right hand with timing (with a stop watch); then once without, and three times with the left with timing and once without, with sufficient pauses between tests to avoid fatigue. It was explained to the observers beforehand that the timing was merely an incidental matter and that they should write at a convenient speed, merely writing each sentence continuously. After each sentence had been written, the observer was asked to give introspections as to methods used, points attended to, and any other items which might be of interest.

Inference as to the part "unconscious" factors play must rest partly on the fact that the observer fails to mention them and it is therefore open to the error of supposing that facts not remarked upon are unconscious, whereas the fault may be due to incomplete introspection or report. Yet it was impossible to ask definite questions as to position or methods, for then entirely unnoticed factors became clearly conscious and the subsequent course of procedure was apt to be changed. Exact objective measurements of improvement in writing are naturally, impossible, but must be judged in a rough way by greater uniformity in the slant and strength of the characters, and by their greater clearness and legibility.

The different observers manifested individual differences in their adaptation to the task, their methods of procedure and the speed and proficiency acquired; yet there are elements common to all. It is evident that easy and natural writing movements with the left hand cannot be made unless one assumes a position nearly symmetrical to the customary right-hand position and lets the hand take a free and uncramped movement. This will result in script with a "back hand" slant of a rather uniform character if one writes on a horizontal plane. It was to this position and writing that all observers tended, though they arrived at it in various ways and adopted it to different degrees.

Only three observers assumed an entirely symmetrical position from the start. Two of these, who were left-handed

observers, did so unconsciously, guided purely by the "feel" of the thing. The other observer analyzed the situation, discovered that this would be the proper way, and so took the position voluntarily. A fourth observer assumed a position almost symmetrical, and one which was little changed during the progress of the experiment. This he said he did "consciously and unconsciously," *i. e.*, semi-consciously. Of the remaining four observers all began with the paper in exactly the position used for the right hand, with the body turned at the same angle to the table and the left hand and arm twisted into an awkward position, the wrist cramped over to the right side of the body. With one observer the right hand held the paper at the upper left corner, taking the position which the left hand had always used, thus showing that each hand had changed places with the other.

All observers, save one, finally used some finger movements for the left hand, but only four began with them. Two of these were left-handed and one ambidextrous, and used the movements unconsciously. Observer V, who analyzed his position and assumed a symmetrical one consciously, used finger movements at first, but after the first sentence the natural tendency to use larger arm movements manifested itself, and the finger movements disappeared. One observer made them to get out of a difficulty and after that tried for them; another noticed her right hand carefully while writing, observed the finger movements, realized that skill could be obtained only if the finger movements were used in the left hand, and therefore consciously adopted them, but with considerable effort, and it was only when attention was directed to the hand that they were constantly made. Another observer "found finger movements coming of themselves" and continued them because the writing as a result was better, but even then they were hard to keep. Of the observers who did not use finger movements at the start, one adopted them on the second day, one on the fourth and one on the fifth. The only observers using finger movements naturally are those possessed at the start of some skill with the left hand.

Four observers went from a larger to a smaller hand, three to a slightly larger one, and for one, size remained about the same.

The large movements at the start may be due to one of two things. They may be the result of a general tensing up of all the muscles in the intense effort of the new occupation, and a general spread of energy over the whole body—a thing which could be observed in the tense muscles of the hand in five cases; in the digging and scraping of the pen in three cases; and in tension about the mouth, head-movements or

raising the heels from the floor, some of which were noticeable in all but the left-handed subjects. Or they may be due to the fact that the muscles involved in the larger movements have been trained in many daily occupations while the finer movements have been very little practiced. In the progress of the race one hand has been specialized for the more skilful work, the other hand (in most people, the left) being used far less. The left hand of an adult just learning to use it in left-hand writing, is in about the same condition as the right hand of a child who learns writing for the first time. The child's arm and hand have been used in larger activities, but the finer adjustments have not been practiced. When, therefore, the child and the adult begin the new task there is in both a general innervation of all the muscles and the larger movements are first made. The finer ones together with economy of energy appear later.

Writing in reversed slant appeared to a greater or less degree with all the subjects. In three cases the natural tendency was noticed in a few strokes and consciously continued; in two it was the result of letting the hand take its own position and "swing." Another tried to let the writing take its natural slant, which finally resulted in "back hand" script.

Improvement is characterized subjectively by a freeing of attention from the writing itself so that the observer is able to attend to details, to correct errors, and to make improvement in methods. Attention at first is so absorbed in the writing that the observer is not aware of his awkward methods. One by one he notices these and corrects them. Observers starting with good methods have fewer difficulties at the beginning and are able to anticipate them sooner. Attention is not only differently directed, but far narrower at the beginning than at the end of practice. As skill begins to develop, consciousness is wider and attention can shift from the task to extraneous matters with little disadvantage; where, as in the beginning, wandering of attention means distraction, and the work suffers. The relation, which exists between late and early conditions, exists also between the right and left-hand writing. In the latter, attention is easily disturbed; a strange pen, a slight illness, or a simple external hindrance have far more effect.

The general results of these tests with left-hand writing show the rôle of consciousness in learning of this kind to be corrective, its function being to criticise, to eliminate habits producing either physical discomfort or dissatisfaction with the product, and to make permanent any favorable variations which may chance to occur. The focus of consciousness

changes during the learning, attention at first being on the process itself, the details existing in consciousness only marginally or not at all. Later the learner attends to his methods and at the same time is more clearly aware of the elements leading to his satisfaction or dissatisfaction. As the methods are perfected they in turn become automatic, the learner assuming automatically the position which he has acquired consciously. As the process becomes still more automatic, attention wanders from it from time to time to foreign matters, without interference.

Mirror Script Experiments. Experiments in learning to write mirror script (that is, writing which begins at the right-hand side of the paper, and may be read by holding it up to a mirror or from the reversed side of the sheet) were carried on for a period of fourteen days with six observers, all of whom had served in the above mentioned left-hand experiments. The general conditions were the same as before. The subjects wrote the standard sentence three times with the right hand and three times with the left, and *vice versa* on alternate days, the writing of each sentence being timed as before except in two cases. No untimed tests were taken save in the case of two observers. As timing seemed to have no effect, untimed experiments were not made by the others.

The greatest difficulty was noticed by all observers in the first few trials, and consisted in knowing what the form of the letters should be. A certain amount of extraneous practice was allowed in order to meet this peculiar hindrance. Two observers began by writing on the blackboard with both hands at once, mirror script with the left hand and normal script with the right. This was easier than the writing with the pen, which required smaller movements. The other observers, seated at a desk with paper before them, were told to write the sentence in mirror script, after it had been explained to them what mirror script was, and were allowed to write the sentence, to hold the paper to the light and to correct mistakes.

Attention at the start was confined to the writing as a whole, but soon general difficulties decreased and particular ones were attended to, certain letter combinations being more difficult than others. After trying to make a letter of a certain more difficult form, the observers consciously chose a simpler style. As in the normal script experiments, excessive muscular tension was shown at first but later disappeared. With ease in writing, foreign ideas again begin to enter in every case; but attention cannot get too far from the process without disastrous results. One observer, for example, became so absorbed in a train of thought that he stopped writing. Ease of writing and freedom of attention, as before, allowed

difficulties to be anticipated and overcome before they were met. One observer consciously pronounced the difficult letters, because he found himself doing this in one instance with good results. Two observers visualized the movements, in difficult places, before making them. No observer mentioned attending to the process itself as a means to improvement, but two stated that attention to the process brought confusion.¹

Had there been a good copy to give an idea of the letter forms, and had instruction been given as to position and relaxation of muscles in hand and arm, much of the difficulty would probably have been obviated.²

Learning to write, as evidenced in the above experiments, depends on consciousness mainly for perfection of methods. Adjustments which are at first "unconscious" become highly conscious then later automatic, a great degree of perfection requiring the third stage—automaticity. It is only as the grosser elements become automatic that attention is free to consider the finer ones.

A certain degree of difficulty is necessary to interest. As the task becomes automatic and easy, it is impossible to keep foreign ideas out of mind.

Progress is from coarse to finer muscular adjustments, and from larger to finer writing in most cases. This means a specialization of the smaller finger and hand movements, and a saving in energy, since less exertion is needed to call these into use, than for the larger arm movements.

Progress may take place without a high degree of consciousness, yet it will not go so far nor proceed so rapidly as when there is consciousness of the process itself.

5. *Learning to Multiply large Numbers Mentally*

The experiments of this series consisted in learning to square three-place numbers mentally, and were suggested by recent work of Thorndike in multiplying mentally a three-

¹The matter of increased speed in the writing does not especially concern us here, though the records were kept and tabulated. It may be mentioned in passing, however, that there was very often to be observed an increase in speed in the left-hand writing, in the mirror script and even in the normal writing with the right hand, from the first to the third executions of the standard sentence in a single test—a transient gain in skill by practice of a particular set of movements.

²In any instruction it is just this which should be the function of the teacher, *i. e.*, to provide good methods and to call attention to errors which the narrow attention of the learner will not enable him to see. In a task like writing much mechanical repetition is needed; yet repetition without attention will not result in improvement but merely in the strengthening of abilities then possessed, and even of awkward procedures.

place number by a three-place number.¹ He found in the 33 observers who did from 28 to 96 examples of that sort, a gain of over 50 per cent. in skill, but gives no introspective results, except that strength of visual imagery was not responsible for the improvement, and that more individuals reported decrease than increase of visual imagery. My own experiments were carried out in a manner similar to his, except that for simplicity's sake my observers squared one number instead of multiplying two different numbers together. This made it necessary to hold but three digits in mind at the start, instead of six. In making the number list, digits above two were written on cards and drawn at random from a box. If a number contained the same two digits as the number before it, it was given a later place in the list, in order to avoid the distraction, or aid, of too great similarity.

The manner of conducting the experiment was as follows: A number was read to the subject, the observer repeated it, and the stop-watch was started. When he finished the example he gave the result, the watch was stopped and the time recorded. In the first day's trials the observer was asked to work aloud, but in some cases this proved a distraction and was not required later, though those who wished to work aloud were permitted to do so. In reckoning the results Thorndike's arbitrary method of transmuting errors into time by adding to the watch time one-tenth of its amount for each error made, was used.

Preliminary tests consisting in running through the forty-nine two-place numbers possible from combinations of digits above the digit three were carried out with two observers, and took six and five days respectively. In this short time great improvement was made; for observer A the average daily score in seconds for each multiplication was 51.1, 40.1, 35, 43.1, 41.9, 30.6, and for B, 81.7, 29.1, 26.2, 16.3, 21.4. (B's last score was raised by one exceptionally long time where the example was worked twice. If this one case be left out the last score is 16.2.)

Introspection showed that the gain was partly the result of the refreshing of the mathematical associations, *i. e.*, practice in multiplying and adding, but far more, of choosing and using new methods to avoid obvious difficulties, and of improvement of methods already in use. Ideas as to means of improvement were not the result of analysis previous to work, but came after some practical experience, when the observer was oriented and realized his deficiencies. The formulation of

¹Thorndike: The Effect of Practice in the Case of a Purely Intellectual Function, *Am. Jour. Psy.*, XIX, 1908, 374-384.

such improved procedures was really a process of generalization. When the same peculiarity occurred several times, the observer recognized its universal character spontaneously, and not as the result of conscious search after it. The common element seemed to drop out of itself. Then, because of his strong desire for improvement, he consciously made use of it; but had he not been alert, it might easily have escaped his notice, and have been of no profit to him in his progress.

Instances of this process are the following: After a few examples *A* realized that, with the numbers in use, the answer must always have four digits. Visualization for this observer was at first impossible, the whole process having to be carried on in auditory-motor terms. The two partial products were retained as a sound whole; and to get the separate digits for the addition the observer must run through these products several times till the required digit was found. After several such experiences she realized that it was only in an auditory-motor way that work could be done, so in adding, she repeated the first partial product through as far as the units digit, held that in mind as the units digit of the complete product, repeated the first partial product again as far as the third digit from the left and the second partial product as far as its final digit, added these two and placed them in the tens place of the complete product, and so on. Later, visualization increased to some extent as the result of extreme effort, but remained almost entirely visualization of a special "form" into which the digits were fitted as they were required. After practice in addition, it was noticed that the first digit from the left of the second partial product and the last of the first partial product had no digits above or below them to add to them; and consciously less attention was given to them and more to the other four digits. Again, having worked slowly and deliberately so that one partial product escaped her by the time the other was obtained, she worked more rapidly in subsequent multiplications, spending more time repeating and emphasizing the results.

Observer *B* worked a single day, multiplying the numbers out by full multiplication. He then served as experimenter, with *A* as observer, and while so doing realized that the binomial method might be used, and used it in going over *A*'s work. In his next work as observer the change of method reduced his record from 81 to 29. The process was first as follows: Required to square 35. $a^2 + 2ab + b^2$, $35^2 = (30 + 5)^2 = 30^2 + 2 \cdot 30 \cdot 5 + 5^2$. Later he noticed that a^2 always ended in two zeros and simplified the process by simply setting together a^2 and b^2 then adding $2ab$. Then the method unconsciously came of getting $2ab$ while repeating $a^2 + b^2$, was recognized as a method and continued. Later, while making notes, it occurred to him that in multiples of numbers ending in 5, $2ab$ will be the first digit $\times 100$. This was then consciously used with success. Superfluous words fall away in the process, only numerical results being given. This last was unconscious, however, as to intent. In all such work, a rule which is first conscious becomes an unconscious habit.

In squaring three-place numbers six persons served as observers, three men and three women, all of whom were university students. Observer *V* is the same as observer *A* of the two-place number experiments. The observers were practiced for two days on two-place numbers and three-place numbers, each subject working three three-place problems before the regular experiments began. The results included in the tables are only those of the regular experiments. The rec-

ords are based on the fifty examples worked by each subject, and cover between ten and fifteen days. Each worked half an hour a day. The method of timing was that described above for the two-place numbers. A rest of from two to five minutes between problems was given. If the original number was forgotten in the midst of the work, that problem was given up, and after a rest, a new one taken. As the observers worked at different rates, and as some forgot more numbers than others, the number of days taken varies. The following tables give the average results in errors, time and combined result for the six observers.

A decided gain is clear as far as speed is concerned, though accuracy seems to remain about the same, unless in the case of observer IV, where there is slight improvement. This, however, may be a matter of chance. The accuracy corresponds to one's skill with the addition and multiplication tables, which have been so much practiced that they have reached a "plateau stage" where no further improvement is likely. Improvement, in this experiment, is not in accuracy of work, not in the speed of computing (at least not to an observable extent) but in the ability to hold more things in mind and to attack the work directly and with more advantageous methods. The asterisks in the table indicate the points at which new methods were introduced. An asterisk occurring before the first day's score indicates that a method of work peculiar to the observer was developed in the preliminary three-place examples. Observer VI began the first day's regular experiments with a method which required the retention of but a few numbers at a time and made use of no new method. Observers I, II, IV had developed methods before this day, but made improvements in them during the progress of the experiment. It will be noticed that introduction of new methods resulted in a large drop in the time, except in the case of observer IV, who did not continue methods used after they were once developed, except the general method used on the first day.

The most difficult part of squaring three-place numbers by full multiplication was found, by all observers, to be the retention of the partial products long enough to add them, and to add the proper digits together. As they were allowed to work in any way they chose, the effort of each was to find some way to lessen this difficulty. Visualizers, as might be expected, had less difficulty than observers with little visual imagery. A brief account of the procedure of each of the six observers will best show the methods of improvement. Fatigue or distraction made retention difficult for all the observers and affected chiefly that part of the work.

TABLE III

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Obs. I															
Errors	2.	.5	.6	.3	.25	.6	1.	.6	.5	1.6	1.2	1.	.6	1.0	1.0
Time	420.5	252.5	162.6	263.3	197.7	149.9	112.1	127.6	85.5	130.9	105.7	72.2	103.6	120.9	116.1
Comb.	504.1	265.7	173.4	269.	202.6	159.9	123.3	136.1	89.6	152.7	118.4	81.2	109.8	132.9	127.6
		*			*										
Obs. II															
Errors	3.3	1.5	1.3	5.2	2.0	1.5	2.0	1.4	2.5	1.8	3.0	1.6			
Time	102.7	140.3	113.5	98.3	71.6	78.7	86.9	87.1	104.7	81.4	60.8	52.3			
Comb. *	136.5	165.8	128.6	174.9	85.9	90.5	91.3	99.3	130.8	96.0	76.5	60.6			
					*										
Obs. III															
Errors	3.	2.6	3.2	2.0	2.5	.6	1.8	1.6	2	1.6	1.4	1.2			
Time	178.2	128.6	145.7	149.2	169.7	167.2	112.	134.4	145.8	124.3	127.8	108.			
Comb. *	231.8	162.8	193.1	179.0	212.1	177.3	132.2	155.9	175	155.4	145.6	109.4			
		*		*				*		*					

TABLE III (Continued)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Obs. IV														
Errors	1.3	1.3	1.0	1.3	.7	0	1.7	1	.4	.6	1.2	.6	2.	3.
Time	325.9	263.1	399.6	246.0	325.2	313.6	254.9	172.3	180.4	184.9	199.6	191.7	168.8	161.6
Comb.	369.4	271.1	439.6	278.8	349.8	313.6	263.4	187.3	187.6	196.0	227.1	204.5	202.6	210.0

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Obs. V														
Errors	2.0	2.0	1.3	1.0	2.	1.3	1.0	2.0	1.6	2.0	1.0	2.6	2.5	2.0
Time	332.9	409.3	304.5	333.5	285.1	222.6	206.8	157.7	219.0	152.6	240.0	242.9	151.1	177.7
Comb.*	399.5	487.9	345.1	366.8	342.1	252.4	227.5	189.0	163.8	182.9	264.0	307.6	188.9	210.0

	1	2	3	4	5	6	7	8	9	10	11
Obs. VI											
Errors	4.0	1.5	1.6	2.0	1.1	1.5	1.0	1.0	1.0	1.2	1.3
Time	96.7	60.2	67.1	59.7	42.1	39.3	51.5	37.3	46.5	44.9	37.8
Comb.*	132.1	69.2	77.8	71.7	46.9	43.3	56.6	41.0	51.2	50.5	42.8

Observer I. This subject possessed no visual imagery for this sort of work. By the second example of the preliminary practice, he was consciously repeating the first partial product to the last digit, setting this in the answer; then the third digit of the first and the last of the second were obtained in the same manner and added, etc. As the required figures were taken out and embodied in the sum they were forgotten. The next day he tried to add two columns at once. On the day before the regular experiment he was permitted to make writing movements with his pencil, which he did henceforth, finding it an advantage. On the second day of the regular tests he tried remembering the numbers by pairs. (Notice the decrease on this day from 504.1 to 265.7.) On the fifth day the very advantageous idea of adding the first two partial products and adding the third to their sum was hit upon, thus having at no time more than two numbers to keep in mind. After this the drop is decided. The increase in time in the last three days was due to fatigue from other work earlier in the day.

Observer II. This observer is a good visualizer and has had experience in teaching mathematics. On the first day the observer tried to square two numbers, but did not succeed in either case, and refused to work. The next morning while thinking of other things, a method occurred to her, which she used with success throughout the work. It was to multiply the number by the multiple of a hundred which stood nearest, then by the hundreds digit multiplied by ten, and adding or subtracting the result, according as the first multiplier was below or above the original number, then multiplying by the units digit and adding or subtracting. Thus only two sets of numbers had to be retained and work was considerably in ciphers. For the first two days of the regular experiment a few seconds were taken to think out the method of work, but later this became unnecessary and the problem was attacked directly. The method had grown automatic. On the fifth day a permanent modification occurred in adding or subtracting by thousands, tens, and units visually. A question by the experimenter as to the way the work had been done suggested this.

Observer III—A visualizer. The first three-place number was declared to be "terrible," the greatest difficulty being to retain and add the partial products. In the second example she added the first two partial products and then the third. The next day the method was improved further, consciously, by getting the sum of the first two partial products before multiplying for the third. The regular experiments were begun with this method, which was afterward modified but slightly. The modifications were as follows: "putting" the first partial product on all the four fingers of the left hand and the second on all but the little finger and including the sum. This resulted in the thumb and little finger having only one digit, and the others two; this method was not continued. The third partial product was "put" spatially above the sum of the first two on the fourth day. On the eighth day she "put the first sum in the left ear" and got it again when needed. From the first, the observer automatically made writing movements on the table to accompany her work.

Observer IV (no visual imagery for this sort of thing). The observer developed no method until the sixth day except the repetition of the partial products until he came to the digit required for the addition, as described for observer I. He consciously hurried through the process of multiplication in order to spend the time on repetition and emphasis of the result. Notice the decided decrease in the score after the sixth day when the new method was taken by adding the figures as soon as they were obtained instead of first multiplying for each partial product. Subjectively the work was much easier after this way was taken.

Observer V (the writer, little visualization for this sort of work). The method of observer III, namely, the addition of the first two partial products was deliberately borrowed and used in the first day's work. The

calculation was consciously made as rapidly as possible that the parts might not be forgotten before the whole had been secured. On the fourth day the last two digits of the first sum were discarded leaving only a three-place number to add. Figures when they were required for addition were fitted into a visual "number-form" which had unconsciously developed. On the sixth and eighth days new methods were tried which worked well at the time, but were used only on that occasion. The first was to add the first and third products first, the second to place the digits on the fingers of each hand and set in proper juxtaposition. It was evidently forgotten. Two column addition was consciously tried and used in instances when the numbers to handle were not too large.

Observer VI. This subject developed his method in the practice tests and did not change it; improvement for him therefore consisted in practice in the use of his method. It was to break the number up into two numbers, the first consisting of the hundreds and tens and the last of the units, and to use the binomial method of squaring; in obtaining the square of the first (two-place) number, the a^2 , the binomial formula was also used. This method occurred to the subject after he had gone to bed on the first day of squaring a three-place number by full multiplication, and he considered it an original method until several days later a distinct visual image of his old arithmetic book with its thumb-worn page bearing an illustration of a formula similar to this for extracting the square root, flashed up before him. Part of the original experience, without its localization in time or place, had been recalled, and given him what he considered an original idea. Stronger stimulation of the complex brought it back in all its original setting. By the third day introspection shows less attention and strain than at first, the method had become spontaneous and it was easier to keep two different sets of numbers in mind. Practice had resulted in a widening of the field of consciousness. The sixth day the observer said the work was easier because he got his results almost at a glance and when they came he saw the figures under the ones to which they should be added. When he added a certain column he saw only these figures distinctly; the others were hazy, but he could call them up when he wanted them.

At the close of the experiment, each observer was asked to give a report as to what he thought his improvement had consisted in, what part of the processes was conscious and what unconscious. All said the task was easier at the close than at first. Observer II, who developed a very simple method at first, said that after the first few days she was conscious of little improvement. Three observers attribute most of their improvement to the adoption of an easier method, and three assigned "practice" a large place. For all, the calculation itself was a highly conscious affair, though for three, the results sometimes seemed to come spontaneously. Adoption of new methods was in every case clearly conscious, and not from falling into a certain habit, noticing, and continuing it, as was the case in the target throwing and the writing. Very slight suggestions from one's own work or from outside were often responsible for the idea of the new way of working, or it "just popped in" as one observer stated. However it came, the idea had to be there to effect the change. After the method was practiced a little, it was used directly and with-

out thought. "Unconscious" improvement came in the widening of the conscious field, adaptation to the experiment and the like, so that the feeling of strangeness and awkwardness disappeared. What at first seemed an impossible task no longer looked so when one became oriented. Possibly another "unconscious" factor was the gain in speed in making computations as the result of greater familiarity with, or rather, refreshing of, the addition and multiplication tables. Mere practice caused improvement in the use of methods consciously adopted.

6. *Results of the Last Three Series of Experiments*

The results of the last three series of experiments seem to agree in showing that the function of consciousness in learning is to improve the process by bringing errors to light and correcting them, and by adopting improved methods suggested by some habit fallen into, or by some idea as to better possibilities. The more purely muscular the process to be learned, the less conscious the learning of it. In the target throwing improved methods of throwing came about of themselves and were not noticed until later. Attention to the mechanism only resulted in disaster. The most one could do consciously was to attend closely to the bull's-eye and throw, the proper co-ordinations seeming to take place of themselves; gross errors only were consciously corrected. In the writing experiments, consciousness played a greater rôle in supervising and correcting the process, and for some observers in starting an advantageous method. In the intellectual task of squaring a three-place number every decided step in advance was the result of a conscious change.

But these three grades of learning all showed "unconscious" improvement as the result of repetition (even the arithmetical computations), improvement which was entirely at the physiological level. Improvement, therefore, does take place without the control of consciousness. Yet even at the grade of learning where this is the truest, we cannot say that one is unconscious, but perhaps rather that marginal awareness, in the sense of organic and peripheral sensations, and feelings of satisfaction and dissatisfaction, is always present and affects the result. It is, one may conjecture, a feeling of dim awareness akin to this unanalyzable, undifferentiated state which accompanies the learning of animals low in the scale. That it *directs* the learning is at least not certain, if one reasons by analogy from human learning where only the higher, more specialized acts are under conscious control. The simpler and more "muscular" the learning, the more vague and

indefinite the subjective accompaniment. Practice alone is the improving factor. In more complex processes like writing the learner is able to assume an objective attitude and direct and criticise his own activities and to shorten, by choosing new methods or avoiding observable mistakes, a process which would otherwise require much mechanical repetition. In still higher operations, like arithmetical calculations, consciousness of the process is still clearer. It acts vicariously for practice, which takes a subordinate rôle. The rôle of consciousness is similar to that of the teacher who can do little for one learning feats of muscular skill save give a few simple instructions, leaving the rest for the pupil to get by the hit and miss of practice; but in more complex activities he can act as a pattern, giving methods and pointing out deviations from them. Since right methods and easier work result in a widening of consciousness, this will leave the pupil's attention free for still further advances.

SUMMARY

In Part I we have considered consciousness as an ultimate fact, undefinable, identical with awareness. Unconsciousness denotes for us its opposite—entire absence of awareness, that which is entirely outside of our experience at any moment of time. Subconsciousness, for which we prefer to substitute "perceptual" factors, gives focal consciousness its qualitative character. Subconsciousness is *consciousness* of a less distinct degree. Divided consciousness, such as is present in cases of multiple personality, is best called "co-consciousness."

The question of the existence of "unconscious psychic processes," *i. e.*, psychic accompaniments of physiological processes lacking awareness, depends for its answer on one's metaphysical concepts, which are in the end a purely temperamental matter. Denying them leads to the interactionist position. Throughgoing psychophysical parallelism demands the assumption of psychic factors accompanying physiological changes, and this position we have taken, insisting, however, that such "psychic" processes are qualitatively different from anything which enters into consciousness. According to such a view one may speak of physiological processes in addition to "unconscious psychic processes."

We have incidentally reviewed the arguments *pro* and *con* as to the presence of "unconscious" factors and their influence on mental phenomena. Our main interest was, however, in the relation of consciousness to learning. From general observation we have seen no case of learning where one is absolutely unconscious, yet one may be unconscious of the

end, the process and even of the development of the habit or association itself.

Our own experimental results are the following:

1. Our experiments on the nonsense-syllable material give chiefly negative results, but justify, so far as the conditions of the experiment permit, an inference that what is entirely outside of consciousness, though it is in such a position that it might easily become conscious, has no great effect, positive or negative, on the learning of the same material when it is presented later to clear attentive consciousness.

2. In the experiments on the "Motor Set" (*Motorische Einstellung*) we find that a habit may be formed despite the fact that one is unconscious that one is forming it. Yet, withal, attention to the task produces in all cases a more definite habit, a stronger "*Einstellung*," than that which is caused when one is almost unconscious of his performance. Attentive consciousness without doubt is accompanied by greater tension in the particular muscles involved in the current activity of the organism and in their nervous connections. Here activity is concentrated. The more fully the physiological mechanism is thus put into activity the more it is affected in the direction of easier and more efficient activity of the same sort.

3. The experiments on throwing at a target involved learning of a sensory-motor kind, the doing of a definite thing: it was practice with a fixed aim in view. Here focal consciousness was almost entirely projected on the target, the ball and hand occupying a peripheral place. Conscious control was exercised only over the grosser parts of the process. Methods gradually changed, and improvement appeared, without conscious change or control. The sensations from the arm and body no doubt contributed to the improvement, but these were always at the "perceptual" level and consisted rather of an undifferentiated background.

4. In the writing experiments conscious direction of the process and methods was more marked. At first consciousness is bound down to the general execution of the task. The more general, larger elements, becoming automatic, leave consciousness free to turn to details, when disadvantageous methods are noticed one by one and eliminated. Unconsciously modifications in the method crop out, and as consciousness becomes freed from details these are noticed, practiced, and improved upon. This sometimes results in a considerable change of adjustment of the different factors.

5. In the experiments on mental multiplication consciousness had a more immediate effect than in the more "muscular" sorts of learning. Here advantageous methods occurred to the

subjects while they were working, or between the experiments, and when these were adopted the improvement was immediate and permanent, whereas in the more "muscular" sorts of learning one's muscular co-ordinations had to be practiced somewhat before the new method was perfected. In the number experiments, just because one is alive to the situation, he notices clumsy methods and slight errors, and is therefore ready to improve upon them. After a method was consciously developed, however, it was soon used unreflectingly—it became a habit. In proportion as an activity is conscious, consciousness is an aid or even an essential factor in its acquisition. This applies to details and part-processes as well as to the larger units of activities.

CONCLUSIONS

In conclusion we may say that in learning of any sort both conscious and unconscious factors exist. Unconscious factors are those involved in the fixing of the association by practice, and the cropping out of modifications of behavior subsequently utilized by consciousness.

The more intellectual and highly conscious the material to be learned, the more direct and immediate the effect of conscious control. Practice results in a standing out of common features of the process; these are focalized, and generalized into rules for new and better procedure, which immediately takes place.

In complex processes involving both an intellectual and a muscular side, the activity as a *whole* is conscious. Details are gradually mechanized, leaving attention free to attack new difficulties. Factors of the activity which are at first only at the "perceptual" level become clearly conscious, are then practiced and improved upon, and finally become mechanized and unconscious again. Consciousness is a corrective agent, eliminating errors, improving on elements unconsciously developed, and organizing the whole procedure.

In learning simple muscular co-ordinations consciousness is focussed entirely on the end—on the outcome of the movement. One is only dimly aware of the different sensations and feelings entering into his bodily adjustment, and should any of these become the object of attention, disturbance of co-ordination results.

Learning can progress, however, without consciousness of the end or of the fact that one is learning, but even here a high degree of attention to one's task brings more marked results than work under distraction.

So far as our experiments go, factors never entering consciousness have neither a beneficial nor hindering effect on the learning.¹

¹The writer wishes to express her obligation for the faithful service of those who served as observers in the above experiments, and particularly to Dr. E. C. Sanford, in whose laboratory the work was done and at whose suggestion the subject was begun.

APPENDIX

Experiments with Meaningless Syllables

The general plan of these experiments has already been described in the body of the paper (pp 179 ff). It is only necessary here to record the details of procedure. The work was done with twelve-syllable series of meaningless syllables prepared in accordance with the method of Müller and Schumann's "*verschärft normal*" series,¹ except that additional letters were used to increase the possible number of syllables and to adapt them to English speaking observers familiar with German. There were 20 initial consonants and double consonants, (b, d, f, g, h, j, k, l, m, n, p, r, s, t, v, w, z, th, sh, ch), 19 finals, (b, d, f, g, j, k, l, m, v, p, r, s, t, v, x, z, th, sh, ch) and 14 vowels and diphthongs (a, e, i, o, u, y, ä, ö, ü, ai, oi, ee, oo, ou).² These syllables were presented by means of a rotating drum of the intermittent-movement type, manufactured by Spindler and Hoyer of Göttingen, which permitted the syllables to remain at rest during the greater part of the time of their exposure. (See Fig. II which shows the apparatus from the back and side.) The syllables were seen through the opening of a suitable screen (See Fig. III) in such fashion that a single syllable of the series to be learned appeared each time between two syllables of series with which at the moment the observer had nothing to do. Thus the syllables

tam pog bex

would be shown, the observer being required to learn the middle series to which *pog* belongs, but having nothing to do at the time with the series to which *tam* and *bex* belong.

The observer sat before the screen and read the syllables of the middle series as they appeared through the slit. The experimenter sat at the side of the machine to the observer's right, his movements being entirely concealed from the latter by a large screen of gray cardboard. Directly in front of the apparatus and resting on the same table was a second drum, a portion of the surface of which could be seen through a slit in the black cardboard screen before it. (See Fig. III.) The syllables were all learned by the "Treffer method," and the "Treffer syllables" were shown on this drum, which was turned by the subject as the syllables were required.

The syllables to be learned were written on strips of white paper $12\frac{1}{2}$ x 3 inches, ruled with fourteen lines and so proportioned to the drum, that after one complete presentation of the series, two blank spaces were shown before the first syllable of the series reappeared. The odd numbered members of the series—the "Treffer syllables"—were also written on strips $10\frac{1}{2}$ x 3 inches to fit the smaller drum.

The experiments fell into three series, A, B, and C, and were carried out with two trained observers S and E.

¹ Müller u. Schumann: Experimentelle Beiträge zur Untersuchung des Gedächtnisses, *Zeit. f. Psych.*, VI, 1893-94, 106.

² The series were prepared throughout the greater part of the work by a competent assistant not otherwise connected with the experiment.

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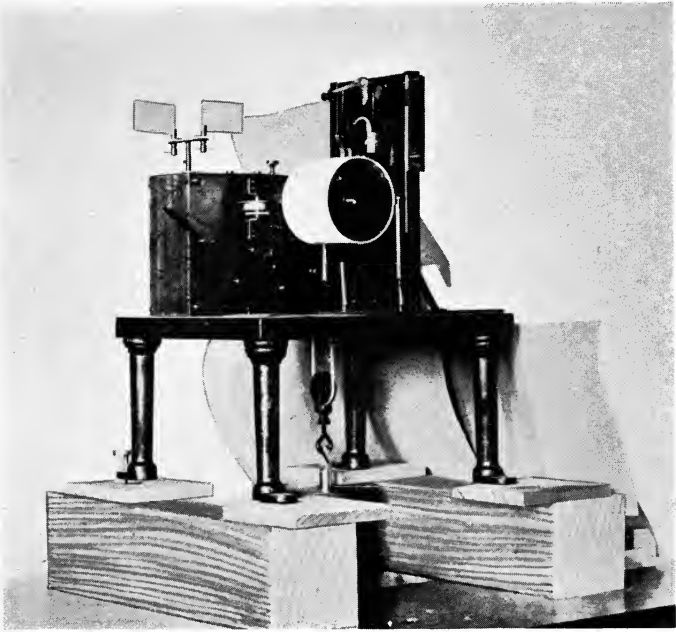
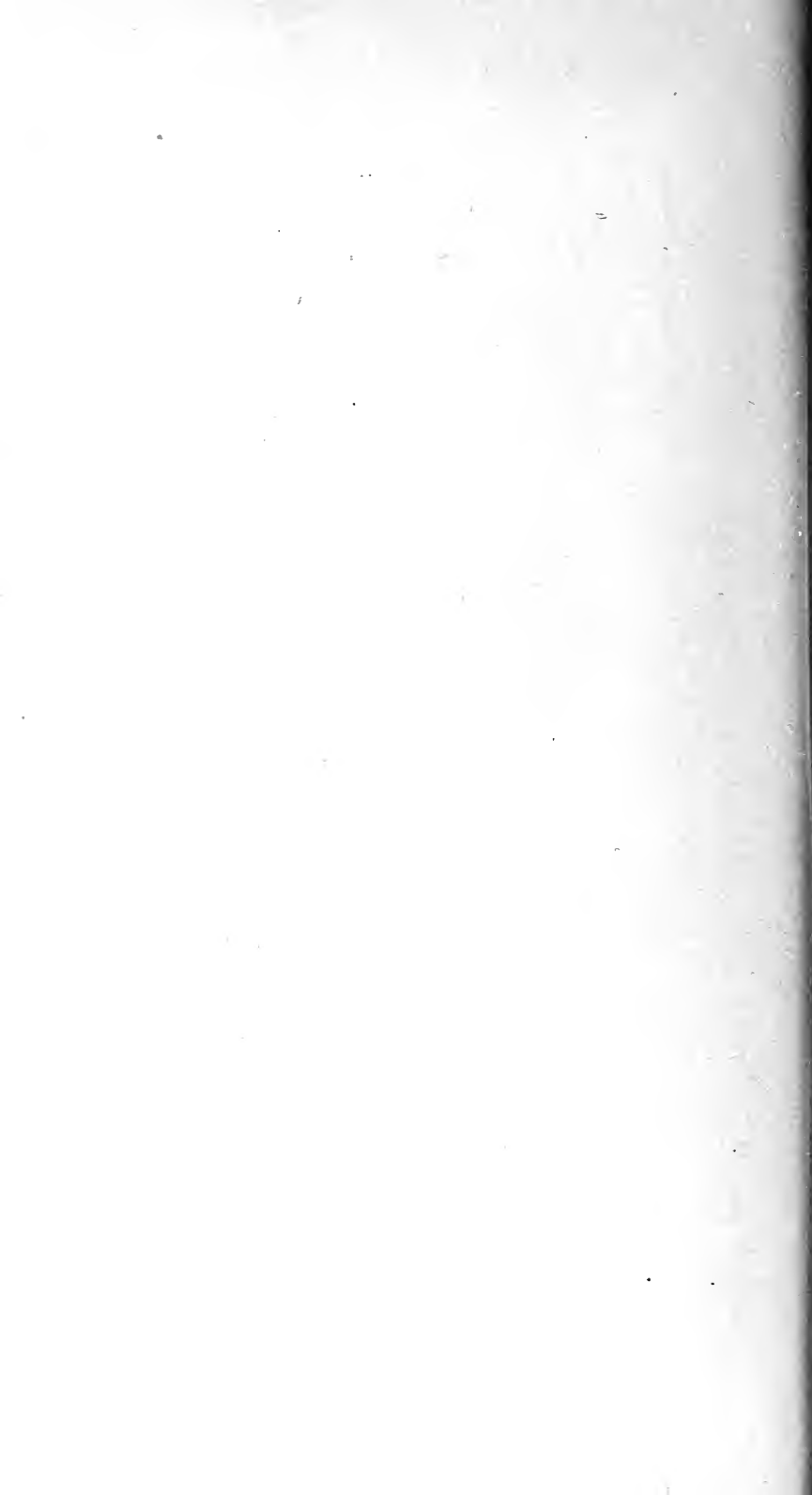


Fig. II



Experiment A. Problem: If a series of syllables is presented within the range of clear vision alongside of a second series which is to be learned, and engages the observer's attention, to find whether such a side series to which no especial attention has been given will later be more easily learned because of its exposure to possible "unconscious" perception. If Sidis's and Scripture's experiments are valid in their results, it was judged that we should find evidence of "subconscious" learning in the more ready memorizing of series which have been so exposed.

Three of the above mentioned twelve-syllable series were written in three vertical columns, so that the first syllable of each series appeared to the observer through the slit at the same time, then, as the drum turned, the second syllable of the three series together, and so on to the end. These series we shall call *a, b, c*, reading from left to right, *b* being the middle series, and the one learned.¹ On a second strip were written three series, *d, a, e*; *d* and *e* being entirely new series and *a*, the one which had stood to the left of *b*, the first series learned. On a third strip was written an entirely new set of three series, *f, g, h*; on a fourth strip, *i, h, j*, of which *h* had stood to the right of *g*, the series previously learned. All ten of these series were "verschärft" series, and the ten together constituted a regular set.

The number of series which could be used for each subject was, from the nature of the case, limited (11 sets for *S* and 12 for *E*). The method of obtaining syllables enough for all the experiments and avoiding familiarity with the syllables as much as possible was this. Syllable series which had been side syllables for *E*, but not learned (there were 6 such in every set), were transposed so that they occupied the places of *b, a, g* and *h* which had been learned, and were then given to *S*, and *vice versa*. Thus, if series retained their former lettering they would read *j, i, h*; *g, j, f*; *e, d, c*; *b, c, a*. After each subject had learned these transformed series, *E* was given the transformed series for *S*, and *S* for *E*. Then each was given the series the other had learned first, but the syllables of the individual series were shifted. This made a possibility of 552 new syllables for each subject. When these had been run through, the second and third permutation (which the subject had learned before) was repeated.

An extra series (indicated in what follows by *x*) was also used each day and was obtained by taking a strip from an unused set and learning the middle series. One set thus divided furnished extra series for four days. This extra series was given sometimes first, sometimes third and sometimes fifth in the order of the day, each subject being kept in ignorance of its position, so that he was never aware of which series he was learning. The order which was maintained for the learning was, therefore, either: *x, b, a, g, h*; or *b, a, x, g, h*; or *b, a, g, h, x*.

The odd numbered syllables of the middle series of each strip were written in the centre of a slip fitting the "Treffer" drum; here, therefore, there appeared through the slit only one syllable at a time instead of three. For the first three days, the "Treffer" order was given according to the permutations of Müller and Pilzecker² but afterwards the "Treffer" syllables were written in a direct order from one to eleven, as it was decided that a varied scheme would give less uniform results with a moderate number of experiments, and would not in any case influence the point at issue in the experiment.

The average time taken for ten revolutions of the drum during the practice work and for two days of regular work was 87.1 seconds for ten revolutions or 0.62 + seconds per syllable. This speed was accelerated on March 10 (the third day), and from then on was 80.2 sec. per 10 revolutions or 0.57 + second per single syllable.

¹ The middle series is always the one learned in these experiments.

² Müller u. Pilzecker: Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zeit. f. Psych. Erg. Bd. I*, 1900.

Each observer served as experimenter when the other was observer. Both knew the object of the experiment, but neither knew the results until the experiment was finished, as no accounts were cast until the end. With the exception of the first two days of regular experimentation, when *E.* worked at 2 P. M., *E.* served as subject from 8 until 9 A. M. and *S.* from 12 to 1 P. M. (The time for *E.* was changed because syllables which *S.* had recited at 12 persisted and acted as a disturbing factor.)

From January 24, 1909, to March 7 practice work was carried on every day except Sunday and four days of the Easter Vacation. Regular work began March 8 and was carried on every day except Sunday and one day when *S.* was out of town and another day for *E.* when the machine was out of order, making 25 days of regular work for *S.* and 24 for *E.*

At the words "In your place" the observer took his seat before the machine. This was started and the drum allowed to make one revolution to get its speed; as the first blank space appeared the experimenter called "ready," and lowered a shutter previously hiding the drum. The observer began to read as soon as the first syllable appeared, reading through the whole series of twelve syllables. After the two blank spaces had passed the series was read through again, and so on, until it had been repeated twelve times. The syllables were read pair-wise, at first in trochaic rhythm; later both observers fell into the iambic. When the last syllable was read on the twelfth revolution, the experimenter raised the shutter and started a stop watch. The observer began immediately to turn the "Treffer" drum, which was adjusted so that a single blank space preceded the first "Treffer" syllable, and read each "Treffer" syllable as it came up, giving, if possible, its associated syllable (*i. e.*, the syllable which had formed the other half of the pair); when he could not recall a syllable he said "don't know" and passed on to the next. When the last associated syllable had been given (or given up) the stop watch was stopped. This made a somewhat rough method of timing, but was effective enough for this experiment in which time was only a minor consideration. The observer was neither hindered nor helped by the fact that he was being timed. A list of each series to be learned had previously been written in a blank book and opposite each syllable was placed a check mark, if the correct syllable were given, a dash if none came, or the syllable which was given if a wrong one was given. After testing his associations with the "Treffer" syllables the observer's introspections were taken on such items as the difficulty of the series, the conditions of his attention, and influences which might have favored or been disadvantageous to the learning or reproducing of the series. The whole process—repeating the series, giving the associated syllables and the introspections—took, on an average, two and a half minutes. Ten minutes after the first series was begun, the experimenter again called, "In your place," and began the second series by starting the drum and, at a ready signal, letting down the shutter. Between the learnings a free time of about seven and a half minutes elapsed. In this interval the observer was allowed to relax as he pleased, either in looking over books, walking around the room, or gazing out of the window. But any taxing occupation which absorbed the attention to a considerable degree was avoided as it was found to have an unfavorable effect on succeeding series.

In computing the results a unit was allowed for each perfect syllable, 6 therefore being the score for a perfect record, *i. e.*, the recall of each of the six even numbered syllables. An average was taken for each of the series for the whole period of the experiment (24-25 days). A modified average was also made in order to include partial successes as follows: One-third was given for each vowel, diphthong, consonant or double consonant correctly given. Thus a syllable having only the vowel correct would score $\frac{1}{3}$ and one having its two consonants or a consonant and the vowels, $\frac{2}{3}$.

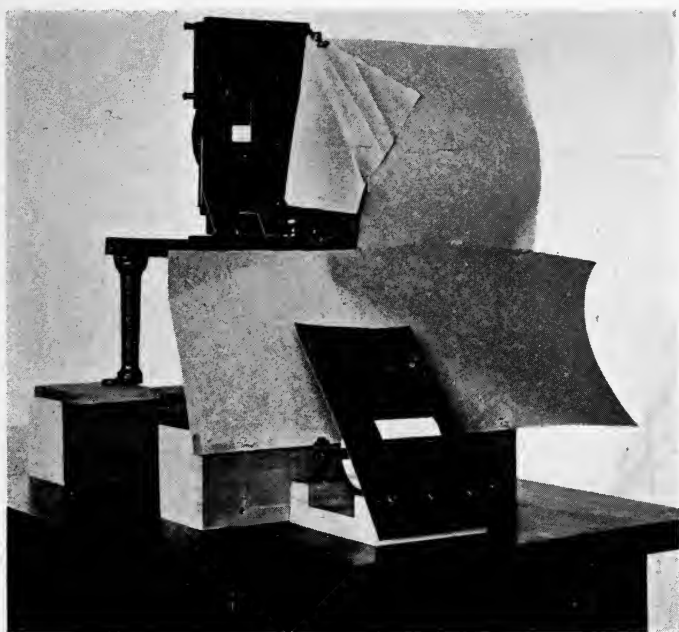
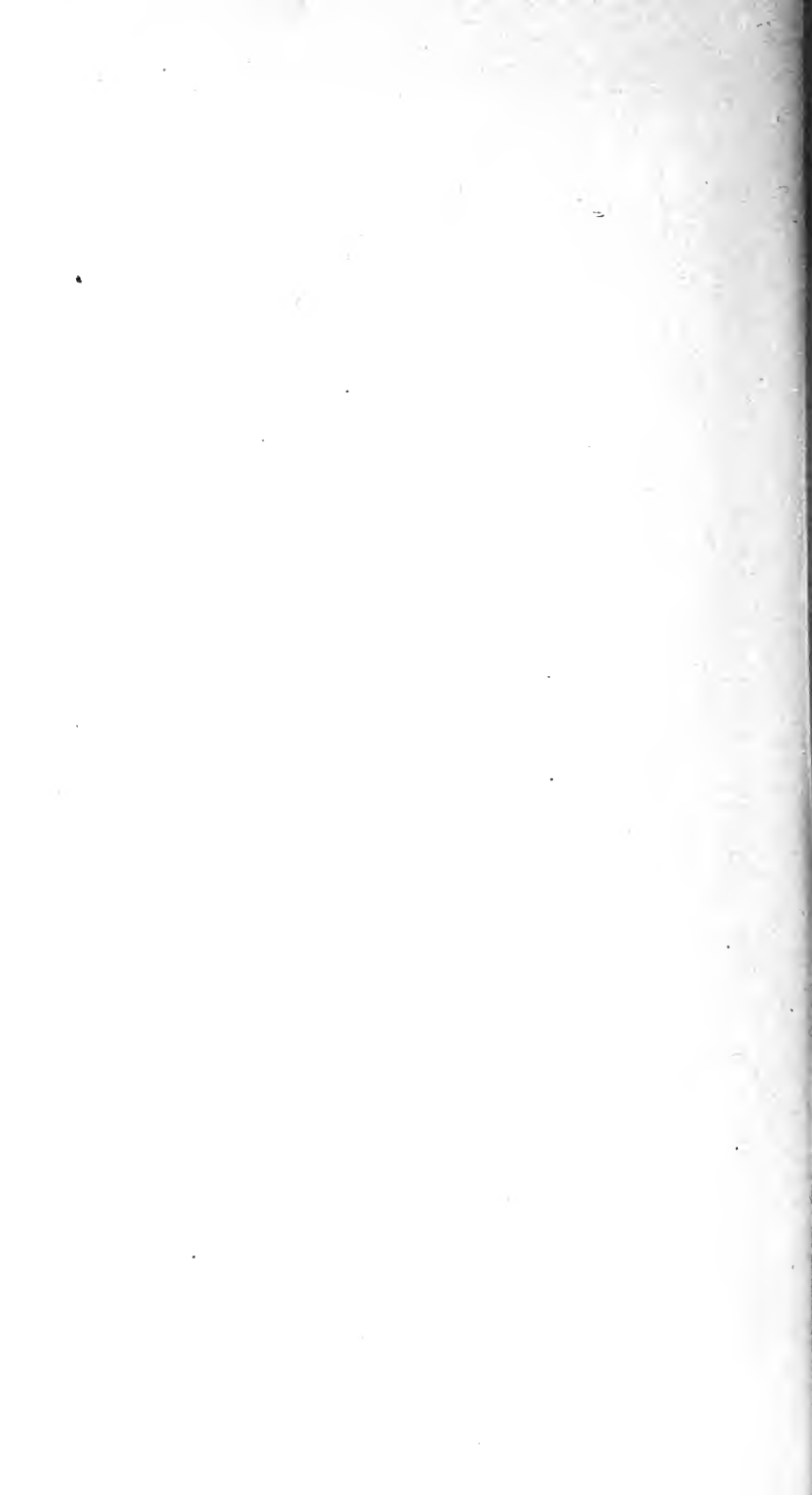


Fig. III.

Fig. III. In this figure the screen of the upper drum is arranged to show but a single syllable, an arrangement used in Experiments *B* and *C* below. In Experiment *A* it was open full width horizontally and showed three syllables at a time. The treffer drum is seen below behind the black cardboard screen.



The average of these results gives the modified average. Stated in tabular form the results are as follows: The figures in parentheses stand for the averages reckoned from perfect syllables only, and those standing free, for the modified averages.

TABLE I

Series	b	(P. E.)	a	(P. E.)	g	P. E.	h	P. E.	x	P. E.
Obs. S	(2.62)	± .21	(3.05)	± .19	(2.86)	± .12	(2.86)	± .12	(2.91)	± .18
E	3.12 (3.28)	± .13	3.21 (3.09)	± .17	3.24 (2.42)	± .16	3.19 (2.86)	± .19	3.34 (2.84)	± .15
	3.55		3.33		3.00		3.00		3.28	

For observer S the *a* series has a slight advantage over the *b* series, but the *P. E.* is so large that its advantage is quite uncertain. The *h* series is inferior to the *g* series. Taking account of subjective conditions we find that the *a* series was on the average learned under more favorable circum-

stances than the *b* series, and the *g* series than the *h* series; *i. e.*, there were more "mnemonics," the syllables were easier, or attentive conditions were better. For *E* the *a* series is poorer on the average than the *b* series, but the *h* series is better than the *g* series, if only perfect syllables are considered; if the modified averages are compared the two are equal. Here subjective conditions are slightly in favor of the *g* series. We may therefore infer from the results of both observers that the mere fact of having been shown as a side series does not favor that series when that series itself is to be learned—at least not to a degree sufficient to be detected by this method of experimentation.

If we arrange the averages of each series according to the position it occupied in the day's programme we have the following table:

	Observer S					Observer E				
	TABLE II									
Place in day's programme	1st	2nd	3rd	4th	5th	1st	2nd	3rd	4th	5th
X	3.2		3.		2.5	3.0		2.9		2.8
B	2.8	2.3				3.37	3.			
A		2.9	3.25				3.19	2.6		
G			3.3	2.68				2.43	2.42	
H				3.3	2.75				3.29	2.77
Average	3.0	2.6	3.18	2.99	2.62	3.18	3.09	2.64	2.85	2.80

The figures on the first line, reading across, denote the place in the day's work; the first column gives the name of the series. The partial successes are included in the figures used for this table. These figures show that when series of a certain denomination come early, they almost without exception show better results than when they occupy a later place in the experiment, and there is also a tendency to a general decrease from the first to fifth place as the X series and the averages show. We find a slight exception for *S*, where there is a rise for the third series of the set after a low score for the second series. Subjective conditions probably account for the increase here. What we had therefore in Table I is probably only the result of this general tendency.

Experiment B was next undertaken. The problem was to find whether a side series actually read a single time with full attention would be learned more readily for that fact, if between the reading and the learning, another series were learned. The apparatus and conditions of the experiment were those for experiment *A* with the following changes: Two pieces of black cardboard were made to fit in a groove under the slit in the screen. One was a straight piece which was just long enough to cover the middle and one side syllable, letting the other side syllable show. This could be slipped to the right or the left, exposing the syllables of whichever side was desired. A second piece of cardboard had a square hole cut in the middle, so that when it was slipped into the groove, only the middle one

of the three series appeared. One or the other of these pieces of cardboard was kept before the slit all the time, so that only one syllable ever appeared to the subject at a time, and that a syllable of the series being read or "learned." The cardboards were manipulated by the observer. The procedure was as follows: One series was read a single time by the observer, the shutter was closed by the experimenter and the cardboard changed or shifted so that the series to be "learned" would show; the experimenter lowered the shutter at the end of the one intervening revolution, and the observer then began the reading of the series to be "learned." As before, ten minutes intervened for rest and introspections between the beginning of successive series. The programme in outline is as follows,—
Read *a* once, and wait 1 revolution of the drum

"Learn" *b*, i. e., read *b* 12 times
 " *a* " " *a* " " beginning 10 min. after beginning *b*
 " *g* " " *g* " " " " " " " *a*
 " *h* " " *h* " " " " " " " *g*

Read *i* beginning 10 min. after learning *h*; wait 1 turn of the drum

"Learn" *j*, i. e., read *j* 12 times
 " *i* " " *i* " " beginning 10 min. after beginning *j*.

The *a* series was always written at the left of one strip, the *b* series in the middle, the *g* series in the middle and *h* on the right of a second strip, the *i* and *j* series on the left and right, respectively, of a third strip of paper. No extra series were used. The *g* and *h* series, which were learned without having been previously read were used as a check on the results of the other four series.

The experiment extended from April 14 through April 24, omitting the intervening Sunday, making ten days of experimental work. Twelve repetitions were used for each series except for *S*, for whom the number of repetitions was reduced to ten on the seventh day, because he was frequently getting more than half the syllables right.

The results are given in the following table:

TABLE III

Series	b (P.E.)		a (P.E.)		g (P.E.)		h (P.E.)		j (P.E.)		i (P.E.)	
Obs. S	4.7	.36	4.6	.19	4.5	.26	3.7	.26	3.7	.26	4.7	.24
	4.7	.36	4.3	.27	4.1	.15	3.5	.23	3.3	.23	4.5	.31
E	3.7	.15	3.2	.24	3.3	.30	3.4	.17	3.2	.28	3.4	.32
	3.5	.23	3.0	.23	3.0	.17	3.1	.21	2.8	.28	3.3	.37

The second line of averages, reading across, are those for perfect syllables only.

If a single reading of the series before learning a second series has helped the first series when it was learned ten minutes later, the *a* series will be better than the *b* and the *i* than the *j*. But considering the large P. E. neither series for *E* is helped by its reading, nor the *a* series for *S*. But for the latter the *i* series shows a marked superiority to the *j* series. This is in part explained by the introspective accounts which show that this series was favored by slightly better conditions, as ease of syllables and attention paid while learning the series. The superiority of the *g* over the *h* series is also to be explained in the same way.

It was thought, after obtaining these results that perhaps the *a* series did not show an increased average because the average of the *b* series was kept high by the fact that it was the first series and, for that reason, learned with special energy; also, the *i* series might have been favored above the *j* series because of a renewed impulse to succeed which often comes when one is almost at the end of his task. Therefore, a third variation was tried as follows:

Experiment C. The two indifferent series, *g* and *h*, were made respectively the first and last series learned. The places of the *a* and *b* series on the drum were interchanged, and also those of the *i* and *j* series to avoid any effects which position at the right or left of the paper might have had. The principle of the experiment is, however, precisely that of Experiment B, viz., one series was read, a second learned, and then the series which had been read was learned. The time intervals are those of Experiment B. The scheme in outline is:

Learn <i>g</i>	Read <i>j</i>
Read <i>b</i>	Learn <i>i</i>
Learn <i>a</i>	Learn <i>j</i>
Learn <i>b</i>	Learn <i>h</i>

In order to get as exact information as possible as to the relative ease of the series compared, account was taken of all the "mnemonics" and of slight associative aids. After giving the syllables associated with the "Treffer" syllables, the observer was shown the series again and asked to give with that help any "mnemonics" or other aid he had had in learning the series, and these the experimenter noted down.

The experiment extended from April 28 through May 10, omitting Sunday, May 2, and covered ten experimental days. The results are given in Table IV. The second line of averages for each subject takes account of perfect syllables only.

TABLE IV

Series	g (P.E.)	a (P.E.)	b (P.E.)	i (P.E.)	j (P.E.)	h (P.E.)
Obs. S	4.7 .17	4.5 .22	3.9 .23	3.4 .30	4.2 .33	3.7 .20
	4.2 .14	4.2 .23	3.6 .25	3.1 .32	3.7 .26	3.5 .25
E	4.2 .21	4.3 .24	4.1 .30	2.9 .40	4.0 .24	3.7 .31
	4.1 .21	4.1 .26	3.9 .32	2.6 .42	3.7 .23	3.5 .31

If reading the series were a help, the *b* series would show better results than the *a* series, and the *j* than the *i*, but this is not the case except for the *i* and *j* series when learned by *E*, where, despite the large P. E., the *j* series shows a real superiority. This is due to the fact that on two of the experimental days the *j* series was extraordinarily better than the *i*, and if these two days are left out of consideration the difference is too slight to be of importance. As experiments *B* and *C* are really two divisions of the same experiment, there being no difference in character, we may average the results of the two, which will give the results for twenty days experimentation. Series 1 and 3 are those learned without previous reading, and 2 and 4 those learned with one reading before an intervening series was learned. They are contained in the table following; only the averages including partially correct syllables are given.

TABLE V

Series	1 (P. E.)	2 (P. E.)	3 (P. E.)	4 (P. E.)
Obs. S Av.				
Exper. B	4.7 .36	4.6 .19	3.7 .26	4.7 .24
“ C	4.5 .22	3.9 .23	3.4 .30	4.2 .33
Av.	4.6 .29	4.3 .21	3.6 .28	4.5 .29
Observer E				
Exper. B	3.7 .15	3.2 .24	3.2 .17	3.4 .32
Exper. C	4.3 .24	4.1 .30	2.9 .40	4.0 .24
Av.	4.0 .20	3.7 .27	3.1 .29	3.7 .28

If the extra reading was of advantage, Series 2 should be better than series 1, and 4 than 3. This is indeed the case with series 4 for *S* and to a slighter degree for *E*, but for series 1 and 2 the required relation is exactly reversed. Unless one is willing to infer that the preliminary reading is a hindrance at the beginning of the day's session and a help at its close (for which there seems no obvious reason), one is forced to regard the result of the experiment as negative. Subjective conditions, such as better attention, will perhaps explain the difference.

In conclusion we may say, then, that the results of all our experiments with meaningless syllables were negative. The presence of a series on the side of a series learned did not cause this side series to show more facile learning than the series not so aided, at least to a degree discoverable by our method. Nor did actually reading the series before learning a series give clearly better results for this series when it was learned later.

CONSCIOUS ATTITUDES¹

By HELEN MAUD CLARKE

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The term *Bewusstseinslage*, in the sense of 'conscious attitude,' was introduced into experimental psychology, at Marbe's suggestion (1901), by Mayer and Orth, who employed it to characterise certain conscious phenomena, describable neither as determinate ideas nor as volitions, which appeared in the course of a qualitative study of association. These phenomena are referred to by Marbe himself (1901) as "obvious facts of consciousness, whose contents, nevertheless, either do not permit at all of a detailed characterisation, or at any rate are difficult to characterise;" instances are doubt, difficulty, effort, assent, conviction. Marbe offers, then, no definition of the conscious attitude; he gives only a negative criterion and a list of examples. Messer (1906) adds to the list, and at the same time extends the range of the term, using it to include experiences of logical relation, of the meaning of words and sentences, etc. Bühler (1907) restricts the attitudes to the "mehr zuständlichen Erlebnisstrecken, die als Zweifeln, Besinnen, Abwarten, Erstaunen, usw. beschrieben werden." Marbe seems not to approve of the restriction: "die neuerdings versuchte Einschränkung des Begriffes der Bewusstseinslage entspricht nicht den Ausführungen Marbes," declares

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a recent writer from the Würzburg laboratory; and the implication is that Marbe holds to his original ideas.¹

It is clear that to attempt a definition of 'conscious attitude' at the present time, would be premature. We use the phrase to denote certain large and comprehensive experiences, not evidently imaginal in character; and it is our aim, in this study, to bring these experiences to the test of introspective observation, and thus to discover whether or not they are analysable. Whatever be the outcome of the discussion of 'imageless thought,' it is probable that the name 'conscious attitude' will be retained as a descriptive term; but how the attitudes are to be distinguished, analytically, from 'thoughts' on the one hand, and 'emotions' on the other, must be left for the future to decide.

INCIDENTAL ANALYSES

The conscious attitude is, in general, an elusive experience, which it is difficult to secure in isolation; we therefore resorted in the main to an indirect method, and sought to arouse conscious situations in which various attitudes would be likely to occur. The observers were instructed to give complete introspections. The stimuli in the first series of experiments were letters or words which were written in the blind point-alphabet, and were perceived tactually. At the first sitting the observer was given a slip bearing several letters in their alphabetical order, and was allowed to feel them, and to associate the name to the tactual perception in any way that he chose. Then letters or words were given him to be recognised. The experiments by this method fall into two periods. Those performed during the spring term of 1909 were tentative, and their results were used for the improvement of the method. The stimuli for single experiments were short words or nonsense-combinations of letters. The introspections were written by the observers.

When the work was taken up again in October, the method was somewhat modified. Only single letters, the first ten of the alphabet, were used as stimuli. The whole of a word, perceived tactually, cannot be 'taken in at a glance' as in visual perception. If the letters were mere nonsense-combinations, each letter had to be recognised separately; the experiences thus became complex, and the introspective report might be incomplete. If the letters made a word, the observer tended to interpret from context, which further complicated the problem. In the new series, the reports were dictated, and writ-

¹On conscious attitudes in general, see E. B. Titchener, *Exp. Psychol. of the Thought-processes*, 1909, 98 ff., 270, etc.; E. von Aster, *Zeits. f. Psychol.*, xlix., 1908, 60 ff.; on Marbe's position, M. Beer, *ibid.*, lvi., 1910, 265.

ten down by the experimenter. The most important change in method, however, was the measurement of the reaction-time. The instrument used was a Vernier chronoscope, one key of which had been replaced by a lever arranged just above the letter to be felt. When the observer touched the letter he moved the lever, and thus broke a current and released the pendulum. A finger of the left hand rested throughout on the other reaction key; as soon as the observer recognised the letter, he pressed this key, and released the other pendulum. In these, as in the earlier experiments, the observer was given the letters, in alphabetical order, to feel at the beginning of every hour, unless he declared, of his own accord, that the letters were clear in memory. There was constant instruction to reduce all experience to its lowest terms.

The observers were Dr. Pyle (P), Dr. Okabe (O), Mr. Foster (F), and Miss de Vries (V). The first was at the time assistant in psychology, and the other three were graduate students of considerable experience in introspection. When the experiments were given in their changed form, Dr. Geissler (G), instructor in psychology, took the place of Dr. Pyle. Miss Mary Clarke (MC) and the writer (HC) were observers in some later experiments. MC was untrained in psychology, though advanced in other lines.

1. *Types of Observers*

P is predominantly verbal in type. He reports verbal ideas in sentence form, with a few visual images, sensations of strain, organic sensations and feelings. Thus, CAB. cab. "I perceived the first letter and said C, but had verbal ideas like 'I am not sure whether it is or not.'"

V represents a mixed type. Visual images play a large part in her consciousness, and many of these are colored. Recognition is often mediated by a tactual image on the finger. Verbal ideas, affective processes, kinæsthetic and organic sensations are also numerous. Thus, DC. dc. "Felt the dots and said d, c. Unpleasant. Got visual image at once from the feel, and then said the letter." E. e, 1. 20. "Very pleasant. Sensations from mouth in smiling. The dot fitted into a hole in my finger which corresponded to E. Reaction automatic and hardly conscious. I visualised the round dot and a printed E. Smiled at the similarity of these."

O is also of a mixed type, reporting visual, verbal and auditory images in great numbers. Organic sensations and strain play a great part in his consciousness, as do also affective processes, kinæsthetic and tactual images, pain and temperature sensations. Thus, I. i, 5.12. "Attention well concentra-

ted and body well adjusted. When I touched the lever, shock from muscular contraction. Vacant consciousness, state of waiting. Organic sensation like irritation in back. Stamping of foot, frowning, extreme unpleasantness. Feeling of motion on left finger; probably a real motion, not an image. Suddenly a visual-verbal image I."

F reports verbal, visual and auditory images; more auditory than any other observer except O. Kinæsthetic images and organic and strain sensations are also reported, as well as a few affective processes. Thus, IF. if. "The two letters have different organic sensations. I cannot describe them except by saying that that which is with I is long, and that which is with F is broad; both are narrow, like the letters themselves."

In the introspections of G, all imagery except verbal is almost entirely lacking. Sensations, both kinæsthetic and organic, and affective processes are prominent in consciousness. When G felt the alphabet in order to learn the letters, he described every letter aloud, and associated the verbal description with the tactual perception. G reports that his consciousness in general is almost without images, and his memory of scenes and events is in verbal form. He places things spatially by means of eye-movements, which are unaccompanied by images. Thus, B. b, 4.42. "At the first touch perception of a long row of dots with absolute clearness. It confused me, that is, it excited a complex of organic sensations, and the verbal idea, Why can't I tell what it is? Organic sensations especially from diaphragm. I said, I have to see whether there is a dot below or a changed dot at the end. Then another careful touch of the letter, with the little dot below most prominent. Verbal ideas: Oh, yes, that is B. The whole strongly pleasant."

MC is of a pronounced visual type, but reports some imagery from other senses.¹ Thus, *Symposium*. "Verbal image of the word with sensations in throat. Visual image of a picture of a Greek symposium from a Greek history, followed by image of a table with people seated around it. Image of a convention and auditory image, Round table."

HC is of the same type of imagery as MC. Thus, *Reliability*. "Visual and verbal images of the word. Very vague visual image of some kind of support bearing a heavy weight."

2. Attitudes Analysed

In going over the introspections, to discover what attitudes have been analysed by the observers, we meet with some am-

¹For the method employed with MC and HC, see p. 236 below.

biguity and confusion of terms. So far as attitudes have been named, they have been listed under the name applied by the observer. With this word of caution we proceed to consider the analyses of various attitudes to be found in the reports.

APPROVAL. V. Pleasantness, with some general kinæsthesia.

AWFULNESS. G. Once analysed as a strong unpleasantness and frowning, and again as the same with the addition of inhibition of breathing.

BAFFLED EXPECTATION. O. Visual image of face and frowning forehead. Left foot stamped. Slight burning sensation in back. Bodily and mental attitude was adjusted to a more difficult letter. Partial relaxation and muscular strain in upper part of body. Organic sensation and muscular strain which I could not localise.

CAUTION. V. Verbal idea, Be careful.

COMFORT. V. Organic sensation and smiling.

COMPARISON. HC. The two things were side by side, visually.

CONFIDENCE. O. A good adjustment of muscles, and sensations from them. Agreeable organic sensations. V. "I sat up straight and took deep breaths, and had a sense of stiffness in the spine, and pleasantness."

CONFUSION is analysed by G as "a complex of organic sensations, and verbal idea, Why can't I tell what it is?" "Unpleasantness, organic attitude of inhibited movement of diaphragm and of breathing." F analyses it once as a number of conflicting strains.

O reports the repeated appearance of conflicting verbal ideas: "It may mean B and it may mean C," unpleasantness, and organic sensations in abdomen which seemed to travel upward and to be checked by some obstacle. "Several initial associations of auditory-kinæsthetic images. Muscular strain in head, organic sensation in trunk." V reports holding of breath and blank consciousness.

CONSCIOUSNESS OF FITNESS. O. Relaxation of organic and muscular strain, stretching of back, long breathing.

CONSCIOUSNESS THAT THE LETTER WAS TOO SMALL. O. Muscular strain and organic sensations.

DECISION. O. Slightly agreeable feeling and successive auditory images, It must be so.—Auditory image. Kinæsthetic bodily attitude. Organic sensations.—Bodily attitude and relaxation of muscular strain.—Memory image, visual or tactual or both, of E. Change of organic and muscular sensations. (This attitude is analysed several times by the same observer in the same way.)

DECISION TO DISREGARD THE LEVER. F. Visual and kinæsthetic images of this action. I saw myself doing it.

DETERMINATION. G. Pleasant mood. Verbal idea, I am going to do that well. F. Nodding of head toward finger, and attending to tactual image of coming sensation. V. Verbal ideas, biting the teeth and jerking the head.

DIFFICULTY. G. Trouble to get a verbal idea. Three ideas a, e, and i were ready, but I do not know how.

DISAPPOINTMENT. V. Organic and verbal. I said Oh. My muscles had been strained, but were now relaxed all over. Sensation from frowning. Very unpleasant.

DISGUST. O. Organic sensations throughout the body. Unpleasantness.

DISSATISFACTION. O. Reported twice. Once not analysed. Once reported as consisting of muscular strain, organic sensations, and unpleasantness.

DISTANCE AND DIRECTION. MC. In almost every experiment this observer reports a sense of distance and direction towards a place that is thought of. The two usually occur together, though sometimes the one is reported alone. "Direction goes on in front of my head, and it feels as

if something inside of my head were moving in the given direction. I think this is an image, not a real movement." "Distance is a visual image of the space, boiled down, which lies between the two places. It is a schematic visual image." In one case, in which the observer had been thinking of, picturing, scenes in the east, these were replaced by images of places in her home town, and she says: "In going from one to the other my mind distinctly moved west. This movement is located in the top of the head, and seems like a movement of something inside of the head." In some cases MC says her mind 'flew' to a certain place, and this experience seems to have been a seen motion across a visual area with some kinæsthesia.

EASE OF RECOGNITION. G. Localised once in the trunk, and again as vaguely organic.

EASINESS. (This is partially analysed by O fifteen times, and is mentioned without analysis four times.) Agreeableness, and motor tendency to say Easy.—Motor tendency to say Quite easy. Faint but massive organic sensation in background.—Verbal idea, This is easy, and visual image of my smiling face.—Bodily attitude and relaxed muscular sensation. (In the remaining analyses the same factors are repeated.)

EXPECTATION. Analysis agrees with those of Pyle's observers (this JOURNAL, XX, 1909, 530 ff.).

FEAR THAT I HAD REACTED TOO QUICKLY. F. Slight sinking in the stomach or diaphragm. Lack of clearness of perception, and sensations from breathing.

V reports a state which she calls FRIGHT, and which usually occurs at the beginning of an experiment. This is analysed three times as consisting of a shiver down the back, and a shrinking backward. Once it consisted, in addition, of holding the breath and gasping; and once it is described as a feeling of nervousness and a shiver. The state of SCARE, which might be supposed to resemble fright, is reported only once, and is described as a sudden muscular contraction. C twice reports *fear* as an unsteady organic sensation.

G-CONSCIOUSNESS is reported by G three times. Once it is not analysed; once, said to be motor; and once, to consist of the muscular setting of the tongue.

HAVE FINISHED MY WORK. O. Bodily attitude which cannot be localised. Tendency to straighten up the body.

The I-CONSCIOUSNESS. C. Was a kinæsthetic sensation in the back of the mouth.

I OUGHT TO KNOW THAT. O. Organic sensation and disagreeable feeling.

IMPATIENCE. O. Frowning, and the verbal idea Let's go.

INJUSTICE. V. Gasping for breath. I started back and threw my head back.

IRRITATION. O. Sensation from frowning. Visual image of frowning face. Tendency to lower and shake the head. Hot sensation in head and back. [In the four rather full analyses that follow, the above facts are repeated, and in addition strain in back and neck (three times), disagreeable feeling (three times), organic sensation throughout the body (three times), verbal ideas This is hard (once) are reported.]

IT MUST BE C. G. A pleasant touch-motor complex, with a horizontal movement, and a downward movement at the end. In the same report, this statement was followed by the attitude *I had C before*, which was verbal-kinæsthetic. The articulation was inhibited. This again gave place to the attitude *That is C all right*, which was a touch-motor complex with clear articulation.

MEANING. G. A single dot below the line means a broken T. This is a motor complex. O. Kinæsthetic and auditory image of the letters. Meaning carried by the shape of the figure itself.

The MEANING OF C is reported by G as an attitude, and is analysed as a complex of tongue-sensations and temperature in the mouth. Tendency to say the letter; pleasantness.

NEWNESS. F. "It impresses me as a cutting edge, as a bit of a knife blade turned up. Definite organic sensations. The whole complex I should call newness."

NON-RECOGNITION. F. Includes vague visual image.

The PASSAGE OF TIME. V describes this three times as a perception resembling that of air currents, passing around and over the observer from back to front; in one case there was the verbal idea That was slow. O says that a feeling of length of time came in kinæsthetic terms, and resembled fatigue.

PASTNESS. HC. The image was projected back into the past. Pastness is more visual than anything else. It seems far away, and foreshortened.

POWERLESSNESS. HC. This was a peculiar tingling sensation all over the body, with sensations similar to fatigue, and warmth.

PRIDE. O. Slight tendency to straighten up my neck and smile. Pleasant feeling.

The READINESS TO SAY A CERTAIN WORD is in one case localised by G in the lower part of the head, and again analysed as a disposition or set of the muscles of the mouth.

HC. REASONING that, if Canute was connected with 1026, Alfred was in the 10th century. This was partly represented by a visual schema of the centuries. Canute was in white at 1026, Alfred in dark, therefore in the 10th century.

REFLECTION. O. Thought turned alternately from A to B. (No further analysis.)

RELIEF. G. Pleasure, and disappearance of organic complex. Pronounced exhaling. O. Consists of organic sensation.

SATISFACTION. C connects this with pleasantness. O. Instantaneous pleasant feeling; faint visual image of my smiling face. Organic sensations.—Pleasant feeling, tendency to smile, organic sensations.—Tendency to smile. Verbal ideas: Yes, it is quite sure. (These analyses are repeated a number of times, while there are also cases in which satisfaction is named but not analysed.)

SECURITY. V. Long, easy breathing and straight position of body; relaxation.

STRANGENESS. F. Weak organic feeling all over the body. Strain, and sensations from breathing, and special sensation from diaphragm. This last is not a strain. It must be one of the sensations that we get when we fall in dreams. O. Insufficient adaptation of body, fingers, and hand. General inhibition, which was a kind of organic sensation; inhibition of arm-movement.

TENDENCY TO STAY AND FIND SOMETHING ELSE. O. Consisted of bodily attitude.

Conviction THAT I WAS RIGHT. V. Reappearance of the verbal image of the letter to which I had reacted.

The attitude THAT I SHOULD HAVE PRESSED SOONER, V describes as a twitch in the finger, organic sensation, and catching of breath.

The conviction THAT IT WAS NOT ANY OF THE OTHERS. G. An inhibited motion of the lips to say B, and the fact that I did not move horizontally and therefore it could not be H. This was partly kinæsthetic, in the fingers and mouth.

THAT IT WAS PROBABLY NOT G. V. Verbal ideas, and visual and kinæsthetic images, of the other letters.

THINKING OVER. O. Consisted of bodily set, and of associations which were not perfected. D and J were repeated several times in auditory terms.

UNEASINESS. O. Reported four times. Once identified with uncertainty. Once includes image of frowning face, and sensations in back of head and neck. Once is a verbal idea, That has no meaning. Once is not analysed.

UNFAMILIARITY. G. The touch-motor complex was unfamiliar and unpleasant. It led to movements of tongue and lips in whispering: Oh me, that must be H. Organic attitude in trunk; unsteady shutting of eyes and horizontal movement of eye-balls. V. Rapid mental comparison of feeling of C now with feeling of C last year, though I do not remember how C felt then.

The UPPER ROW WAS TOO SHORT FOR J. O. Tactual memory of J. Bodily attitude.

WAITING. O. This is described twice, in almost exactly the same words. Sensation of heat in back, stamping of feet, and extreme unpleasantness.

WONDERING. V. Once not analysed; once organic attitude, holding the breath and strain. HC. Once merely reported; once described as a complex of sensations in the top of the head.

ATTITUDES NAMED BUT NOT ANALYSED. Besides the attitudes named, there are others which are merely reported and not analysed. They include one case each of identity of self with the lever (G), the motive to distinguish the dots (G), the consciousness that I had reacted (G), the C-consciousness (G), not being right (F), curiosity (O), relation (O), that has no meaning (O), indecision (O), consciousness of an obstacle (O), unknownness (V), fast reaction (V), contrast (MC), sense of past time (HC); and two cases each of certainty (V), wondering (MC), and ownership (MC; this was at least partly visual).

Many of these incidental analyses are, evidently, imperfect. Even at the best, the observers might report in large and sweeping phrases, such as 'bodily attitude' or 'organic sensation.' There is, however, no doubt that the reports were intended at the time to represent the attitudes themselves, and not merely incidental or concomitant occurrences. After the second set of experiments, the observers were confronted with an outline of their reports upon various attitudes, and were asked to say whether, so far as they could remember, the analyses read were, as analyses, correct. The regular answer was that they were correct, and in several cases the observer added, of his own accord, that he could reproduce the attitude, at the moment, and that it corresponded with the analysis given.

While, therefore, we readily admit that introspective analysis might, under more favorable conditions, have been pushed further, we believe that the descriptions given are reliable, and that more detailed work would simply resolve the complexes mentioned into their elements.

3. Gradation of Imagery

Messer finds five stages in the *Entfaltung* of visual images: (1) mere spatial direction or externality; (2) a trace of visual localisation; (3) vague and schematic images; (4) images just named, and not characterised as to degree; (5) very

clear and strong images. In addition, he distinguishes general and particular, partial, changing and moving, and symbolic images. The stages found in the observers in our own experiments are as follows:

P mentions only two stages, and these were only in visual and verbal imagery. The two stages were the vague, and those merely named.

F distinguishes the following stages. For visual images, mere localisation, vague, and just named. For verbal images, which do not lend themselves to exactly the same classification, very vague, just named. Auditory images fall into the same two classes. The idea of movement, which is both visual and kinæsthetic, is very often merely named. Organic sensations were faint, and just named.

V. For visual images: faint, just named, very clear, partial image, change, and movement. For verbal: faint, just named, and very clear.

MC. For visual images: vague, schematic, very clear, partial, change, generic. For verbal: vague, just named, very clear, aloud, partial. For auditory: very vague, just named.

HC's images are in general much like MC's. Visual: localisation vague, just named, very clear, partial, change. Verbal: very clear. Kinæsthetic: vague, just named. Motor: very clear. Auditory: vague, just named.

O has for visual images: localisation, vague, just named, clear, incomplete, movement. He also speaks on several occasions of 'initial associations,' which seem to have been inhibited tendencies toward the articulation of words. For verbal: faint, just named, very clear, aloud, partial. Auditory: same as the verbal, and usually connected with them. Organic: faint, just named, very clear, changing, movement.

G reports for visual imagery only three cases; for kinæsthetic other than verbal, five; and these are all described as vague. His organic sensations are mostly merely named, but a few are faint. On any page of G's reports the verbal stages just named, whispered, and aloud, may be found; yet there is an unmistakable general tendency which, as in the case of V, is in the opposite direction to the order here given. G invariably read the letters aloud when they were being felt for the first time. As they recur again and again, the verbal ideas involved in recognition fade out, from whispered to vaguely verbal, and finally to a mere setting of the mouth or right breathing for the utterance or a certain letter.

Illustrations of the Stages of Imagery.

These typical illustrations of visual imagery represent a large body of similar cases, which cannot be quoted for lack of space. No one of the

observers was instructed to report on the intensity of his images; the experimenter had not this specific problem in mind at the time when the introspections were taken. As the illustrations show, it is extremely difficult to draw a sharp line between the consciousness which is rich in images and that which is 'imageless.' Images may be so vague as to be noticed only by the most careful introspection; and there is every reason to believe that some may escape notice entirely; that, in certain cases, the introspections even of trained observers may not be complete.

MC.

Vague. Vague image of a Massachusetts street. Vague image of a drug store. *Schematic.* Visual schematic image of June in my representation of the months. An image of an environment, a sort of mist, but very vague. *Very Clear.* An image of the girl, including details with much distinctness, such as her light, fluffy hair, bright pleasant eyes, and short dresses.

Partial. Detached image of a face with no setting. Image of a man in the rigging of a ship, but no setting, even of a whole ship. *Change.* Image of the man stepping into the room from the street and looking at himself in the glass. This picture was painted before me. It was not distinct at first, but one part became clear and then another, till the whole was clear.

O.

Localisation. Localisation of the letter in the alphabet slip. (This is repeated many times.) *Vague.* Vague pyramidal image of something. *Just named.* Image of H in point. *Clear.* Clear image of the letters.

Incomplete. Incomplete image of a Japanese key. *Movement.* Image of a worm moving.

V on the recognition of A

The illustrations begin at the point where color is first associated with A. Oct. 22: Visual image of A written in red; Oct. 26: Very brilliant red; Oct. 28: Vivid flash of red; Oct. 30: Visual image of red A in front of me; Nov. 2: A red flash and a little visual written A; Nov. 4: Visual image of A in front of my eyes, and a flash of red; Nov. 6: Faint flash of red; Nov. 6, later: I visualised the two dots; Nov. 9: Slight red blot. After the reaction, a faint visual A. (See p. 234.)

To demonstrate a series of gradual steps from clear images to 'imageless' thought may not prove conclusively that the latter is a fiction; but, at any rate, it suggests an origin and derivation. Imageless thought, as its name implies, is, so far at least, an entirely negative concept. We perform acts of thought in which we can discover no imagery in consciousness, and hence we infer that thought may be 'imageless.' Just what an imageless thought is like, or how it differs from that in which images are present, has never been shown, or even attempted to be shown, except by way of classifications such as that of Bühler. In fact the term 'imageless' is unfortunate; it lends itself easily to misunderstanding. 'Image,' in popular parlance and often in psychology, means a centrally aroused representation of a visual impression. We must, of course, recognise also the place of auditory, tactual, and kinæsthetic imagery, and theoretically of images of taste and smell, though these appear to play but a small part in consciousness. The verbal image, which is especially important

in the thought-process, may itself be visual, auditory, kinæsthetic, or mixed. Even this list, however, does not exhaust the contents of thought. It is becoming increasingly apparent that our conscious states take much of their peculiar quality from the organic sensations, the felt bodily adjustments, which enter into certain situations. The affective processes are also often present, even in thought. It is obvious, therefore, that the term 'imageless' is inadequate to designate a thought from which all these elements are absent. Even the term 'non-sensory,' which is better, disregards the part played by feeling. We all know in a general way, however, what sort of state is meant by 'imageless thought,' and it is just this state which the above introspections tend to discredit.

For they show that imagery does not need to be specific and elaborate in order to carry thought. We may repeat an argument, or review an article, in a very small part of the words used in the original, without sacrificing the essentials of the content. When we are thinking in verbal images, it is by no means necessary to say every word which we should use if we were talking aloud. Again, just because we are not speaking aloud, it is not necessary that every word should be clearly articulated or completed, either in actual throat movements or in images of such movements. The reports of G, especially, show that the mere setting of the mouth, or the right mode of exhalation, serves as well as the complete word. A young child just beginning to read gets no meaning from his words unless he reads them aloud. Thought consists for him in the sound of his own voice or that of another, or in the feeling of the throat movements involved in speaking. When he first learns to read quietly, he whispers; then he confines himself to a mere lip motion; and later, he can dispense with this expression. The reports of our observers take up this progress where the child leaves off, or from an even earlier point, when the words are still spoken aloud, and show that and how the suppression of sound and the shortening of the total process may be carried in the adult to a stage yet more remote from the starting point.¹

DETAILED ANALYSES

1. *Explicit*

The attitudes so far analysed have been those which occurred only a few times. It shall be our task now to consider

¹We are able, owing to limitations of space, to print only the few samples of visual *Entfaltung* given in this section. We have a very considerable amount of introspective material bearing upon the gradations, not only of visual, but also of auditory, kinæsthetic and verbal imagery, and of organic set or adjustment. We hope to present this material in a later article.

some of the more common attitudes in greater detail. One of those which is most often reported is SURPRISE. Surprise, of course, may have various occasions, and whenever these are stated by the observer, they are given in the following quotations.

G. Surprise at the smallness of the letter. Kinæsthetic adjustment, or inhibition of movement, in finger and back.

That I did not feel the letter. Organic sensation, involving inhibited breathing and fixity of the diaphragm.

At not feeling the letter. Something moving along inside the body, a dull pressure going upward from the stomach to the back of the mouth.

Check of horizontal movement and change to vertical.

Peculiar movement of mouth, dropping of jaw, and slight sensation in chest.

Inhibition in diaphragm and setting of the mouth to say Oh. [This last is repeated several times in almost exactly the same words. In two cases the attitude is not analysed at all, and in others it is merely localised.]

O. Muscular strain and visual image of a working nerve. Unpleasant feeling.

Change of position. Organic and muscular sensations somewhere, and blank consciousness.

Relaxation of muscular strain, and a tendency to change the bodily attitude.

V. Holding of breath and kinæsthetic sensation at finger tips.

Running of warm currents all over the body, and tingling sensation. Very pleasant.

Verbal exclamation.

Kinæsthetic and verbal. I dropped my jaw, and opened my mouth. Sudden muscular contraction, a gasp, and turning the head.

Muscular contraction.

Held my breath and gasped and contracted muscles.

On comparing these reports, we find that surprise is, in general, an inhibition of movement, or change in its direction, often accompanied by sensations from inhibited breathing, organic sensations, verbal exclamations, and affective processes. The important thing seems to be the change or inhibition of muscular reaction. Surprise may be called the conscious aspect of the adjustment to a new and sudden event in the environment.

The consciousness of SEEKING for something, of trying to remember, is called by Marbe a *Bewusstseinslage*, and is classed by Messer as an affective attitude, in which the relation of the *Aufgabe* to the object recedes, leaving a subjective state for whose solution the *Aufgabe* suffices. Watt affirms that the consciousness of seeking for the reaction-word is not present in every experiment; when present, it consists of a consciousness of direction, and an emptiness of consciousness, with a repetition of the *Aufgabe*, and sometimes a visual image. Orth says that the attitude of striving to find is a complex of organic sensations, bound up with the representation or immediate consciousness of a goal. Messer assumes an at-

titude of seeking, which is unanalysable, but finds with this in the whole situation motor processes, movements of the head and eyes or representations of such movements, organic sensations, strain and obstructed breathing.. All these components may not be present in a given case. In his "Snap Shot of a Hunt for a Lost Name," Bailey finds three stages, which include visual images, kinæsthetic and organic sensations, and pleasure, beside several complexes such as belief.¹

As the experiments on point-letters were not suited to bring out this attitude, special introspections were taken. The observers were MC and HC. Some of the analyses occur in experiments given for another purpose, and in a few cases the occasion for trying to recall arose incidentally, and the descriptions were written down immediately. Since these cases did not promise to be very numerous, however, a special method was devised. The observer was shown an old and almost forgotten picture of her classmates in the High School. All of the picture was covered except one face, and the observer was asked to recall who the person was. Whenever the response was immediate and no seeking was required, the case was thrown out, and another face exposed.

MC. Visual image of the face, and verbal idea of the name X. In trying to think of the right name, I experienced a kind of working or agitation localised in the top and back of the head. Several names came to consciousness, and visual images of persons that I associate with this girl.

Visual image of the picture and also of the girl. The first glance brought a verbal image of a name XY, followed by images of XY in two situations. I have also a feeling of time, that is that XY was much farther back in time than the subject of the picture. I do not know what this is. It seems to be chiefly a series of instantaneous pictures.—

In other cases, events and scenes which might suggest the name are recalled visually, laughter and voices are heard in image, and even the peculiar carriage and gesture of the person are imaged by the observer. In most of the cases recorded the attempt to recall was unsuccessful. The reports contain also frequent reference to sensations from squinting and closing the eyes, sensations of 'drawing' in the top of the head, 'emptiness in the head caused by the inability to recall the name.' Again: "The brain working seems in this case to be in the front of the head instead of the back as before. Sensation in the eyes and vaguely in stomach. Feeling of unpleasantness caused by inability to remember."

The sense of seeking for something occurs about thirty times in the same observer's later introspections upon the meaning of words, and is invariably described as a groping feeling in the top of the head, with sometimes a knitting of the brows, wrinkling of the forehead, squinting of the eyes, and sometimes a visual experience which is described as staring into a blank field. Occasionally the object of search finally appears as a small object in the middle of this field. Consciousness seems less rich in content in these latter reports than in the earlier.

The introspections of HC were obtained under exactly the same conditions as those already discussed. The common element in them all is sensation from the diaphragm, though that from head and eyes is often

¹*Journal of Philosophy, etc., iv., 1907, 337.*

present. In the cases of seeking which occurred incidentally, the strain is very often in the eyes and forehead. Visual images abound, while kinæsthetic and auditory imagery is sometimes reported.

It is evident that the consciousness of seeking consists of strain in the head and eyes or internal organs, and a feeling of effort localised in the head, together with images of any kind which have any connection with the required fact and would be likely to recall it.

The attitude which occurs most often incidentally is DOUBT. No sharp line can be drawn, it seems, between doubt, uncertainty, and hesitation. The term used by the observer has, however, been preserved in the quotations, so that, if there is any inaccuracy in this interchangeable use of terms, it affects the deductions alone, and not the results from which they are drawn. It is certain, at all events, that in many cases the observers themselves did not distinguish the three attitudes, and that the same kind of analysis is given under all three names.

Doubt is one of the attitudes most often named by Marbe, Orth, and others; and in Ach's separation of *Bewusstseinslage* from *Bewusstheit*, it is included in the former class. It is assumed in general, all through this series of papers, that doubt cannot be analysed. Thus, Orth finds doubt accompanied by sensations in the head, organic and kinæsthetic sensations, images and feelings. "Doubt is not a feeling, but a complex state, whose constitutive element is the *Bewusstseinslage* peculiar to it." Here is really an analysis of doubt, *plus* the assumption that the analysis is incomplete, and that there is a central thread running through the experiences, which is indescribable except by the phrase that it always characterises doubt. Whether or not there is such an element is a question for introspection to decide.

The following accounts are taken from the introspections of G. *Uncertainty*. Slight organic excitement in the abdominal region. Verbal ideas: Well, what is that? The movement changed from the B to the C movement, which was actually repeated. Long dwelling on one impression. The disturbing other dot was always there. There was nothing pleasant after the reaction, because I was not sure.—Suspension of verbal association, or inhibition of movement in the mouth. The mouth did not set itself till the recognition was complete.—Verbal idea: What the deuce is that? The letter seemed too short; inhibited breathing, and pressure in larynx.—*Hesitation*. Hesitation between B and J. I set my mouth for B, then for nothing at all. Renewed touch-motor complex.—Familiar, then unfamiliar aspect. Kept finger on letter and pressed harder on last dot.

F. I was not certain. It did not feel quite right. I compared it with the tactual images. The perception had a blank place in it which did not belong to H, and the real H was in this space.—It was not a clear cut G and did not come forcibly.—*Doubt*. A came, but it was not exactly as it ought to be; E came, but I could not decide which it was. Strain from hesitation. I was not sure. Kinæsthetic sensation high up in arm. Not being sure was not anything in consciousness; I just did not react.—*Hesitation*. It

was not clear, and I kept feeling it. It was not H because not bulky enough; I said J, but it did not seem to be J because too big. I said C, but it was too big for C. Visual image of a triangle. J came, but it still seemed too big.—Kinæsthetic image in forearm and finger. Tactual sensation not very clear. I did not react.

It will be noticed that the element common to almost all of these cases is one which may be called negative: the failure to move on, and to complete the reaction. When there is no doubt, the process proceeds without interruption; but in doubt there is "long dwelling on one impression," "the finger is kept on the letter," there is "renewed touch-motor complex." In addition to this, there is, especially in hesitation, a vacillation between two tendencies; two aspects alternate in consciousness, or the mouth is set to say now one letter and now another, without saying either. Verbal ideas and organic sensations may be present. Doubt is connected with a lack of clearness of the tactual perception. As the reaction is delayed, the attempt to recognise is repeated, and the reaction is prepared for again and again.

O. *Uncertainty*. Swift succession of auditory-kinæsthetic verbal images, initial movements of tongue, and faint sounds. Queer sensation from bodily and facial attitude. Visual image of my face full of confusion and unpleasant feeling.—*Doubt*. Several initial associations, consisting of sensations from irregularly contracted muscles, and a visual image of them; unpleasant feeling, organic sensation above abdomen, and sensation from irregular breathing. Interruption of breathing, organic sensation in chest, and pain in back of neck. Organic sensation in back and spine.—The same, with verbal images.—Organic sensation in head, chest and abdomen, unpleasant excitement, faint visual image of my vexed face, organic sensation in both shoulders, muscular strain in neck.—Auditory-kinæsthetic images of A, and something unclear tried to come into consciousness but was inhibited. Organic and muscular strain in head and chest. (This is practically repeated several times.)—Initial tendency to produce B, C, or D. Faint verbal and auditory image of J, then "it can not be," with organic sensation. (This last is often repeated, sometimes with unpleasant feeling.)—Unsteady and recurring organic sensations, which continued a long time.—Organic sensation like heat in the brain, holding of breath, muscular sensation and pain. Absence of relief, the muscular strain persisted. Verbal idea "I might be mistaken." (Almost every introspection given here is reported a number of times. There are three unanalysed cases of doubt.)—*Hesitation*. Verbal image of D and C alternately. Verbal idea that the dots are very strange, disagreeable feeling, sensation in back. (This is essentially repeated several times.)—Tendency to stay and find something else. (Hesitation is once unanalysed.)

O's consciousness is made up very largely of organic sensations and strains, and shows much more affective coloring than those of G and F. Doubt and uncertainty were unpleasant and irritating.

P gives introspective accounts of the attitudes of doubt, uncertainty and hesitation; the analyses agree almost completely in being verbal. P's consciousness is so strongly verbal that whatever else was present seems, in most cases,

to have been so far in the background as to be overlooked. There are a few references to organic and kinæsthetic sensations, unpleasantness, holding of breath, and general strain. The fingers were sometimes moved over the letter again and again.

V's analyses of uncertainty are meagre.

I frowned and held my breath.—Unpleasant and annoying.—I could not tell the number of dots. I tried to count them separately, but finally had to rely on the image I got from the whole sensation.—*Doubt*. The dot seemed flat and did not fit well into the finger.—The letter did not fit. I held my breath and muscles stiffened. Muscular strain and verbal idea, Perhaps it was D.—Frowning, vague organic complex, and verbal idea: I am not sure. (These are all repeated several times. For V the doubt in these recognition experiments usually came after the reaction, so that it could not consist in the inhibition of reaction. V's recognition, whether right or wrong, was usually immediate, and the reaction time was very short. In the few cases in which doubt occurred before the reaction, the time is increased above the average. As the feeling of fitting on the finger is the most common element in V's recognition, so the absence of this fitting most often marks doubt, but it is very often accompanied by an inhibition of breathing, muscular strains, and verbal ideas of doubt.)

Doubt does not occur in MC's reports, and occurs only three times in HC's. It is here analysed as a feeling of displeasure, organic sensations from stomach and diaphragm, and alternation of attention from one visual image to another.

Although the observers differ strongly in type, it is easy to see that there is a general agreement in the analyses of doubt. It involves an inhibition of the reaction, a checking of the habitually smooth-running process. So far, it might seem that doubt and surprise are alike; but there is a difference. Surprise involves a sudden checking or changing of the motion begun, whatever it is. Doubt tends to check the reaction, if it has not already taken place,—to lengthen the reaction time. The motion involved in feeling the letter, however, is not checked, but is prolonged or repeated. The observers of verbal type have verbal ideas of doubt; others have organic and strain sensations, and often unpleasantness. There is a marked tendency to use the term *hesitation* when there is a vacillation from one word, image or form of reaction to another. The organism meets a situation to which it is not prepared to react promptly. This may be because a particular reaction is begun, and then inhibited, or because several are initiated in swift succession, and inhibit one other. We may recall Washburn's derivation of the feeling of "but." The incipient movements of developed consciousness, in which the mouth sets itself to say now one word, now another, or the hand to make this or that reaction, may be vestiges of larger movements following one another in swift succession and tending to carry the body in opposite directions or to set it for incompatible acts.

2. *Genetic*

The fact of development has already been recognised by the writers on attitudes, though its whole bearing upon the question has apparently not been seen. This statement holds in particular of the case of the *Aufgabe* or task. Marbe reports a *Bewusstseinslage* which the observer called 'memory that it must be answered in a sentence,' and again one that the observer called 'recollection of the problem.' The first of these is referred to, with approval, by Orth, and is placed by him in the second group of his classification. If we turn to Watt's work, in which the course of the experiment is divided into four parts, we find that the *Aufgabe* is not only clearly recognised, but is also analysed and genetically developed. Watt is specific upon the point that the task (or part of it) gradually drops from consciousness as the series proceeds, but that the degree of consciousness of the preparation has no relation to its effectiveness. Messer is, in this matter, in entire agreement with Watt and Ach.

The course of our own experiments was at no time divided into parts, and in general little attention was paid to the consciousness of the task. Sometimes, however, voluntary reports were made, and at other times the observer was asked, at the time of introspection, if the task had been present before the reaction. The instructions were not repeated at the beginning of every hour. At the time, this fact did not seem worthy of notice; but we believe that they were never specifically repeated, to any observer, after the first hour of work. The instruction to recognise a letter is so simple as not to need repetition. The letters in alphabetical order were usually given to the observer at the beginning of the hour; but sometimes he reported, as we have remarked, that he did not need them again. This reminder may have served as a renewal of the general consciousness belonging to the task. Again, the general position of the body must be resumed at every sitting; that is, one finger must be on the reaction key, another at the edge of the card holder, the eyes shut. In so far as the *Aufgabe* is carried by bodily position, it must always have been present.

The TASK is specifically reported in a few instances.

G. Verbal ideas: I must make up my mind to react, and an uncomfortable bodily attitude, very unpleasant. (This was not reported as a part of the fore-period. The recognition and reaction seem to have been delayed, and the task is recalled in order to hasten reaction. This occurs in the second experiment of an hour, the ten letters having been felt at the beginning.) In a second case, the task is analysed as the organic setting in saying: Hurry up, don't be so long. (This too is a case of delayed reaction, and is the seventh of a sitting, the letters having been felt at first.) V reports the task to press the button as being present in kinæsthetic terms. This

is in the fourth experiment of the first hour of the second series. The recall to consciousness does not seem to have been occasioned by any difficulty. In the third reaction of the next hour V "forgot the *Aufgabe* to react," and therefore did not react as soon as she recognised the letter. The whole present purpose was to recognise the letter, and this purpose was a muscular adjustment, and concentration of attention on the finger tip. In the second experiment of the next hour the task was present in motor terms, and especially in a strain sensation in the finger, which relaxed after the reaction. F reports what he calls the "hurry-up" consciousness, and again the *Bewusstseinslage* that I must do it quickly. The former is analysed thus: Strain became noticeable in the abdomen, as if two sensations came from the sides and met. The "hurry-up consciousness" is mostly strain. Again: Organic sensations from diaphragm. The muscles of the diaphragm seem to come up and press the lungs, and the muscles of the ribs seem to tighten. (This always comes with the strain of waiting.) Again: Strain when I heard some one walking. The strain meant "hurry up." (This is often repeated in the same terms. The "*Aufgabe* to do it quickly" is described twice as consisting of muscular strain.)

Nearly all these cases occur early in the second series, and the *Aufgabe* is not later reported by name. It does not follow, of course, that it was not in consciousness at any other time, or that it was not reported in its elements. The conscious contents occurring before the perception of the stimulus were often complex and important, and in some cases occupied much more space than the rest of the introspection.

In the early part of the series O reports sensations from the different parts of the body, as the bending of the neck, stamping of the feet on the floor, pressing together of the teeth, and auditory images of the sound made by the finger in moving over the paper. About the middle of the series he several times remarks: "I did not notice the bodily adjustment of attention," and in the last thirty or more experiments the introspection usually begins with the perception of the letter. If there is anything before this, it is usually the auditory sensation from the rubbing of the fingers over the paper. The strained condition of the body is mentioned only once in the last thirty cases. In the reports of F, also, we notice a marked falling-off in the number of conscious contents occurring before the appearance of the stimulus. The first introspection is almost entirely an account of the fore-period, and includes strain, breathing sensations, kinæsthetic, temperature, verbal and visual images, some of these occurring several times. In the experiments immediately following, the contents of this waiting period are only slightly decreased. Later the observer reports: Kinæsthetic and visual images of moving fast over the letter. Later, again: The 'ready' set me off without a conscious *Aufgabe*. The idea of movement with the tactual image is repeated a great many times, but toward the last is described as vague, and does not appear at all in some of the latest observations of the series.

The conclusions to be gathered from these data are in entire agreement with the findings of Watt, Ach, and Messer, who offer us not only avowed analyses of the *Aufgabe*-consciousness at the beginning of a series, but also repeated proof that it is shortened and modified and tends entirely to disappear. If any observer were stopped just before a reaction, and asked to state in words what he was about to do, he would doubtless be able to reply. We may reason, logically, that he could not state what he did not know, and that here is therefore a case of 'imageless' knowledge. It

may be answered: (1) that such a statement would be a clear case of *Kundgabe*, a report of the significance of a total state and not a description of contents; (2) that, even if such a report be conceded to be the verbal expression of an 'imageless' state, it is nevertheless shown to have derived by direct development from a consciousness whose contents could easily be isolated; and (3) that, so far from any indication that the *Aufgabe* was present in other than sensory terms, it is specifically said not to have been consciously there at all, and yet to have been active. The reports throw us back upon unconscious tendencies, but not upon an unanalysable attitude or an imageless thought.

The attitude which in our experiments shows best from a developmental point of view is RECOGNITION. From the nature of the point-letter experiments, it follows that every introspection is really an analysis of the recognitive consciousness. It may not be complete, and it may on the other hand contain experiences that were incidental, but in the main it represents recognition. The letters were absolutely new to the observers when the experiments were begun, and when they were ended most of the letters were recognised both accurately and promptly. Our conclusions will, however, be based chiefly upon the second series, in which the time was measured. To part of the observers the letters at the beginning of this series were not entirely unfamiliar.

It is to be supposed *a priori* that, as the experiments proceed and a letter is given again and again, it becomes more and more familiar. This inference is supported in a general way by the curves of accuracy and of time, which show that on the whole all the observers make fewer mistakes toward the end of the series than toward the beginning, and that the time required for reaction becomes progressively shorter. Sometimes, it is true, cases of false recognition or of long reaction-time occur at the end of a series. These can usually be explained, however, as due to accidental causes. For example, the raised letters tended to become worn off, and to grow less distinct with repeated rubbing. Care was taken that they should be replaced before this injury occurred, but sometimes it escaped notice. The introspections enable us to trace most of the irregularities to these and other causes. Even without this allowance, however, the curves show a striking similarity of form, and, with one exception, an invariable tendency to decrease both of mistakes and of reaction-time as the series advances. Curves of the length of the introspections, in lines or words, would show the same general slant from left to right. Of course, such a result could have only the most general application, but the differ-

ence in length of introspections is striking, and, taken with the other indications of shortening, has a certain significance. At the beginning of any series there are usually one or one and a half introspections to a type-written page, while at the end they average from five to ten, according to the observer.

The letters chosen, which were the first ten of the alphabet, turned out to vary greatly in degree of difficulty. E, being a single point, was the easiest of all. F, I, and A were also easy, while G, D, H, and J were difficult, and B and C stood midway. The intermingling of these difficult and easy letters brings irregularities into the times, and makes it necessary to study the letters separately.

The letter which gave O the most trouble was D. The average time for D was longer than that for any of the other letters, and the letter was usually not correctly recognised. In fact, it was called D only once, the last time that it was given. The reaction-times are very irregular, increasing gradually from the beginning, suddenly dropping about the middle of the series, and then again increasing. At first D is confused with G, but later consistently with J. The number of elementary experiences reported varies roughly with the reaction-time. When D first occurs (D. j, 4.42), the introspection includes perception of shape, organic sensation, disagreeable feeling, and images. In the next case (D. g, 8.40) the image of the reaction-letter comes verbally as well as auditorily, and there is doubt. Otherwise the report is essentially the same. In the third case (D. j, 11.62), consciousness is still more complex, and includes repeated perception, more complex verbal images, and several visual images, of which some are merely associative. In the fourth (D. j, 10.24), there are added frowning, irritation, organic sensations throughout the body. "Very disagreeable. So many sensations and images that I cannot remember them all." In the next, there is a marked decrease. In the next three or four, there is increase again; and in the final case (D. d, 15.42), the observer says: "Auditory image J, or D. I thought it over again and again. This was bodily setting, and associations which were not perfected. The whole was accompanied by strains and organic sensations, verbal and visual images, and unpleasantness, especially at the last." Evidently the observer himself was not satisfied, and the reaction to D was not a true recognition.

This series will serve as an example of the correspondence between amount of content and reaction time. It is evident that we have not here before us a growth of the recognitive consciousness. As the letters in alphabetical order were given to the observer at the beginning of the hour, he could not form a closer and closer association between the letter D and the name J, which the repeated wrong reactions might otherwise have caused. So we find irregularities, with a tendency toward increase rather than decrease. The result shows, so far as one case may do, that the shortening to be observed with increasing ease of recognition is not to be attributed to a general habituation to this form of experiment. The change due to habituation can be observed, but it occurs mostly in the fore-period, and it is not sufficient to account for the progressively shorter reactions to particular stimuli.

The letter A, which for most observers was one of the easiest, was for O next in difficulty to J. It shows, however, a different change in consciousness. A is always recognised correctly except in one place, in spite of its difficulty, and consciousness shows a general though very irregular prog-

ress from complexity to comparative simplicity. In the beginning (A. a, 14.78) O reports difficulty of perception, organic sensations in abdomen, spine and back, disagreeable feeling, verbal ideas, auditory-motor images, and doubt, which last was a tactual memory image of the former size of the dot and a comparison of this with the sensation. In an introspection chosen from the latter part of the series, there are only tactual perception, visual image of dots, verbal idea A, and surprise. In this case the letter was rightly recognised, and the time was 2.78 sec. F and E show the shortest times, but they are not very regular. Practice was not carried far enough with O to reach automatic and prompt recognition. With every letter except D there is a change toward simplicity of consciousness and shortening of reaction time.

The letter C was one of the easiest for the observer F. The introspections may be compared with the time curve. C. c, 3.20. The reaction was unnecessarily long, I forgot to react. Tactual images as I moved my finger, especially an image of touching the lever. Sensation of strain in shoulders and chest, slight interruption of breathing. Attention all on tactual images. As I was moving across I had the verbal idea: Tactual images. C is a movement across and down. It is kinæsthetic and tactual. C came up verbally and auditorily. Saying C recalled the *Aufgabe* to react, in what form I do not know. C. d, 3.30. Visual and kinæsthetic image of movement. I said 'as before' while I was moving across. Tactual image of a point. Touched the letter, and I came up verbally. I knew that it was not clear enough; it did not fit into what I know I is. The whole complex was not just right to produce reaction. I felt twice more and D came up. The reaction still seems just like saying the letter. 'As before' meant going over it without attending to the lever. C. c, 2.16. Idea of movement, partly visual and involving eye movement. D came up at first; then I felt the rest of the letter, and C came up verbally and auditorily. The breathing was right to say C. I was trying to catch the verbal image and noticed the breathing. C.—1.96. Strain when I heard some one walking. This strain meant 'hurry-up.' I touched the letter and it was not clear at first. I felt it again. It was familiar, but I could not think what it was. C. c, 2.10. I felt two dots above. Attention on upper dots, then vague perception of lower one, and I moved down to feel it. This motion named the letter. C. c, 0.96. Tactual and kinæsthetic image of feeling the letter. The location of the letter in advance is kinæsthetic and tactual. C was visualised as a dark mark. C.—1.16. I did not recognise the letter. It was very clear. I reacted to the tactual sensation. C. c, 0.62. I set myself muscularly, touched the letter, and moved part way over it. F came up auditorily. I moved the rest of the way and C came up. Reaction and relaxation. C. c, 0.44. Vague tactual image and muscular set. Clear tactual and kinæsthetic sensation. There is an image of the kinæsthetic sensation which enters in and makes a part of the C. When I move my finger straight across, it seems as if I had moved it down. This is an image. Auditory image C after reaction. C. c, 0.36. Idea of movement. I felt it, and the letter C came up.

The reaction times of V are uniformly short, but show a very regular decrease. There is a striking similarity among the curves, some of them being scarcely distinguishable. Early in the series there are mistakes, and some entire failures to recognise the letter. In far the larger number of cases, however, the recognition is unerring and the reaction times are short. It has already been shown (p. 223) that the color images, which accompany certain letters for V, gradually fade out. In the introspections, of which we give but the briefest sample, there is a corresponding disappearance of other conscious contents.

In the case of H, V reacted to a vague mass. The voluminousness seemed to be the one thing recognised; and she recognised the letter in a fraction of a second just from its voluminousness, long before the shape

began to be definite. J was associated verbally at the beginning with the strange thing which did not fit, but this letter too came later to have more definite outlines.

As G had done no experiments in the first series, the introspections here given represent the whole course of improvement from the first presentation of a letter to its almost automatic recognition. I. i, 1.86. At first a touch complex which was focal. Repeated movement in the vertical direction, which led at once to recognition and verbal idea I; movement and verbal idea focal, organic complex vague. Feeling of recognition pleasant. I. i, 2.82. Horizontal movement with complex touch led to no recognition but to vertical movement over the dots. Visual image of I and very pleasant mood, localised in trunk. This might be called ease of recognition. I. i, 2.60. Slight surprise, that is a check of the horizontal movement, and a change into the vertical, with peculiar movement of the mouth, a dropping of the lower jaw, and slight sensation in chest. The setting of the mouth led involuntarily to saying I aloud, and reaction. Last slightly pleasant. I. i, 1.40. I is recognised by the checking of the horizontal movement and the substitution of the vertical. Setting of the mouth to say I. Reaction automatic. I. i, 1.14. Inhibited horizontal movement. Strong setting of the mouth to say I, which lasted after the reaction and led to saying I. Slightly pleasant. I. i, 2.40. Slight setting of the mouth to say I. Indifferent or slightly pleasant. I. i, 1.16. Different from the usual I-consciousness. Motion not inhibited. Vertical motion replaced by tactual perception of two dots, one above the other. Slight pressing down of the lower part of the mouth, vaguely localised. I. i, 1.60. Movement horizontal, then vertical. Organic and affective inhibition of horizontal movement. As soon as this vertical movement was established, slight pressure of the larynx in setting of the mouth to say I. I. i, 2.22. Touch-motor consciousness, with change from horizontal to vertical very clear. Vague pressure in back of mouth. This was the I-consciousness. Slightly pleasant. I. i, 1.36. Stereotyped I-consciousness. I. i, 1.36. Stereotyped I-consciousness. Kinæsthetic part in back of mouth more pronounced than usual. Auditory image: I again. I. i, 1.60. Surprise. This was inhibited breathing and setting of the larynx and back of mouth. Later the common I-consciousness.

There can be no doubt that the introspections given, and the far larger body from which they are taken, represent recognition in the making. In the whole number of reports there are at most only one or two cases of the comparison of a percept with a memory image. There is not a single allusion to a quality of knownness attaching to the perception. The feeling of familiarity is reported thirty-five times, but in all but four it is analysed. The introspections of any observer show, in general, a dropping out of conscious contents, and a shortening of reaction-time going parallel with increased ease of recognition. Only one case was noted in which the process was reversed; the reaction-times here become longer instead of shorter, the conscious contents increase in complexity, and the letter is continually mistaken for another.

The cases of reported familiarity almost all occur in the first half of a series. Their analyses show that they are complex states, consisting mostly of strain and organic sensations, with some affective processes and accidental associations.

Among the final experiments of most series, where we may assume that complete recognition occurs, we find cases in which the reaction is said to be automatic, and the letter may be represented in consciousness by a setting of the mouth to say it, a visual image, a flash of color, or by nothing at all.

If the terms 'familiarity' and 'recognition' are used interchangeably by writers on the subject, it may well be that they were also confused by our observers. We should not lay too great stress upon the names applied to the attitude. With all allowance made for inaccuracy, however, there are suggestions here for the arrangement of familiarity and recognition along a scale of continuously varying complexity.¹

If recognition has received considerable attention from psychologists, so also has UNDERSTANDING. In fact, the two approach each other so closely in experience that it is impossible sharply to distinguish them. Our own experiments combined the methods of Ribot, Binet and Taylor. The stimuli were words, sentences, and paragraphs cut from magazines, which were read sometimes to the observer, sometimes by her. The stimuli were varied in two directions, from simplicity to complexity by means of the length of the passage to be read, and from sense to nonsense, some of the stimuli being strange or impossible words. If there is a special consciousness of understanding, it ought to stand out, by contrast, in a series in which part of the stimuli are not understood. The experiments were performed during the summer of 1909 with the observers MC and HC. The latter wrote introspections on only 100 single words; MC was given the same number of words and, in addition, 50 sentences and 20 paragraphs. The general procedure was that the observer drew a slip from an envelope, read the stimulus, and wrote the introspection. When the report was dictated, or the stimulus read by the experimenter, the fact is noted. The words used include nouns, both abstract and concrete, and various other parts of speech. In almost every case MC reports verbal and visual images of the word as the first thing in consciousness; and sometimes, if the word is not very familiar or is one whose meaning does not easily appear in visual images, it is repeated several times.

MC. It was invariably the case that, if the word was unknown to the observer, she immediately associated it verbally to some known word of similar sound, and the other contents of consciousness referred to the meaning of the known word. Sometimes only a part of a word was con-

¹Lack of space, again, forbids a discussion, in this and the following sections, of the views and results of earlier investigators. General reference may be made to Titchener, *Exp. Psychol. of the Thought-processes*. We hope to recur to the theory of recognition, in particular, in a later article,

cerned in this association. *Metappos.* Visual image of Italy and Switzerland on the map, accompanied by sense of direction. Verbal image of the words Matterhorn and Metaphysics. *Frostilla.* Visual image of a frosted cake, then of frost on a window pane. I do not know the meaning of the word, but think it may apply to some kind of extract.

Even when the word was understood, it was sometimes associated to others of similar sound, or divided into parts and then connected with images which referred to but a single part. This verbal association occurred most often when the word was a pronoun or preposition whose meaning was hard to grasp apart from context. *Display.* Visual image of the two syllables separately, and of a stage at the theatre. Later visual image of a woman overdressed, not very distinct. *Which.* Verbal image Witch, with visual image of a volume of Scott and of Meg Merrilies. Then repeated verbal image of the stimulus word, with slight groping in mind, followed by image of a page in a grammar.

By far the greater number of meanings were represented by visual images, and these were most likely to occur with words denoting concrete objects, though they were not confined to them. *Compartment.* Image of a train, and of the words 'train' and 'European.' Sense of direction and distance. Visual image of some pigeon-holes.

The contents of consciousness were usually far poorer in the understanding of such words as prepositions, which have little meaning apart from context. *From.* Repeated visual and verbal images of the word. Sense of distance and direction in going from one place to another. Visual image of a country road between two familiar places. *The.* Visual image of the word, then again in big letters at the top of a newspaper; no particular one. Then a blank. I shut my eyes and looked into a blank field. Consciousness of 'seeking,' but nothing came.

Sometimes the setting which the word called up was verbal rather than visual, and sometimes the verbal ideas constituted a definition. *Adjacent.* Word at first seen sidewise and not recognised. The J was most conspicuous, and this came as a visual image, followed by verbal image; Dr. Jekyll and Mr. Hyde. Then visual and verbal image of the word, the latter many times repeated, and a sense of groping and looking into a blank field, though less blank than usual. It seems at times to be dotted with conspicuous black letters such as made up the stimulus word. Later verbal idea: Things close to each other. *Marvelous.* Visual image of a house occupied by Prof. M (name similar in sound to stimulus). Later verbal image: Wonderful. *Rose.* Visual image of a rosebush, and of two girls named Ross. Verbal idea: Roman de la Rose. Images were unusually numerous here, and I cannot recall the others. They seemed to be present all at once.

As might be expected, there are cases reported in which the word was understood, but the observer could not analyse the understanding. There are, however, only seven or eight of these among the hundred words, and in most of the exceptional cases there is some indication that sensations or images were present. *Right.* An immediate consciousness of the idea 'to do right.' "This was partly verbal, but I seemed also to see something of a schematic nature. I looked downward to see this." (Whatever the observer looked down to see must have been of a visual nature.) *Individuals.* Image of a light place (probably an after-image) with the word in it. It all seemed to be inside my head, then I seemed to try to get it out. Paralysed feeling. I knew what the word meant, but it did not suggest anything. Then visual image of individual people walking. They bowed. Knowing what it meant was a kind of comfortable feeling, a feeling that I could define the word. This was in the back of the head and throat. (The feeling that she could define the word, localised in the back of the throat, was probably the setting of the vocal cords to articulate the definition.)

These introspections, like those of Binet, Ribot, and Bagley, show cases in which the images were inadequate or contradictory to thought. *Noiseless*. Visual image of a house, of a very noisy family of boys who occupy it, also an auditory image of the noise of those children. This was accompanied by a sense of contrast, but I am too tired to analyse this. *Electric*. Visual image of a park that I once visited which, however, was not electric. There seemed to be in the background images of several electric parks but the other was more distinct.

In the next series, of 50 experiments, the stimuli were sentences of various lengths. Here, too, the meaning is most often represented by visual images, if the sentence describes a visible object or a scene. In such cases the whole scene may be painted before the observer's eyes, a part at a time, as the words come. *The engineer and fireman and one or two others were standing by the engine staring at it; and so they hastened thither, well ahead of the outpour of people behind them.* (Read by E.) The series of visual images came during the reading. Verbal images of first two or three words. Visual images of engineer, fireman, engine and crowd. *He climbed up the straight iron steps to the gangway.* Visual image of iron filigree work. Verbal image of whole sentence. Image of a person going up a gangplank in the Brooklyn Navy Yards. Second verbal repetition of the whole sentence.

For this observer all sentences, even of an abstract nature, arouse visual images which represent, more or less schematically, the meaning of the whole or of single words. Sentences which are not descriptive may nevertheless be represented visually. *Every vigorous state pursues two principal aims: to enlarge its dominions and to preserve its independence.* Visual and verbal images of sentence. Words 'principal,' 'territory,' and 'independence' stood out most prominently. Even while I read, I connected it with my history work. Visual image of table at which I work, and a 'feeling' that the sentence belonged to me. Visual image of the Mississippi valley, schematic and a good deal in the background. The country was being extended into this valley. Sense of distance and direction to Atlantic states. The word 'independence' meant the Revolution. This was mostly direction and distance, and a schematic image of the Revolution. Independence Hall was included.

Some sentences arouse very little imagery, and yet are understood. *Life is ruled by the power of the deed.* 'Deed' was at first read as 'dead,' and gave an image of Egypt and some mummies. When I read it again, I knew what it meant, but it was not as clear as usual. Slow repetition of the sentence. Emphasis on 'life,' 'rules,' 'power,' and 'deed'. 'Life' called up an image of a Chicago street. 'Deed' gave an image of myself doing something.

The third series was similar, except that the stimuli were whole paragraphs, varying in length, but all longer than the sentences. Here again a description of anything which could have been seen is very fully illustrated by visual images. In some cases these are added as the words come, and make a whole picture; in others the images seem to refer to individual meanings of the words, and not to the whole situation. *An arduous task must have been that of the first ministers of the Jamestown Church. A part of religious services enjoined were as follows: on week days, early in the morning, the captain sent for tools in place of arms, when the 'serjeant-major' or captain of the watch, upon their knees, made public and faithful prayers to Almighty God for His blessings and protection to attend them in their business for the whole day after succeeding.* Visual image of some ministers. Sense of distance and direction to Jamestown, visual image of the town, and vaguely of a church. At 'religious services,' image of a church. 'Early in the morning' gave an image which seems to grow or develop. First there was what I call the representation of a morning, which was partly visual but included other sensations, such as the pressure of air on the

face. Visual images of captain, tool, army, etc. 'Upon their knees,' image of people praying.—Even when the meaning is abstract, there is still imagery.

The second series shows an interesting case of the process by which a visual image becomes stereotyped and loses its particular quality as it recurs repeatedly in the same connection. Several of the sentences were from an article on Buddhism, and this word called up the same association each time, with some modification, as follows. (1) Visual image of the word Buddhism and of a Dr. X whom I once heard lecture on this subject. Visual image of something brown, which represents India, and contains an idol. (2) 'Buddhist' gave image of Dr. X. A long row of idols and something brown. (3) Image of a sort of conventionalised Dr. X. Any reference to Buddhism always calls up Dr. X, and this has happened so often that the image seems to have ceased to be personal. (4) Visual image of Dr. X; it was rather an image of an image. (5) Schematic image of Dr. X.

The 100 introspections written by HC on the understanding of single words need not be discussed in detail. Some of the reports show that the meaning of a word may be carried, in whole or in part, by a motor image or an organic sensation. *Grip*. Visual image of a hand reached out to grasp something, and muscular image of the sensation in right arm and hand when something is grasped. *Approval*. Image of a person vaguely seen nodding his head and smiling. This was accompanied by motor images of the action, which were much stronger than the visual, and yet the latter was not of myself. *Stroke*. Swift motor image of striking something. *Wanted*. Organic sensation in stomach.

Several of these reports show well how the mind goes from one to another of several possible meanings when the word is presented without context. *Glasses*. Visual image of spectacles alone, then on a person. Then of tumblers on a table. *Reproduced*. Verbal image of the word. Visual image of my abbreviation for 'reproductive tendencies,' then of some eggs, then of two sheets of typewritten matter with a carbon sheet between. Verbal image 'manifold.'

Perhaps this is the least inappropriate place to say a few words about a series of experiments which do not belong exclusively to the discussion of any particular attitude, and yet throw some light upon the whole subject. Their only connection with the preceding is the fact that they were performed by the same observer (MC) at about the same time; their bearing is rather on imageless thought than on the attitudes proper. It will be remembered that Marbe used the *Ausfragemethode*, and that his questions were criticised upon the ground that they were too simple, and could be answered merely associatively, without any thought. Bühler avoided this danger by giving aphorisms, which offered some difficulty to understanding, or asking questions, which required, for instance, the consideration of whole periods in the history of philosophy in a brief time.

The object of the present series was to combine these methods into a differential or contrast method in order to bring out the difference in consciousness between the answer which required thought and that which did not. The subject chosen was history, because this was of special interest to the obser-

ver. She was asked 50 questions, chosen indiscriminately from many periods of general history, and ranging in difficulty from "When was America discovered?" to "What were the constitutional difficulties in the way of reconstruction after the Civil War?"

We find that visual images play an important part in this observer's memory of history. Centuries and special dates within them are seen schematically and often in colors, while persons and events are assigned to certain periods because they are seen to be in them or like them in color. *Name the first five presidents of the United States.* Answer: *Washington, J. Adams, Jefferson, Madison, Monroe.* Visual image of each man as his name was given. Jefferson was especially clear. With Adams, image of date 1801. Schematic image of length of terms. It was dark colored and had some spaces marked off, twice as much for two terms as for one. In connection with Madison, image of War of 1812 and of an old book on the subject. Adams seemed to stand in a corner, a turn between two centuries.

The facts of history as well as their dates are in many cases apparently read off from visual images which had represented them in the past. *Was there any connection between the French and American Revolutions?* Answer: *Yes. The French assisted the American Revolution and are said to have incited it. The French Revolution was influenced by the example of the American.* Visual image of a book on the subject. Image of the American Revolution and of arms and ammunition on a ship being sent by the French to America. Then of a Frenchman sitting in a coffee-house finding out public opinion about the Revolution. I seemed to see the two wars at the same time, and to know that the American happened first and was an example. *What became of the Celts when the Teutons invaded England?* Answer: *They were partly absorbed, partly exterminated, partly pushed into Wales.* Image of Wales and of Celts and Teutons fighting. Then of the whole country with some Celts and Teutons intermingled. This is what made me say 'absorbed.'

Sometimes the answer is so familiar and comes so readily that there is very little else in consciousness. *When was America discovered?* Answer: 1492. Visual image of the date with a red halo, also of Columbus in Spain with distance and direction to Spain. *Who first sailed around the globe?* Answer: *Magellan.* Visual image of Straits of Magellan, of the man, and later of the name. *When was the fall of Rome?* Answer: 476. Image of date, and of Rome surrounded by barbarous hordes.

Let us summarise the facts which the two hundred and seventy introspections show in regard to the understanding of words and sentences.

(1) A word which is not familiar calls up others which are similar to it in sound, and the images are appropriate to these familiar words. (2) Words like 'to,' 'which,' 'of,' which do not ordinarily occur without context and do not refer to an object that can be represented by an image, also show to some extent the tendency toward mere auditory association. Their appearance in this strangely unconnected position is usually followed by groping or blankness of consciousness. They often form a context for themselves, by verbal association with some word which could grammatically follow them. (3) Words which refer to objects of sense, visual or otherwise, are often represented by images. (4) Words which are capable of more

than one interpretation usually excite in quick succession images appropriate to the different meanings. (5) In comparing the average length of the introspections on words, sentences, and paragraphs, we find that they are in no sense proportional to the length of the stimulus; on the contrary, they are all of about the same length.

The very fact that the single words stood alone, out of all connection, introduced an unfamiliar element, and gave opportunity for all sorts of associations. Experiments upon single words are, in fact, comparable to 'free associations.' When a word comes without context, and the time which the introspection is to cover is not definitely limited, the images, visual, verbal and other, which are aroused, are likely to be numerous and varied. Most of them refer in some way to some meaning of the word, or to its connection with events of our own lives; but some may be irrelevant. If now, we read the same word as part of a short sentence, we get somewhat the same effect as when an association is guided by a word just heard, or when the observer adds to the *Aufgabe* an additional self-imposed condition. The *Aufgabe* now is not "Get any meaning of the word," but "Get a meaning which goes with these other words." Moreover, the time is shortened; the words follow one another in quick succession. This limitation in time tends to inhibit part of the images, and the context determines which of them shall be suppressed.

It may be objected that it is not always the wrong association that is inhibited. Our introspections, as well as those of previous writers, show that often the images are inadequate, irrelevant, or even contradictory. So far as inadequacy is concerned, however, we have no criterion, save the facts themselves, by which we can decide how clear or complete an image must be in order to carry a meaning. Again, the image which is logically contradictory may yet have enough in common with the meaning of the word to be psychologically adequate to this meaning. Two words cannot, indeed, be spoken of as contradictory, unless they have something in common; they must at least belong to the same universe of discourse, to the same context; and it is just this context which, recalled in any form whatever, constitutes a more or less general meaning. Logically, it is not easy to see why a bird, described as white with a black ring around its neck, should be imaged in its ordinary colors; or why a description of dogs carried in a basket should give rise to images of cats jumping out of a panier. Our own experiments are not free from such anomalies. The word 'noiseless' arouses an image of some particular noisy children; 'electric' is followed by an image of a park which is not electric; 'home' recalls France,

and the idea that the language of that country has no word of the same meaning. In every one of these cases, however, there is sufficient connection between the logical meaning of the word, and the psychological content of the act of understanding, for the latter to carry a general meaning.

The third case, that in which the imagery is neither inadequate nor contradictory, but irrelevant, is less easy to explain. It is a fact of observation that the wrong meaning is not always inhibited by the setting and the additional *Aufgabe*, but runs along parallel with the understanding of the sentence. Every word, however, is not of equal importance for the understanding of the whole; and even if a single word is given a wrong interpretation at the time of reading, the meaning of the whole may be fairly clear. The introspections show cases in which the word was seen only in part, or was at first misread, and the context of the wrong reading immediately appeared in consciousness. This often occurs in everyday experience, without attracting attention. The mistake is corrected as we go on, and the wrong image is replaced by others which are more consistent with the meaning of the situation.

These attempts at explanation are tentative only. We have the fact that understanding may at times be mediated, psychologically, by images which logically are inadequate, irrelevant or directly unsuited to their office. The road to final explanation lies through a detailed study of the conditions under which such representations of the act of understanding take shape. Their appearance in experiments like our own probably depends, in many cases, upon ingrained habits of reproductive tendency, which by lapse of time are inaccessible to introspection. Our aim must be to catch them in the making,—either by casual observation in everyday life, or by way of specially shaped observations in the laboratory.

Unanalysable feelings of RELATION have been postulated by various writers. Woodworth, in particular, has made experiments with words and with papers of different colors and shapes, arranged according to the rule of three, which, as he believes, show that a relation is sometimes conscious as an 'imageless' thought. His verbal stimuli were presented in the form London: England :: Paris: X. The observer was to supply the fourth term of the proportion, and to give a complete introspection. The reports fall into four classes. (1) When the relation is easy to grasp and the missing term is readily found, very little consciousness appears. "There was nothing in my mind," said one of the subjects, "except that I wanted to answer your question right." The answer

comes immediately, on the hearing of the three given terms. (2) When there is more difficulty, the relation sometimes receives a name before the answer is found. (3) Sometimes the relation is pictured, in some form of imagery. (4) Sometimes the subject reports that he felt the relation, but did not name it or have an image of it.

Unfortunately, the author does not tell us what fraction of the whole number of experiments belongs to the fourth class. If the reports of this group were numerous, they might offer some evidence for the existence of unanalysable feelings of relation. If they were not, they may well be explained by the incompleteness of introspection. All observers probably fail at times to analyse into simplest terms, or to report all the contents of consciousness.

We have ourselves made several series of experiments on this subject, all of which, in form at least, are based on those of Woodworth. In the first series, the stimulus consisted of three words in the form of a proportion with the fourth to be supplied, exactly as in Woodworth's experiments. In fact, a few of the examples were taken bodily from his paper. The observers were V, G, and F.

The relation is sometimes present in consciousness as a word.

V. *A book: a magazine: : a chair: a stool.* Visual image of a red book and a magazine side by side. At hearing the word 'chair' great surprise,—a muscular contraction and a gasp. I looked at a chair in the room, then at a table. Tendency to say 'table,' because I had a kinæsthetic idea that a book is squarer and higher than a magazine. Same of table and chair. This was inhibited, I don't know how. Visual image of a footstool. Then I said 'less than a chair.' Then said 'stool' aloud. No balance. The whole took effort.

F. *Red: blue: : green: yellow.* I started to say this automatically. Then I repeated the stimulus and said 'intermediate' verbally. Some kind of consciousness that meant 'principal colors'. I did not say 'principal.'

G. *Family: individual: tree: fruit.* Verbal completion in background. Said 'group to one.' When you said 'tree,' I said 'tree is individual itself.' This was in the background. Articulated 'Tree produces what? Fruit.' There was no association ready, and I had to make one by making a new sentence. 'Species' also present verbally. Verbal part in background.

Sometimes the relation is represented visually, as in Woodworth's third division.

F. *Book: chair :: table: floor.* I saw a chair with a book on it. Visual image of a table on the floor. The relation was kinæsthetic and visual.

V. *Man: boy :: woman: girl.* Visual image of a small boy in a blue sailor suit. Meaning of 'woman' carried by a vague image of a red plaid skirt. Then a blank. I said 'girl,' but I don't know why. I was surprised when I said it. This was vaguely organic, and a little gasp occurred. The relation between man and boy was one of height,—a tall and a short line side by side. Just after I finished, I thought perhaps I should have said 'little girl.' This was vaguely verbal.

By far the larger number of relations which were carried in sensory terms could not be put strictly in either of these

classes, but were combinations of images from different sense departments. Organic sensations were prominent for all the observers.

V. *London: England :: Paris: France.* I did not think of London till I heard 'England.' Vague image of the map of England with a black dot standing for London. Kinæsthetic image of drawing a circle and putting a dot inside it. At 'Paris' I had a kinæsthetic image of making a dot, and visual image of a black dot. Without any effort I said 'France,' and had image of drawing a circle around the dot. Very pleasant. The pleasantness included a kind of balance which was vaguely visual and organic. At 'London' image of a capital L, and at 'France' image of a capital F.

F. *To: fro :: back: front.* Repeated the stimulus twice. Organic and kinæsthetic images or sensations of swinging arm in a circle while saying 'to and fro.' I marked the rhythm with words and breathing. Very vague visual images. A thin black thing which was moving like a pendulum. I could not see the whole pendulum, but only the arc that it described. Organic sensations with the pendulum. Suddenly 'back' coincided with one swing, and 'front' with the return swing. The visual part was six or eight feet off and below me.

G. *I: we :: he: they.* This was kinæsthetic. I put 'I' in the first line, 'we' in the fourth, he (he, she, it) in the third, and 'they' in the sixth. This was the declension in an old grammar. I did not see 'he, she, it,' but the line was long kinæsthetically, while 'I' and 'they' were dots.

Woodworth's fourth class, of cases in which the relation was present in consciousness but not analysable into sensory or affective terms,—the class upon which he bases his whole conclusion,—reduces, in our own experiments, to two equivocal instances.

G. *Color: brightness :: tone: intensity.* (G thought of intensity as an attribute.) Short period of confusion, which was muscular contraction. I repeated verbal stimulus and completed it almost automatically. Background filled with vague memories, in visual images and eye-movement, of experiments and discussions on brightness and intensity.

F. *London: England :: Paris: France.* I fell into the swing as soon as you started to read. It was familiar. This was a real change of muscular attitude, a sort of relaxation.

Woodworth's first class, in which the relation is not present in consciousness in any form, is abundantly illustrated.

G. *Father: son :: mother: daughter.* Purely verbal. The vaguest articulation of 'mother.' No relation about it.

G. *Red: blue :: green: yellow.* No relation. I was listening to the colors, and added the one you did not name, as I should have been ready to mention any one.

F. *Is: are :: was: been.* Verbal rhythm. I used to say 'is, are, was, been.' There was just the swing.

F. *Boy: man :: girl: woman.* Verbal. No image.

V. *Is: are :: was: (am) were.* Strong tendency to say 'am,' though I knew it wasn't right. This was kinæsthetic. When the stimulus was repeated, the 'r' in 'are' caused me to say 'were,' the two r's balanced. I did not think of the meaning till afterwards.

These seemed to be cases of mere association, in which the relation had no part. In order to test this conclusion, the method was slightly modified in two ways. Mingled with the

other stimuli were proportions made of pairs of familiar expressions, in which there was either no relation or one that could be recognised only with some ingenuity. The question implied was, whether these pairs would be replied to as promptly as the true relation pairs. Almost invariably this was the case. The other modification was the introduction of proportions in which the relation was reversed, that is, in which the third term really corresponded with the second and the fourth (to be supplied) with the first. In a few cases the observer refused to react; but this inhibition occurred only after such a proportion had already been given, and automatically reacted to. After the reaction, the observer sometimes saw that the order was wrong, and was therefore more careful the next time. In some instances, the change was never discovered at all. The following will illustrate the reactions to familiar phrases.

V. *Up: down :: out: in.* I responded immediately without reasoning, then wondered aloud why this was right. Felt tired.

G. *Loud: soft :: dark: light.* Almost reflexly. A little pause of hesitation during which I quickly went over the whole again in abbreviated form. No relation conscious.

F. *Live: die :: sink: swim.* Mere verbal association. When I stop to think, there is opposition, but this was not conscious. (F. can repeat the quotation from which this proportion is taken, but was not conscious of it when the reaction was made.)

Some of the wrong proportions were reacted to as follows:

F. *Father: mother :: aunt: uncle.* Quite verbal. I don't know whether the answer is right or not.

F. *Day: night :: winter: summer.* It is a muscular attitude which makes me answer. I was set for the rhythm, and reacted just as I should fill out an incomplete line of metre. The word came up of itself; winter and summer go together.

V. *Day: night :: winter: summer.* I said 'summer' because it seemed to belong there. I heard myself saying it before I said it. Later had a kinæsthetic feeling that it was backwards,—lack of balance and a feeling of twisting around.

A few experiments were performed, with the same observers, by means of little slips of colored paper. Three colors were given, and a fourth was to be added that would have the same relation to the third as the first to the second. The relations were not complicated by the introduction of different shapes and sizes. The method was not promising, and was soon discarded. When three of the four principal colors of the spectrum were given, the fourth was added without any 'feeling of relation.' It was simply the 'filling out of the series.' In the remaining cases, the relation was represented visually, verbally, or kinæsthetically.

The next series was intended to approach the problem of relation from a genetic standpoint. The purpose was to

establish some arbitrary relation, and to observe what took place in consciousness as it became more and more familiar.

The observer was told that

S is the cause of T
 H " " " K
 L " " " M

The proportions then combined three of the letters, in various ways, and the observer was asked to add the fourth term. The association was made by G originally from the written page. He was given the above statement to read and fix in mind. He disregarded the word 'cause' entirely, and remembered the letters as related merely by spatial arrangement, not visually, but by eye-movement. A movement of the eyes across and down must be followed by another in the same direction in order to make the relation correct. When the proportion $H : M :: S : ?$ was given, G reacted with K because this reply made two parallel diagonal lines. Although the proportions were too easy at the start to give the method a fair trial, the 76 experiments done with G show some effect of habit. At the beginning, the whole relation was carried by eye-movement; it was movements that were equated and that therefore represented the relation. Verbal ideas sometimes entered in. As early as the fourth experiment, $H : K :: L : M$, G reports: 'I did not jump to L. I went down from K to M.' Abbreviation is beginning. From the twenty-eighth, the verbal reactions are numerous, though the eye-movement continues to some extent to the end. When $H : K :: L : ?$ was given, G simply went down the alphabet automatically.

In order to prevent this unforeseen possibility, different letters were given to F.

D is the cause of E
 R " " " L
 T " " " Q

The statement was read to him, in order that a merely spatial relation might not fix itself in the mind. Nevertheless, this result appeared, to a certain extent; the causal idea was entirely forgotten. Two letters of a pair were associated, and at first F did not distinguish the first from the second. If both numbers of one pair were given and only one of another, the missing term was immediately added. F also tended to read meanings into the letters. These methods of association made the reaction so easy that it was automatic, and conscious content was lacking from the first. F explained his reactions by saying that they 'just go together.' As the series offered no chance for improvement, it was abandoned after the nineteenth experiment.

The same letters were read to V. She visualised the top pair with 'cause' written between them, and the others below with ditto marks under the 'cause,' though she had not seen the paper. Sometimes the reactions were merely read off from this image. The fourth term was often supplied because it went with the third; there was no reference to what had come before. Only fifteen tests were given.

In all the experiments by this method, there is not a single case of a relation being consciously carried in non-sensory terms. Either it is definitely describable, or it is not conscious at all and the reaction is automatic. In the latter case it was usually immaterial to the observer whether the answer was right or not. Sometimes it was worked out carefully afterwards, and judged as to correctness; but in that event the relation was represented in some form of imagery.

As this method had proved too easy to exhibit the automatic reaction in process of development, another was devised. This time the stimuli belonged to three sense-orders, auditory, tactual and kinæsthetic. The observer sat with his right arm on a Sanford elbow-board, and the index-finger of his left hand on a small lever which moved up and down. The tactual stimuli, large and small pieces of sand-paper and of felt, were presented by being laid under the fingers of the right hand on the arm rest. A low and a high tuning-fork stood near by. The observer was told that the low tone was to be thought of as large, the high as small, while each one might be either strong or weak. The sand-paper was intended to represent strength or harshness, the felt weakness or softness; each might be either large and small. The arm movement was large, the finger movement small, while each might be either strong or weak. This arrangement was, no doubt, arbitrary, but it was arbitrary for a purpose. Easily perceived relationships had proved inadequate, and it was hoped that the artificiality of these new ones would make the reactions sufficiently difficult. Only G and V took part in this series of experiments.

G's reactions were at first almost without exception mediated by verbal expressions of the relation. He would say 'loud-soft' or 'strong-weak,' and the reaction followed. The verbal ideas were sometimes more complex than this, or the relation was partly kinæsthetic. It will give some idea of the progress of mechanisation if we fractionate the results. In the first 24 experiments there is not a single automatic reaction. The relations are carried in verbal terms. From the twenty-fifth to the forty-eighth there are 11 automatic reactions, in which the relation was not conscious. From the forty-ninth to the seventy-second there are 13, and from theseventy-third to the ninety-sixth, 17. The reactions are most often automatic when the three given stimuli are from the same sense department. There were only four possible variations in one sense department, and when three were given the fourth followed automatically. The series was carried so far, however, that even proportions between stimuli of different sense orders were sometimes reacted to automatically.

V made 40 experiments by this method but with no new result. The relation was usually verbal, once or twice kinæsthetic, and several times purely associative.

When we consider all of the relation experiments, we see that by far the larger part of the reactions were accompanied by some 'consciousness of relation' in terms of sensory or verbal images, and that the rest prove to be mere associations or else tendencies to fill out a group, by adding the inevitable fourth member, without any consciousness. Woodworth's fourth group is not paralleled in our experiments.

CONCLUSION

In conclusion, we may attempt to sum up the arguments which make against the simplicity of the 'conscious attitude'

and the existence of 'imageless' thought. These may be divided into the negative or critical, and the positive or those based upon our own experimental work. Under the former head the following may be noted:

(1) Having been named and negatively defined by Marbe, the *Bewusstseinslagen* are henceforth taken for granted. They are reported, along with sensation and image and feeling, in the analyses of complex states, and little or no attempt is made to analyse them.

(2) Nevertheless, they are, on several occasions, at least partly analysed, as witness Orth's account of doubt, Messer's and Watt's of trying to remember, and the discussions of the *Aufgabe*, which show it to be an attitude derived by practice from an analysable situation.

(3) The cases in which thought-elements or imageless thoughts or attitudes are reported as the 'consciousness that,' etc., are cases not of psychological description, but of the translation into words of the meaning of a conscious state (*Kundgabe*).

(4) Our own conclusions are based upon the introspections of seven observers, of whom all but one had had several years of psychological training. These observers were not all of one type, but ranged from the strongly visual to one who almost never has visual images, and from those who almost never report kinæsthetic sensations to those for whom these sensations and images are essential. These seven persons wrote, altogether, somewhat more than fourteen hundred introspections. In the series with point-letters alone, over four hundred cases of attitude are specifically reported,—aside from recognition, which is assumed to be present in all observations. Of these four hundred, about one fourth were merely named, while the remaining three fourths are more or less completely analysed. When the attitudes occur often enough to give a basis for generalisation, there is striking agreement between different observers and for the same observer at different times, and we are thus able to pick out, with a fair degree of assurance, the pattern of consciousness which represents a given situation. The introspections of any one observer show different stages of clearness and intensity of imagery, which allow us to connect, by a graded series of intermediate steps, a complex of vivid and explicit imagery with a vague and condensed consciousness which we suppose to represent what is called 'imageless' thought. The *Aufgabe*, recognition, and the feeling of relation are shown to be capable of development, by a process of change and mechanisation, from states which are obviously complex and imaginal.

It may, we think, be fairly said that the attitudes here analysed are typical of the whole class; they are certainly among those most often mentioned by writers on the subject. But, if part of the class can be reduced to simpler terms so often, so definitely, and so uniformly, there is every reason to believe that the rest will show themselves similarly complex, when they are subjected to the same analytical treatment. The general conclusion to be drawn from the sum of our results is that conscious attitudes can be analysed into sensations and images and feelings, or traced genetically to such analysable complexes, and therefore do not warrant the proposal of an additional conscious element.

AN EXPERIMENTAL DEMONSTRATION OF THE BINAURAL RATIO AS A FACTOR IN AUDITORY LOCALIZATION

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I. HISTORICAL

It is difficult to ascertain just when the belief in the binaural ratio as a factor in auditory localization came into vogue. The experimental arguments offered in favor of this belief, however, are not so hard to trace. They began with the tuning-fork experiment of Weber,² and have been continued

¹Vide von Kries: *Ueber das Erkennen der Schallrichtung*, *Zeitschr. f. Psychol. u. Physiol.*, I, 1890, 236-251; and Dunlap: *The Localization of Sounds*, *Psychol. Rev.*, Monog. Suppl., Vol. X, No. 1, 1908, pp. 5, 8, 15.

²Vide Dunlap: *Op. cit.*, pp. 5, 10, and 15.

³Weber: *Programm. Coll.*, 4 2. This experiment was not offered by Weber as an argument for the binaural ratio of intensity, although it has frequently been cited as furnishing such argument.

by the work of Fechner,¹ Rayleigh,² Politzer,³ von Kries and Auerbach,⁴ Tarchanoff,⁵ Steinhauser,⁶ Urbantschitsch,⁷ Thompson,⁸ Kessel⁹ von Bezold,¹⁰ Schaefer¹¹, Smith,¹² Bloch,¹³ Pierce,¹⁴ Matsumoto,¹⁵ Melati,¹⁶ Stenger,¹⁷ Starch,¹⁸ and Wilson and Myers.¹⁹

A résumé of this work down to 1901 has been given by Pierce. It will be sufficient, therefore, for the purpose of this report to continue the résumé up to the present time, and to make a brief statement of all the lines of argument that have been advanced for the binaural ratio as a factor in auditory localization.

¹Fechner, G. T.: *Ueber einige Verhältnisse des binocularen Sehens* (Chap. XVIII, *Ueber einige Verhältnisse des zweiseitigen Hörens*). Abhdlg. d. Sächs. Gesellsch. d. Wiss. (Mathemat. Klasse V), Bd. V, S. 543. 1861.

²Rayleigh, Lord: *Our Perception of the Direction of a Source of Sound*, Trans. Mus. Ass. 1876; *Acoustical Observations*, Philos. Mag. (5) Vol. III, 1877, p. 456.

³Politzer: *Studien über die Paracusis Loci*, Archiv. f. Ohrenheilk. 1876, XI, 231.

⁴von Kries u. Auerbach: *Die Zeitdauer einfachster psychischer Vorgänge*, Archiv für Anatom. u. Physiol., 1877, 321-337.

⁵Tarchanoff: *Das Telephon als Anzeiger der Nerven und Muskelströme beim Menschen und den Thieren*, St. Petersburger med. Wochenschrift, 1878, No. 43, pp. 353-354.

⁶Steinhauser, Anton: *The Theory of Binaural Audition: A Contribution to the Theory of Sound*, Philos. Mag., Ser. 5, Vol. VII, 1879, pp. 261-274.

⁷Urbantschitsch, V.: *Zur Lehre von der Schallempfindung*, Pflüger's Archiv, XXIV, 1881, 579.

⁸Sylvanus Thompson: *The Pseudophone*, Philos. Mag. (5), VIII, 1879, 385-390. *On the Function of the Two Ears in the Perception of Space*, Philos. Mag. (5), XIII, 1882, 406-416.

⁹Kessel: *Ueber die Function der Ohrmuschel, bei den Raumwahrnehmungen*, Archiv f. Ohrenheilk. XVIII, 1882, p. 120.

¹⁰W. von Bezold: *Urteilstäuschung nach Beseitigung einseitiger Harthörigkeit*, Zeitschr. f. Psychol. u. Physiol., 1890, pp. 486-488.

¹¹Schaefer, K. L.: *Lokalisation diotischen Wahrnehmungen*, Zeitschr. f. Psychol. u. Physiol., I, 1890, S. 300-309.

¹²Smith, G.: *How do we Detect the Direction from which Sound Comes?* Cincin. Lancet-Clinic, n. s., XXVIII, 1892, p. 542.

¹³Bloch: *Das binaurale Hören*, Wiesbaden, 1893, pp. 61; *Zeitschr. f. Ohrenheilk.*, XXIV, 1893, pp. 25-86.

¹⁴Pierce, A. H.: *Studies in Space Perception*, 1901.

¹⁵Matsumoto: *Researches in Acoustic Space*, Studies from the Yale Psychological Laboratory, V, 1897.

¹⁶Melati, Gino: *Ueber binaurales Hören*, Philos. Studien, XVII (3), 1901, 431-461.

¹⁷Stenger: *Zur Theorie des binauralen Hörens*, Zeitschr. f. Ohrenheilk., XLVIII, 219.

¹⁸Starch, D.: *Perimetry of the Localization of Sound*, Psychol. Rev., Monog. Suppl. (Univ. of Iowa Studies), Vol. IV, No. 28, 1905, pp. 1-45; *ibid.*, Vol. IX, No. 2, 1908, pp. 1-55.

¹⁹Wilson, H. A. and Myers, C. S.: *The Influence of Binaural Phase Differences in the Localizations of Sound*, The British Journal of Psychology, 1908, II, pp. 362-386.

Since 1901 reports of work on the general subject of auditory localization have been published by the following investigators: Lobsien,¹ Angell and Fite,² Melati,³ Gamble,⁴ Angell,⁵ Seashore,⁶ Bing,⁷ Urbantschitsch,⁸ Stenger,⁹ Bard,¹⁰ Starch,¹¹ Rayleigh,¹² More and Fry,¹³ Bowlker,¹⁴ Wilson and Myers,¹⁵ and Hicks and Washburn.¹⁶

Of these only six bear with sufficient directness and definiteness upon the subject of this report to warrant consideration here; namely, the papers of Angell, Angell and Fite, Starch, Rayleigh, More and Fry, and Wilson and Myers.

In 1903 Angell,¹⁷ in furtherance of the suggestions and ob-

¹Lobsien, Marx: *Ueber binaurales Hören und auffällige Schalllocalisation*. Zeitschr. f. Psychol. u. Physiol., XXIV, 1900, S. 285-295.

²Angell, J. R. and Fite, W.: *The Monaural Localization of Sound*, Psychol. Rev., VIII, 1901, pp. 225-247; *Further Observations on the Monaural Localization of Sound*, *ibid.*, 449-459.

³Melati, Gino: *Op. cit.*

⁴Gamble, E. A. McC.: *The Perception of Sound Direction as a Conscious Process*, Psychol. Rev., IX, 1902, 357-373; *Intensity as a Criterion in Estimating the Distance of Sounds*, Psychol. Rev., XVI, 1909, 416-426.

⁵Angell, J. R.: *A Preliminary Study of the Significance of Partial Tones in the Localization of Sound*, Psychol. Rev., X, 1903, pp. 1-15.

⁶Seashore, C. E.: *Localization of Sound in the Median Plane*, Univ. of Iowa Studies in Psychology, 1899, 11, 46-54; *A Sound Perimeter*, Psychol. Rev., X, 1903, pp. 64-68; *The Localization of Sound*, Middletonian, 1903 (Dec.), pp. 15.

⁷Bing, A.: *Bemerkungen zur Lokalisation der Tonwahrnehmung*, Monatschr. f. Ohrenheilk., XXXVIII, 1904, 220-225.

⁸Urbantschitsch, V.: *Ueber die Lokalisation der Tonempfindungen*, Archiv. f. d. ges. Physiol. (Pflüger's), CI, 1904, 154-182.

⁹Stenger: *Op. cit.*

¹⁰Bard, L.: *L'orientation auditive angulaire*, Archiv. gen. de Med., CXCIV, 1905, 257.

¹¹Starch, D.: *Op. cit.*

¹²Rayleigh, Lord: *On our Perception of Sound Direction*, Philos. Mag., XIII, Ser. 6, 1907, pp. 214-232; *Acoustical Notes, Sensations of Right and Left from a Revolving Magnet and Telephones*, *ibid.*, pp. 316-319; *Acoustical Notes, Discrimination between Sounds Directly in Front and Directly Behind the Observer*, *ibid.*, XVI, 1908, pp. 240-241.

¹³More, L. T. and Fry, H. S.: *On the Appreciation of Phase of Sound Waves*, Philos. Mag., Ser. 6, XVII, 1907, pp. 452-459.

¹⁴Bowlker, T. J.: *On the Factors Serving to Determine the Direction of Sound*, Philos. Mag., Ser. 6, XV, 1908, pp. 318-332.

¹⁵Wilson, H. A. and Myers, C. S.: *The Influence of Binaural Phase Differences on the Localization of Sounds*, British Journal of Psychology, II, 1908, pp. 362-384.

¹⁶Hicks, J. and Washburn, M. F.: *A Suggestion towards a Study of the Perception of Sound Movement*, Am. Jour. of Psychol., XIX, 1908, 247-248.

¹⁷Angell, J. R.: *Op. cit.*

servations made by Rayleigh,¹ Thompson,² Mach,³ and Pierce,⁴ undertook a systematic investigation of the influence of timbre on the localization of sound. Careful observations, in the open air, were made of the accuracy of the localization of simple tones and of clangs. The sounds employed were a tuning-fork of 1,000 vibrations per second, a stopped pipe of 768 vibrations, a reed pipe of 768 vibrations, a bell with a fundamental tone of 2,048 vibrations, and the noise made by a telegraph sounder. An interpretation of his results, based on the relative accuracy of localization at different points in the vertical, horizontal, and transverse planes, led him to conclude that intensity differences alone are sufficient to enable our confident and correct assignment of the sound (even in case of pure tones) to the median plane, the lateral hemisphere, and, in a general way, to the transverse plane. But accuracy as regards altitude in the transverse plane, or in the region between the transverse plane and the median plane, is apparently dependent upon the modifications of timbre which complex sounds, coming from different directions, undergo, through changes in the intensity of their partials. Considered with reference to its bearing on the binaural ratio, the paper, in its general tone, is against the ascription of too much importance to this ratio as a factor in localization. This position is further supported by experiments conducted by Angell and Fite.⁵

The object of these experiments was to determine the localizing power of subjects who were deaf in one ear. In the first series, only one subject was experimented upon; in the second, several were used differing in age and varying in the length of their period of deafness from one to thirty years. The results of the experiments are as follows. (1) These subjects, especially when practiced, are not greatly inferior, in their power to localize, to subjects of normal hearing.⁶ Dis-

¹Rayleigh, Lord: Transactions of the Musical Association, 1876; and Philos. Mag. (5), III, 1877, p. 456.

²Philos. Mag., XIII, 1882, p. 415; *ibid.* (5), VIII, 1879, pp. 385-390.

³Mach, E.: *Bemerkungen über die Function der Ohrmuschel*, Archiv f. Ohrenheilkunde, IX, 1875, p. 72; *Bemerkungen über den Raumsinn des Ohres*, Poggen. Annalen, CXXVI, 1865, p. 331; *Ueber einige der physiologischen Akustik angehörigen Erscheinungen*, Sitzungsberichte der Wiener Akademie, Abth. 2, L., 1864, pp. 342-363; *Zur Theorie des Gehörorgans*, *ibid.*, Abth. 2, XLVIII, 1863, pp. 283-300.

⁴*Op. cit.*, pp. 92 and 163.

⁵Angell, J. R. and Fite, W.: *Op. cit.*

⁶It is assumed here that these writers would exercise caution in drawing conclusions, with regard to the relative importance of timbre and the binaural ratio of intensity in normal subjects, from the localizing power shown by subjects who have been deaf in one ear for a number of years; because the latter, deprived of the use of the binaural ratio as an aid to localization, would doubtless develop a discrimination of direction based upon difference

tinctions between front and back may be even sharper for these subjects than for those of normal hearing. The localization, however, is generally not so prompt for them as for the normal subject, nor are these subjects so accurate in dealing with unfamiliar sounds. (2) Complex sounds, especially those in which qualitative differences can be introspectively distinguished for the different positions, are localized best. The more nearly the sound approximates a simple tone, the more inaccurate is the localization. "Genuinely pure tones are essentially unlocalizable." (3) There is a marked increase in accuracy with practice. The accuracy of the practiced monaural subject, for example, was found to compare very favorably with that of the unpracticed normal subject. (4) Accuracy was also observed to sustain a close relation to the length of time the defect had existed, and to the age at which it began. For example, subjects of advanced age who had recently become deaf showed much poorer ability to localize than younger subjects who had been deaf for a number of years.

Working in 1905 and again in 1908, Starch¹ carried out an extended series of experiments on the localization of simple tones and clangs. Both monaural and binaural hearing were investigated. In the experiments with clangs, a singing flame, a Galton whistle of 10,000, 20,000, and 30,000 vibrations, the human voice, an electric hammer, a wooden clapper, and a whiff of air were used as the sources of sound. In the experiments with simple tones, a tuning-fork of 100 vibrations per second was used. In the latter experiments, tests were made at different points in the different planes of direction, (a) of the accuracy of localization, (b) of the size of the j. n. d. of direction, (c) of the limen and j. n. d. of intensity, and (d) of the j. n. d. of pitch. A number of conclusions were drawn relative to intensity and timbre as factors in localization.

Space will be taken here only for a résumé of the evidence bearing upon the binaural ratio as a factor in normal hearing, and upon intensity difference as a factor in monaural hearing. No new evidence is advanced in support of the binaural ratio, the object of the experiments apparently being a testing of the arguments already advanced by Rayleigh, Thompson,

in timbre, considerably beyond that possessed by the normal subject. This supposition is, in fact, borne out by their own results, which show how poorly subjects recently deaf localize as compared with those in whom the defect had existed for a number of years. For example, Case F. (*Op. cit.*, p. 453), aged 60 years, deaf one year, gave correct judgments of location in only 19.5 per cent. of the total number of cases; while Case C., deaf from 26 to 30 years, gave 55 per cent. of the total number correctly.

¹Starch, D.: *Op. cit.*

Bloch, and others. Starch finds these arguments confirmed by his own results. The arguments are: (a) the presence of front-back confusion, and its special case, the difficulty of median plane localizations; (b) the inferiority of monaural localization; and (c) the occurrence of the greatest accuracy of localization at points where slight changes in the binaural ratio are most readily perceived, *i. e.*, in front and back near the median plane, and the poorest where these changes are least readily perceived, *i. e.*, at the sides near the aural axis. Starch disagrees with Angell as to the factors in monaural localization. He maintains that, in addition to changes in the quality of a sound when it comes from different directions, there are systematic changes in intensity, which serve as a localizing clue.¹ The following evidence is given for systematic changes in intensity: (1) the limen for intensity, which is lowest in the region of the aural axis, and highest in front and back; (2) the observers' introspections with supra-liminal sounds; (3) the distance tests, which showed that a sound is estimated to be nearest in the region of the aural axis. That these changes of intensity serve as a localizing clue is attested (1) by the introspection of the observers, and (2) by the poor localization when the intensity of the stimulus was varied frequently during the course of a series of experiments. The smaller j. n. d. of direction for front and back, as compared with the region near the aural axis,² he thinks, however, cannot be due to the intensity factor, for there is no corresponding difference in the intensity j. n. d.'s in these positions. He seems inclined to attribute this smaller j. n. d. of direction in front and back, at least in the case of his own experiments with the tuning-fork, to the qualitative factor; for his results show a smaller j. n. d. for pitch in front and back than in the region of the aural axis. Starch interprets his results as, on the whole, favoring the intensity theory. The traditional intensity theory is in the main correct; but, in order to account for monaural localization, and localization in the median plane and the planes parallel to it, this theory must be supplemented by the quality and the monaural intensity factors.

In February, 1907, Rayleigh³ published an article in which he attempted to show that the binaural ratio cannot be a factor in the localization of sounds with a vibration fre-

¹Starch: *Psychol. Rev.*, Monog. Suppl., No. IV, Vol. VI, 1905, pp. 11-12; *ibid.*, No. V, Vol. IX, 1908, pp. 52-53.

²Vide Bloch: *Op. cit.*, p. 55-58; Matsumoto: *Op. cit.*, p. 65-69.

³Rayleigh, Lord: *On our Perception of Sound Direction*, *Philos. Mag.*, Ser. 6, XIII, 1907, pp. 214-232.

quency of 128 per second, or less. In a previous article, published in 1876,¹ he had shown by calculations relating to the incidence of plane waves upon a rigid spherical obstacle, that a sound-wave of that vibration-frequency travelling in the line of the aural axis could not differ in intensity at the orifices of the two ears by as much as one per cent. of its total intensity. It is difficult for him to see how so small a difference could play a very important part in localization; yet he finds, at least within the limitations of his somewhat rough tests, that the tones of forks of 128 and 96 vibrations per second are localized as accurately as those of higher frequency. He infers, therefore, that there must be some other localizing clue for tones of low pitch. The only alternative to the intensity factor, he thinks, is a direct recognition of phase differences by the auditory organ.²

He discusses phase difference in its relation to localization as follows. When the stimulus is at one side, in the line of the aural axis, the opposite ear is "roughly about one foot" (measured on the circumference of the head) farther from the stimulus than the nearer ear. For a fork of 128 vibrations per second, this would make the phase difference between the ears about $\frac{1}{8}$ period; for a fork of 256 vibrations, about $\frac{1}{4}$ period; for a fork of 512 vibrations, about $\frac{1}{2}$ period; and for a fork of 1,024 vibrations, about a whole period. "Now it is certain," he says, "that a phase relationship of $\frac{1}{2}$ period furnishes no material for a decision that the source of sound is on the right rather than on the left, seeing that there is no difference between a retardation and an acceleration of $\frac{1}{2}$ period. It is even more evident that a retardation of a whole period or any number of whole periods would be of no avail."

Having shown that sounds of 128 vibrations or less per second reach the ears in a difference of phase which *a priori* might be considered recognizable in sensation, Rayleigh next attempts to show that these differences actually furnish the clue for the localization of the graver tones. He works with two slowly beating tones of near 128 vibrations per second. In completing a cycle or beat, the phase differences of these tones assume all possible values. When the tones are led to the two ears simultaneously, but separately, he finds that instead of getting plainly recognizable beats, as would have occurred had both the sound-waves been given to each ear, the

¹Rayleigh, Lord: *Our Perception of the Direction of a Source of Sound*. Transactions of the Musical Association, 1876.

²The sound wave coming directly to both ears from a single source could show differences only in complexity, intensity, and phase. The first of these differences is ruled out of consideration by the use of the tuning-fork; the second, by his mathematical calculations.

whole sound mass seems to be transferred alternately from one side to the other. In order to interpret these results, he conducted a second series of experiments. The following results were obtained. (1) It was shown that the transference of the sound from one side to the other came directly after ("followed") the maxima and minima of sound as heard by a second observer, for whom the beats were allowed to occur. This established a correlation between the maximal changes in phase of the sound-waves received by the two ears and the phenomenon of transfer. (2) It was found, in addition, that when the wave of greater frequency was received by the right ear, for instance, the transfer to right occurred directly after agreement of phase, and the transfer to left came directly after the maximal opposition of phase. "The transitions between right and left effects correspond to agreement and opposition of phase, not usually recognized. When the vibration on the right is the quicker, the sensation of right follows agreement of phase, and (what is better observed) the sensation of left follows opposition of phase." The writer interprets this quotation to mean that the sound is heard on the right from agreement to opposition of phase, and on the left from opposition to agreement. Now a consideration of the phase relationships of two sound-waves differing in frequency shows that the wave of greater frequency leads in phase from agreement to opposition, and the wave of lesser frequency leads from opposition to agreement.¹

¹The writer can best show in the following manner what he conceives Rayleigh to mean by leading in phase. The vibrating particles forming each sound-wave execute simple harmonic motion. They may thus, in each case, be considered as moving on the circumference of a circle whose diameter is equal to the amplitude of vibration. For the sake of ready comparison, their amplitudes of vibration may be assumed as equal; and both may be considered as moving on the circumference of the same circle,

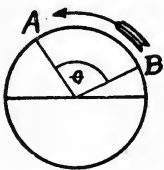


FIG. 1

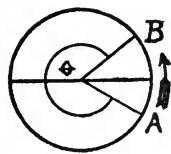


FIG. 2

but at different rates of speed. Taking any two corresponding particles of the two waves, he considers that when the angle θ (the angle separating the radii at the outer termini of which the two moving particles are located, measured in the direction in which the particles are moving) is less than 180° , the particle moving at the greater rate of speed, considered with reference to the direction in which both are moving, will be ahead of the

Thus it can be inferred that the sound was referred throughout in these experiments to the side receiving the wave leading in phase. Rayleigh proposes to make of this a localizing clue, and applies it as a principle of explanation to the phenomenon of localization, as ordinarily observed, for all tones of low pitch. For example, when the source of sound is situated to the right or left of the median plane, the sensation is referred to the right or left, as the case may be, because at any given instant the wave acting upon the ear in question leads in phase the wave acting upon the other ear. And when the source is in the median plane, the sound is referred to that plane because the wave reaches the two ears in phase agreement. Continuing his experiments with forks of higher pitch, Rayleigh finds that the right and left effects occur without considerable diminution up to pitches of 320 vibrations per second. At this point the phenomenon begins to become indefinite and confused. After careful variations of his conditions, he concludes that 768 vibrations per second furnish the limit beyond which no trace of the effect is observed.

In a later report of work,¹ Rayleigh says: "When the sounds proceed from tuning-forks vibrating independently, the phase differences pass cyclically through all degrees, and if the beat be slow enough there is good opportunity for observation. But it is not possible to stop anywhere, or in some uses of the method to bring into juxtaposition phase relationships which differ finitely." He then describes a method of experimentation which allows any particular phase relation to be maintained at pleasure. Two telephone receivers were used as sources of sound. They were excited by a revolving magnet which acted indirectly upon two coils, one in each of the telephone circuits. The planes of the coils were vertical, their centres being at the same level as the magnet. One was fixed, and the other was so mounted that it could revolve about an axis coincident with that of the magnet. The angle between the

slower particle; and, conversely, when the angle θ is greater than 180° , the faster particle will always be behind the slower particle. To illustrate (Fig. 1), let A represent the position of the faster particle on the circle of reference and B the position of the slower particle, both moving in a counter-clock-wise direction. When angle θ is less than 180° , A will be ahead of B ; but when angle θ is greater than 180° (Fig. 2), B will be ahead of A . When angle θ is 180° , or 360° , neither will lead in phase.

The phase relations which any two particles vibrating at different rates will sustain at different times can be very prettily shown for class demonstration by two hands geared to move at the required speeds around a graduated dial.

¹Lord Rayleigh: *Acoustical Notes, Sensations of Right and Left from a Revolving Magnet and Telephone*, Philos. Mag., Ser. 6, XIII, 1907, pp. 316-319.

planes of the coils represents the phase differences of the periodic electro-motive forces, subject it may be to an ambiguity of half a period, dependent upon the way the connections are made. If the circuits are similar, as is believed, the phase differences of the circuits and the electro-motive forces are the same. The circuit of one telephone included a commutator by means of which the current through the instrument could be reversed, corresponding to a phase change of 180° .

In conducting an observation, the sounds given by the two telephones are brought to equal intensities by a proper regulation of the distances between the magnets and the inductor coils. The telephones are thus brought into simultaneous action, and differences of phase are produced by rotating the movable coil; or if complete reversal is wanted, it may be got by means of the commutator. The results, he says, confirm those obtained with the tuning forks. A lead in phase was followed by the reference of the sound to the side receiving the wave which led in phase, and when the planes of the coils were parallel, *i. e.*, when the phases were in agreement or opposition, the sound was located in the median plane.

It may be of interest to note here the results of other observations made under conditions similar to those obtaining in Rayleigh's experiments. Thompson, working in the following way, reports beats, but makes no mention of right and left effects. (1) Tuning-forks, unresonated, were held one to each ear. (2) The sound of one fork was conducted to one ear through a rubber tube and the second fork was held to the other ear. (3) The forks were placed in different rooms and the sound was conducted separately through rubber tubes to the ears. The sounds had no opportunity of mingling externally or of acting jointly on any portion of the air columns along which the sound travelled. Speaking of this observation, he says (*Philos. Mag.*, Ser. 5, III, 1877, p. 274): "The beats were most distinctly heard, and seemed to take place within the cerebellum." So W. H. Stone reports (*Ibid.*, p. 278) that he has been in the habit of using both ears, with a tuning-fork applied to each, in counting beats; and that he finds no difference between the results of this method and that of listening to both forks with one ear. Rayleigh (*On our Perception of Sound Direction*, *Philos. Mag.*, Ser. 6, XIII, 1907, p. 220), speaking of Thompson's results, says: "In an observation of my own (*Philos. Mag.*, Vol. II, 1901, p. 280; *Scientific Papers*, Vol. IV, p. 553), when tones supposed to be moderately pure were led to the ears by means of telephones, a nearly identical conclusion was reached. But although the cycle was recognized, in neither case, apparently, was there any suggestion of right and left effect. In repeating the experiments recently, I was desirous of avoiding the use of telephones or tubes in contact with the ears, under which artificial conditions an instinctive judgment would perhaps be disturbed. It seemed that it might suffice to lead the sounds through tubes whose open ends were merely in close proximity one to each ear, an arrangement which has the advantage of allowing the relative intensities to be controlled by a slight lateral displacement of the head toward one or the other source." This apparently was the only difference in the conditions between the experiments which gave beats and no right and left effects, and the experiments which gave right and left effects but not "plainly recognizable beats." Hermann (*Zur Theorie der Combinationstöne*, *Pflüger's Archiv*, XLIX,

1891, pp. 499-518) found that when the waves from two tuning forks were conducted one to each ear, he heard both beats and combination tones. In this case he supposed that the tones, through the mediation of the bones in the head, both acted together on each ear. No mention is made of right and left effects. Cross and Goodwin (Charles R. Cross and H. M. Goodwin: *Some Considerations Regarding Helmholtz's Theory of Consonance*, Proc. of the Am. Acad. of Arts and Sciences, XXVII, 1891, pp. 1-12) found beats and apparently the phenomenon of transfer from ear to ear. The meatus was closed with beeswax, leaving an air column between it and the tympanic membrane. The conduction under these conditions they think was directly to the tympanic membrane by means of the air column and not through the bones of the head to the middle ear or cochlea, because the sound of the fork, when the stem was touched to the wax, was heard long after it had ceased to be audible when the stem was touched to the pinna of the ear. It was also found that it could be heard longer when the stem was touched to the wax than when it was held against the teeth. When two small tonometer forks, tuned to four beats per second, were struck and their stems held against the teeth, "loud beats were heard in the ears. . . . The forks were held in this position until the beats had entirely ceased to be audible, when they were removed and the stem of each was touched to the wax closing the two ears. Instantly the two notes were heard, faintly but distinctly, in the ears to which they were held, and accompanying them were faint beats seeming to wander in the head from ear to ear, as is always the case with binaural beats." The experiment was then varied slightly as follows. One ear only was closed with wax; the other was immersed in a large basin of water. "The experiment was then repeated as above, with the difference that one fork, instead of being touched to the ear, was touched to the marble basin, its vibrations being transmitted to the enclosed ear through the water. The same results were obtained as before." One of the conclusions drawn from these experiments, which is especially of interest relative to Wilson and Myers' experiments and their explanation of the localization of tones of low pitch (*Vide* this paper, pp. 266-69), is that "aerial vibrations acting upon the ear are not transmitted through the skull, or bony parts of the head, from one ear to the other."

The phenomenon of beats has also been reported in this connection by the following experimenters, but none of them has mentioned a right-left transfer: Dove (*Repertorium der Physik*, Bd. III, 1839, S. 494; *Pogg. Annal.*, CVII, 1859, S. 653); Seebeck (*Pogg. Annal.*, LIX, 1841, S. 417; *ibid.*, LXVIII, 1846, S. 449; *Akustik*, Abschn. II, *Gehler's Repertorium der Physik*, 1849, S. 107); Mach (*Wiener Sitzungsber*, L, 1864, p. 356.); Stumpf (*Tonpsychologie*, Bd. II, S. 208, 458, 470); Bernstein (*Pflüger's Archiv.*, LIX, S. 475); Ewald (*Pflüger's Archiv*, LVII, 1894, S. 80); Schaefer (*Zeitschr. f. Psychol., u. Physiol.*, I, 1890, 81); and Melati (*Philos. Studien*, Bd. XVII, 1901, pp. 431-461). Sanford (*Experimental Psychology*, 1898, p. 82), however, working with forks beating once in two or three seconds notices, a shifting of the sound from ear to ear corresponding to the rate of beating.

It is with considerable reluctance that the writers present the preceding brief exposition of Rayleigh's theory and its experimental confirmation, because neither of these is worked out in the original article with sufficient detail to warrant the risk of a definite interpretation. In every case, therefore, where more than one interpretation has seemed possible, the one most favorable to the theory has been chosen. Until all the points involved both in the theory and in its confirmation have received more definite treatment by Rayleigh

the writers feel that positive criticism, either favorable or adverse, is out of the question. The following comments, however, may not be out of order. (1) The theory is purely physical. No attempt is made in any of the points to bridge over the gap between stimulus and sensation. In the treatment of lead and lag, for example, no consideration is given to what the ear, as a sense-organ, might be assumed to recognize as lead and lag. The mathematical propriety of Rayleigh's use of the terms is granted. And by mathematical definition, the faster wave will lead when angle θ is less than 180° . There will always be this characteristic phase relation between the waves coming to the two ears. But to grant that the ear can discriminate which is leading and which lagging, when no position or motion of any of the vibrating parts of the ear can, in the complete cycles of its changes, characteristically stand for lead or lag, and when no lead and lag aspect can be discovered in the sound sensation itself, seems to be ascribing to the auditory mechanism a logical or mathematical power which not even all educated beings possess as an item of culture. For example, when angle θ is say 160° , the faster wave may, at different times when this angle of separation occurs, be in every conceivable stage either of condensation or of rarefaction. During a part of this time, the slower wave will be at appropriate points in rarefaction when the faster is in condensation, and *vice versa*; and the rest of the time both waves will be either in rarefaction or condensation. Thus there is nothing in the position or motion of the vibrating structures of the ear that can be seized upon as characteristic of the lead or lag, as Rayleigh uses the terms, except a relation between direction of motion and angles of separation, and this is discovered only by a mathematical consideration of simple harmonic motion.¹ Just as Helmholtz's theory of vision has

¹There is, for example, an alternative interpretation of lead and lag, which, it seems to the writers, the ear might more plausibly be assumed to recognize; namely, neither particle might be said to lead or lag unless both be moving either in condensation or rarefaction. Then the vibrating structures of the ear will be moving in the same direction, and at any given moment will be displaced in the same direction. Thus, as far as sensations of motion or position are concerned, if such sensations can be assumed for any of the vibrating structures of the ear, there would be a better chance for comparison than by the former interpretation. The writers, however, do not consider that this is the interpretation Rayleigh means to be taken for lead and lag; because (1) it is not the interpretation commonly given to the terms, and (2) in his experiments, it would leave the ear a part of the time in both halves of the cycle of changes without a localizing clue, for there will come times both from agreement to opposition and from opposition to agreement when one wave will be in condensation and the other in rarefaction, and conversely. Thus for a part of the time, in both cases of reference, right and left, there would be no localizing clue. This interpretation would

been called pre-psychological, so may this theory of localization be called pre-psychological.

(2) Speaking of the difference in phase in which the sound-wave from a source to the right on the aural axis would arrive at the orifices of the two ears, Rayleigh says:¹ "It is easy to see that the retardation of distance at the left ear is of the order of the semicircumference of the head, say one foot. At this rate, the retardation for middle C ($C'=256$) is nearly one quarter of a period; for C'' (512) nearly half a period; for C''' (1024) nearly a whole period, and so on. Now it is certain that a phase retardation of half a period affords no material for a decision that the source is on the right rather than on the left, seeing that there is no difference between a retardation and an acceleration of half a period. It is even more evident that a retardation of a whole period or of any number of whole periods would be of no avail." In the preceding quotation just two stages of phase relationship are ruled out as furnishing no localizing clue; namely, a difference of a half period, and a difference of a whole period. A difference of a half period furnishes no clue, because the angular separation of corresponding particles of the two waves is 180° ; hence it would be the same whether considered as acceleration or retardation. Similarly, a difference of a whole period furnishes no clue, because the angular separation of the corresponding particles of the two waves is 360° , hence would be the same considered either as acceleration or retardation. But in the scale of pitches, there are only certain members higher than 512 and 1,024 vibrations whose sound-waves coming from a given direction would always arrive at the two ears with a difference of 180° or 360° . Phase difference, so far as can be readily seen, should furnish a clue for the localization of the higher just as well as of the lower pitches. There seems to be no good reason, then, for making the direct recognition of phase difference a localizing clue for the lower pitches only, and for giving over to difference of intensity the exclusive rôle for the higher pitches. And again, if a direct recognition of phase difference be a localizing clue, it is not easy to see why the right and left effects in Rayleigh's experiments should suffer considerable diminution when the tones were as high as 320, and should cease entirely near 768 or above. It is just as clear that the faster wave will lead from agreement to opposition and the slower from opposition to agreement in this case as in the case of lower tones. And if the tones

work even worse in localization as ordinarily observed, for in every direction there would be certain distances from the ear for which there would be no localizing clue.

¹*Op. cit.* p. 218.

were near together in pitch, the transitions from right to left effects should have come just as slowly and should therefore have been just as easily observed. That is, if 768 and 769 forks were used, for example, the change from agreement to opposition and from opposition to agreement, and the corresponding right-left, left-right transfers, should have occurred only once per second, just as would have occurred with forks of 128 and 129 vibrations per second. (3) Phase difference must make itself felt in consciousness either by means of some change in the sound sensation itself, or by setting up some new sensation alongside the sound sensation; for example, a sensation of position or movement of some of the vibrating structures of the ear. Wilson and Myers¹ conclude that phase difference makes itself felt as a difference in the intensity of the sound heard by the two ears. They explain the localizations in Rayleigh's experiments in terms of this difference in intensity produced by cyclic changes of interference between the sound-waves coming directly to the two ears and those transmitted from one ear to the other through the bones of the head. Rayleigh, however, obviously considers that the effect of phase difference is extra to any differences that may occur in the intensity aspect of the sensations given to the two ears. The question arises as to whether either explanation can be applied further than to the special phenomenon created by the experimental conditions under which they worked. This point will be taken up in a later section of this paper. (4) Until more sensitive tests than those conducted by Rayleigh are made to find out the relative sensitivity of direction-discrimination for low and high tones, it is not demonstrated that there is any need for a supplement to the intensity theory, to account for the localization of low tones.

Later, in 1907, More and Fry² also attempted to show that phase difference serves as a clue for the localization of sound. They worked with tuning-forks of 320 and 512 vibrations per second. The observer was seated at the centre of a large circle marked on the floor of a room. The zero point in the circle was taken directly behind the observer, the 180° point in front, and the 90° points at the sides. A glass funnel 13.5 cm. in diameter was mounted horizontally on a table at the zero point, about 7 ft. behind the observer. Heavy rubber tubing with an inner diameter of about 1.2 cm. connected the funnel with the stem of a glass Y-tube, on the two branches of which rubber tubing of the same size was fitted. These branch tubes ended in glass tubes bent so as to fit into the ears of the ob-

¹Vide pp. 267-68, this paper.

²*Op. cit.*

server. Each of the branch tubes was cut in two at the middle; and by inserting pieces of glass tubing, the experimenter readily altered their lengths without the listener's being aware of the change. Fourteen observers were used. The sound was given at the mouth of the funnel, and the observer was asked to indicate the direction from which it came. This direction was estimated by means of the graduated circle at the centre of which the observer sat. Before each observation, the length of one of the tubes was changed $\frac{1}{8}$, $\frac{1}{4}$, $\frac{3}{8}$, $\frac{1}{2}$, $\frac{5}{8}$, $\frac{3}{4}$, or $\frac{7}{8}$ of a wave-length of the sound used as stimulus. The results, expressed in general terms, showed that when the tubes were exactly equal in length, the sound seemed to come directly from behind; but if one tube was made shorter than the other, by as much as 2 cm., the sound was referred to the side having the shorter tube. These results were taken to indicate that sound is localized by a direct appreciation of the difference of phase of the waves coming to the two ears.

With regard to this work, the writer would point out the following facts. (1) The forks employed were both above the pitch limit at which Rayleigh claims the difference in intensity becomes too small to serve as a localizing clue. Hence these writers, in their support of the phase difference theory, work with tones for which Rayleigh claims the localizing clue is difference of intensity and not of phase. (2) Their results, stated in general terms, do not seem to have any differential value. When one tube is made shorter than the other, the stimulus received by the ear on that side is more intensive than that received by the other ear; hence, by the intensity theory, as well as by the phase difference theory, the sound should be referred to that side. The sounds worked with had a wave-length of 64 (512 fork) and 104 (320 fork) cm. per sec. A change in the length of one of these comparatively short tubes by from $\frac{1}{8}$ - $\frac{7}{8}$ of a wave-length of these sounds would produce a considerable change in the ratio of the distances the sound had to travel to reach the two ears; hence it would produce a considerable difference in the energy of the stimulation given. As nearly as the writer can determine, by comparing the measurements and localizations given by More and Fry with the measurements of the direct paths of transmission of the sound to the two ears from these locations in his own sound-cage, the change of ratio is quite of the same order in the two cases.

(3) Although it may be considered that the results in general are not differential as between the two theories, More and Fry find a crucial argument in the fact that as one tube was progressively made longer than the other, there was not a constant increase in the displacement of the sound toward the ear sup-

plied with the shorter tube. In a few cases, for example, the 90° displacement was made when the change in the length of the tube was only $\frac{5}{8}$ - $\frac{3}{4}$ of the wave-length; and from that point on to a change of a whole wave-length, there was even, in certain cases, a decrease in the angle of displacement. This, they say, on the basis of the intensity theory, ought not to be. There should be, following a change in the relative lengths of the tubes, a regular increase of displacement. To quote: "If it were a question of change of intensity, the change in direction would increase continually and not reach an angle where further increase in the length of the tube produces either a doubtful increase in angle or even at times a decrease." In reply to this, it may be pointed out (*a*) that a sound cannot be displaced more than 90° to either side, and the ratio of the length of the tubes which gave their observers a displacement of the sound 90° to the right was of the same order as the ratio of the lengths of the direct paths of transmission to the two ears when the sound is actually given 90° to the right in the sound-cage used in our own experiments. Moreover, as localizations ordinarily occur in everyday life, the ratio of the distances the sound travels in order to reach the two ears is, in most cases, not even so great as it is with the Titchener sound-cage. The above result, then, is all that could be expected by the intensity theory. (*b*) To account for the decrease in the angle of displacement in a few cases mentioned, it may be said that when the ratio of intensity, by the conditions of the experiment, is made to exceed any ratio that could occur in our daily life, there is no basis for the association of any given direction with that ratio; hence, once this limit is exceeded, regularity of results should not be expected. Under such conditions, one might expect almost any irregularity, but certainly not regularity.

(4) The discrepancy between More and Fry's and Wilson and Myers's¹ results should not be ignored. In both cases apparently the same method of working was used, yet very different results were obtained. For More and Fry's observers a change in the relative lengths of the tubes was followed uniformly by a displacement of the sound toward the side of the shorter tube; while for Wilson and Myers's observers, changes in the length of the tubes were followed by a cycle of changes of localization,—first to the side of the shorter tube, then to the median plane, to the side of the longer tube, back to the median plane, and so on. So great a discrepancy cannot but throw both sets of results open to question, until more work is done under similar conditions.

¹Vide this paper, pp. 266-67.

In October, 1908, Wilson and Myers reported a series of experiments suggested by the work of Rayleigh. The general plan of experimentation was similar to that used by More and Fry. The apparatus,¹ however, was more carefully designed, and the effect of changes in the ratio of the length of conducting paths was possibly more minutely tested out.² The sound was led to the ears separately by the two arms of a rectangle made of tubes of glass and brass joined at the corners by India rubber tubing. The observer's head, in position, occupied the mid-point in the back of this rectangle. Opposite his head, in the mid region of the front of the rectangle, a section 120 cm. long was removed and in its place a section of T-tubing was inserted, the horizontal arm of which was small enough in diameter to slide freely within the main tube, and long enough to permit the perpendicular arm to be moved 60 cm. on each side of the median plane. The perpendicular arm ended in a funnel-shaped collector, in front of which the tuning-fork was sounded. A centimeter scale was mounted behind the T-tube, upon which could be read the displacements of the perpendicular arm to the right or left of the median plane. The tubes leading to the ears ended in wooden receivers or ear-caps, which were pressed against the observer's head, and held in place by retort stands fastened to the table in front of the observer. The movements of the experimenter were shut off from the observer's view by a large screen across the median plane. When the funnel receiving the sound was placed in the median plane, the arms leading to the ears were of equal length, in most cases 317 cm. each. In this position it was found that the sound was located in the median plane. When the funnel was displaced by amounts varying from $0-\lambda/4$ (λ = wave length) or $\lambda/2-3\lambda/4$, the sound was referred to the side toward which the displacement had been made. When it

¹Apparatus somewhat similar to that used by Wilson and Myers is described by Urbantschitsch (*Arch. f. d. ges. Physiol.*, 1881, XXIV, 579-585).

²Sylvanus Thompson (*Philos. Mag.*, Ser. V, Vol. VI, 1878, pp. 386-387) worked in much the same fashion as Wilson and Myers, but with less minute measurement. The ends of a curved copper wire 3 ft. long, bent into two rings, were inserted one into each of the observer's ears. It was found that when the stem of a vibrating fork was set on this wire at the mid point, the sound seemed to come from the ends of the wire in each ear. A change of an inch and a half from this position produced a sufficient difference in the length of the path travelled by the sound to cause it to reach the two ears in complete difference of phase. Given in this position, the sound seemed to come from the back of the head. When the sound was given in intermediate positions, the effect was of a mixed character; part of the sound seemed as if located in the ears themselves, and part of it seemed to come from the back of the head. No change in this result was observed with forks of different pitches, providing that the proper differences in length of path were chosen.

was displaced by amounts varying from $\lambda/4 - \lambda/2$ or $3\lambda/4 - \lambda$, the sound was referred to the side opposite to that toward which the displacement had been made. The same relations between the reference of the sound and the displacements of the stimulus were observed to hold for simple multiples of λ . Thus it was found that when the stimulus is displaced either to the right or left of the median plane, the sound is successively referred, as the displacement is increased, first to the side toward which the displacement is made, back to the median plane, to the opposite side, and back to the median plane, repeating the cycle when the displacement reaches an amount exceeding one wave length.

In explaining these results, Wilson and Myers agree with Rayleigh that the localization of tones of low pitch is dependent upon the difference in the phase of vibration in which the sound waves reach the two ears. They do not, however, believe it is necessary to assume that the localizing clue is a direct recognition of phase difference by the two ears. They contend that "while binaural differences in phase are a primary cause of the observed lateral effects, these effects are ultimately referable to binaural differences in intensity." The stimulus in either ear is a resultant of two vibrations, one communicated directly to the ear, the other indirectly, through bone conduction from the opposite ear. The resultant, now stronger in one ear, now in the other, now equal in both ears, because of progressively changing phase differences between the direct and transmitted waves, determines the direction-reference. Suppose, as in the case of the Rayleigh experiments, two sound-waves of equal amplitude but of unequal frequency enter the two ears. Then from agreement to opposition the faster wave will lead the slower in phase, and from opposition to agreement the converse will be true. When the faster is leading, the resultant of the direct and transmitted waves in the ear receiving the faster wave will be of greater intensity than in the ear receiving the slower wave, and the total sound mass will be referred to that side. Similarly, when the slower leads in phase, the resultant will be stronger in the ear receiving the slower wave, and the localization will occur on that side. When, however, the two waves are in opposition or agreement, the resultants in both ears will be equal, and the localization will be in the median plane, as the intensity theory requires. Since, as they believe, this hypothesis satisfactorily explains the results of Rayleigh's experiments, they do not think that these experiments should be considered as affording differential evidence for the phase-difference theory. Nor do they claim differential value for their own experiments. With reference to physical features alone, both hypotheses are

capable of explaining both sets of results. Wilson and Myers, however, claim an advantage for their hypothesis on the ground of its greater plausibility and of the auxiliary facts that can be cited in its support. On the ground of plausibility, they maintain that their hypothesis is in better accord with the prevailing conception of the origin and nature of nervous impulses. For example, Rayleigh states:¹ "It seems no longer possible that the vibratory character of sound terminates at the outer ends of the nerves along which the communication with the brain is established. On the contrary, the processes in the nerve must in themselves be vibratory, not in the gross mechanical sense, but with the preservation of the period and retaining the characteristic of phase—a view advocated by Rutherford as long ago as 1886." Wilson and Myers believe that there is too much evidence of the specific functioning of end-organs to be overweighed by the results of Rayleigh's experiments. A special sense-organ may be excited not only by the stimuli to which it is especially adapted to respond ("adequate" stimuli), but also by "inadequate" stimuli; for example, electrical, chemical, and mechanical. "Inasmuch as the sensations are similar despite the diverse character of the stimuli, we have hitherto believed that the impulses ascending a sensory nerve depend on the mode of response of the end organ and not directly on the character of the stimulus." By way of auxiliary facts, they cite the results of Mader,² the tuning-fork experiment of Weber, and its modification suggested by Schaefer,³

¹Philos. Mag., Ser. 6, Vol. XIII, 1907, pp. 224-225.

²In Mader's experiments (Sitzungsber. d. kais. Akad. d. Wissens., Wien, 1900, Bd. CIX, Abth. 3, S. 37-75) two tones of nearly identical pitch were separately led one to each ear hole of a skull. A microphone applied to the roof of the skull gave evidence of beats. This is cited as presumptive evidence that the tones were actually passing across the roof of the skull from one ear to the other. (Both tones, however, were generated in the same room, hence there is no guarantee that the microphone was not acted upon by beats in the air wave.)

³Schaefer, K. L.: Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg. 1891, Bd. II, S. 111-114. Wilson and Myers (*Op. cit.*, p. 318) describe this experiment as follows: "A fork, fixed at some distance from one side of the observer, is very gently struck. The observer listens, and notes when the dying tone has become quite inaudible. He then inserts an appropriately attuned resonator into the ear which is nearest the fork; whereupon, the tone is at once softly heard again on that side, as if it came from the resonator. If the meatus of the more distant ear be now closed, the tone becomes at once stronger, and its localization approaches the median plane. If the meatus be then re-opened, the tone immediately leaps back again to the ear in which the resonator is inserted."

Mach has suggested that when a tuning-fork is placed on the vertex, and the meatus of one ear is closed, the tone is localized to that side, because the sound travelling by bone conduction to that ear not only stimulates the cochlea, but sets up in the meatus vibrations which are reflected back and intensify the sound in that ear.

as evidence of bone conduction¹ under conditions similar to those in their own and in Rayleigh's experiments.²

As was stated earlier in the paper, this review will be concluded by a brief résumé of the arguments that have been advanced, up to the present time, for the binaural ratio as a factor in auditory localization. They are as follows. (1) Confusion points are found in the median plane and in the planes parallel to it on either side (Rayleigh,³ von Kries and Auerbach,⁴ Pierce,⁵ and Starch⁶). (2) Monaural localization is inferior to binaural localization. (Politzer,⁷ Arnheim,⁸ Preyer,⁹ Bloch,¹⁰ von Bezold,¹¹ Smith,¹² Angell and Fite,¹³ and Starch.¹⁴) Politzer, Arnheim, Preyer, and Bloch worked with cases of monaural hearing artificially produced; von Bezold, Angell and Fite, and Smith worked with pathological cases. Starch worked with two observers in which the defect was artificially produced, and two in which it was pathological. For Politzer, Arnheim, Preyer, Smith, and Angell and Fite, the test used was accuracy of localization; for Bloch it was the size of the j. n. d.; and for Starch it was both accuracy of localization and the size of the j. n. d.

(3) The greatest accuracy of localization occurs at points where a change of direction produces the greatest change in

¹Against bone conduction *vide* the experiments and conclusions of Cross and Goodwin (this paper p. 260.)

²There may be cited additional casual advantages, so obvious, however, as to be scarcely worthy of mention. (1) Wilson and Myers's explanation does not involve the assumption of any new power on the part of the ear, hence it has the advantage of systematic simplicity. (2) Introspective analysis does not show any aspect of the sound sensation, or any new sensation simultaneous with the sound sensation, corresponding to difference of phase.

³Rayleigh, Lord: *Acoustical Observations, Perception of the Direction of a Source of Sound*, Philos. Mag., Ser. 5, Vol. III, 1877, pp. 456-458.

⁴*Op. cit.*, p. 330, 336.

⁵*Op. cit.*, pp. 56-78.

⁶Psychol. Rev., Monog. Suppl., Vol. IX, 1908, p. 53.

⁷*Op. cit.*, p. 231-236.

⁸Arnheim: *Beiträge zur Theorie der Lokal. von Schallemp. mittl. der Bogengänge*. Diss. Jena, 1887.

⁹Preyer: *Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Pflüger's Archiv, XL, 1887, pp. 618-619. It will be remembered, however, that Preyer and Arnheim believed that the localization is in terms of space feelings aroused directly by the action of the sound wave upon the semi-circular canals.

¹⁰*Op. cit.*, p. 59-73.

¹¹*Op. cit.*, p. 486-487.

¹²*Op. cit.*, p. 542.

¹³*Op. cit.*, pp. 225-246 and 449-458. Angell and Fite claim that a considerable degree of inferiority of monaural hearing exists only in the case of unpracticed monaural subjects. Monaural subjects can be practiced up to the point of localizing almost as well as the unpracticed normal subject.

¹⁴Psychol. Rev., Monog. Suppl., IX, 1908, pp. 40-48.

the binaural ratio, *i. e.*, in front and back near the median plane; and the poorest localization occurs where a change of direction produces the least change in the binaural ratio, *i. e.*, at the sides, near the aural axis¹ (Bloch,² and Starch³).

(4) A difference in the amount of collection of the sound-wave at the orifices of the ears determines the localization to the side receiving the greater energy of the wave (Thompson,⁴ and Kessel⁵).

The total sound-mass is referred to the side of the stronger stimulus when two sounds, one stronger than the other, are given to the two ears. (Steinhauser,⁶ Tarchanoff,⁷ and Matsumoto⁸).

When two tuning-forks sounding with equal intensity are placed one on each side of the head, but one nearer to the ear than the other, the total sound-mass is referred to the side on which the nearer fork is located (Stenger⁹).

When two tuning-forks, sounding with equal intensity and located in the aural axis on either side at the same distance from the ears, are swung in unison from left to right and right to left, a transfer in the localization of the total sound mass takes place following the rhythm of the swing. When both are swinging to the left, the sound is referred to the right, and, conversely, when both are swinging to the right, the localization is on the left. When both are swinging with equal speed in opposite directions, the localization is in the median plane (Fechner¹⁰).

When two vibrating bodies are in contact with the head or very near to it, and the energy of vibration is unequal, the

¹Starch (*Op. cit.*, p. 52.) phrases this as follows. "The accuracy of localization is greatest where slight changes in the ratio are most readily perceived, *i. e.*, in front and back. Localization is poorest where changes in the ratio are not so easily perceived, *i. e.*, on the sides, in the region of the aural axis."

²*Op. cit.*, pp. 31, 35.

³*Psychol. Rev.*, Monog. Suppl., Vol. VI, No. 4, 1904-05, pp. 11-12 and 44; *ibid.*, Vol. IX, No. 2, 1908, pp. 52-53.

⁴Thompson: *Philos. Mag.*, Ser. 5, Vol. VIII, 1879, p. 386; *ibid.*, Vol. XIII, 1882, p. 412.

⁵*Op. cit.*, p. 120.

⁶*Op. cit.*, pp. 188-189. Steinhauser used as the source of sound an instrument called by him the homophone. This instrument consisted of two organ pipes of the same pitch, one of which was supported near to each ear on the level with it. The intensity of the sound was regulated by means of valves controlling the pressure of the air blast used to excite it.

⁷*Op. cit.*, p. 354.

⁸*Op. cit.*, p. 18. Matsumoto used two telephone receivers placed opposite the two ears, one on each side. The intensity of the sound in each ear was controlled by a sliding inductorium.

⁹Stenger: *Op. cit.*, p. 223.

¹⁰Fechner, G. T.: *Op. cit.*, p. 543.

sound is localized within the head but is referred to the side receiving the greater energy of vibration (Urbantschitsch,¹ and Thompson²).

When the stem of a vibrating tuning fork is placed on the vertex of the skull, the tone is localized somewhere midway between the two ears; but if the meatus of one ear is stopped and the wave is reflected back toward the internal ear, the sound is transferred immediately to that side (Weber³).

When a tuning-fork is faintly sounded on one side and heard by the ear on that side by means of a resonator, the sound is referred to that side; but when the meatus of the opposite ear is stopped, the sound approaches the median plane (Schaefer⁴).

One of two fusing sounds may be placed in either of the lateral quadrants without altering the localization of the fusion (Pierce⁵).

II. EXPERIMENTAL

A. THE DEMONSTRATION OF THE BINAURAL RATIO AS A FACTOR

(a) *Lines of Argument.* The object of this paper is to add three lines of argument to those mentioned above. (1) Observers having a natural difference in sensitivity of the two ears show a constant tendency to displace the source of sound toward the axis on the side of the stronger ear; and, conversely, observers without this difference in sensitivity show no consistent tendency toward right or left displacement. (2) Changes in the ratio of sensitivity of the two ears, produced by plugging either ear, are followed by corresponding displacements of the sound toward the more sensitive ear. (3) A natural tendency toward right or left displacement can be corrected by making the proper change in the ratio of sensitivity of the two ears.

The principle involved in the second argument is not entirely new.⁶ It aims at a direct and systematic correlation

¹Urbantschitsch: *Lehre von der Schallempfindung*, Pflüger's Archiv, XXIV, 1881, 579.

²Thompson, *On Binaural Audition*, Philos. Mag., Ser. 5, Vol. IV, 1877, pp. 274-276; *Phenomena of Binaural Audition*, *ibid.*, Ser. 5, Vol. VI, 1878, pp. 383-391.

³Weber: *Op. cit.*, p. 42.

⁴Schaefer: *Op. cit.*, pp. 111-114.

⁵Pierce: *Op. cit.*, pp. 63 and 147.

⁶It might probably be said that the principle involved in the first line of argument is also not entirely new. Results of monaural localization have been reported by numerous investigators; and occasional mention has been made of a suspected influence of difference in sensitivity of the two ears upon the results obtained in cases of binaural localization. (For the best example of this, *vide* Seashore: *Localization of Sound in the Median Plane*, Univ. of

between the intensity of the sound as heard by the two ears, and the direction in which it is referred. This has not been attempted before, although it has been shown more or less definitely that a difference in the energy of the sound-wave delivered to the two ears affects localization. Thompson,¹ for example, with his pseudophone, tried to produce a difference in the energy of the stimuli given to the two ears by means of the way in which the shell-shaped collectors were turned with reference to the direction of the stimulus, and to show thereby that the localization was determined toward the side receiving the stronger stimulation. Though the underlying principle of this general line of argument is not new, the writers have followed it up for the following reasons. (1) Its possibilities for demonstrating the binaural ratio as a factor in localization have not been fully utilized. (2) In order to confirm the intensity theory, it is necessary to show a definite correlation between the ratio of intensity of sensation, and the direction in which the sound is referred. The method of varying the sensitivity of the two ears gives a much safer and more direct means of establishing this correlation than is given by varying the intensity of the stimuli. The writers find, for example, that the ears of many people vary greatly in sensitivity. In fact, so far as his experience goes, it is more common to find a difference than to find the ears of

Iowa Studies in Psychol., 1899, II, p. 49). But for theory, monaural hearing presents a very different case from difference in sensitivity in binaural hearing (when working with monaural hearing the binaural factors drop out entirely); and, furthermore, no systematic attempt has ever been made to utilize differences in sensitivity as a means of demonstrating the influence of the binaural ratio.

For reports on monaural localization, see Politzer (*Loc. cit.*); Preyer (*Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Pflüger's Archiv, XL, 1887, S. 586); Arnheim (*Beiträge zur Theorie der Lokal. von Schallempf. mittelst der Bogengänge*, Diss. 1887.); Münsterberg (*Raumsinn des Ohres*, Beiträge zur Exp. Psy., Bd. II, 1889, S. 182), von Bezold (*Urteilstäuschungen nach Beseitigung einseitiger Harthörigkeit*, Zeit. f. Psy. u. Physiol. II, 1890, S. 486); Schäfer (*Ein Versuch über die interkraniale Leitung leisester Töne von Ohr zu Ohr*, Zeit. f. Psy. u. Physiol. II, 1891, S. 111); Smith (*How do we Detect the Direction from which Sound Comes?* Cincin. Lancet-Clinic, N. S. XXVIII, 1892, 542); Angell and Fite (*The Monaural Localization of Sound*, Psy. Rev. VIII, 1910, 225-266, and *Further Observations on the Monaural Localization of Sound*, *Ibid.*, 449-458); and Starch (*Perimetry of the Localization of Sound*, Psy. Rev. Mon. Supp. IX, 2, 1908, 1-55). For mention of a suspected influence of difference in the sensitivity of the two ears upon localization, see Pierce (*Op. cit.*, p. 106), and Starch (*Op. cit.*, pp. 43-44). An influence of difference of sensitivity of the two ears is suggested by Arnheim (*Op. cit.*, p. 10, note) to explain his results when working with monaural hearing artificially produced. The left ear was found to have the superior power to localize correctly. He thought this might be due to its better blood supply. Pierce speaks against this part of Arnheim's work (*Op. cit.*, p. 107).

¹*Op. cit.*, pp. 385-390.

approximately equal sensitivity.¹ A method, then, which seeks to vary the intensity of the sound as heard by the two ears, and does not take into account their probable difference in sensitivity, is obviously at fault; for there is no guarantee that a slight difference in the energy of the sound-waves delivered to the two ears, such as was produced by a different setting of the small shell-shaped collectors of Thompson's pseudophone, will be sensed as the relative intensities of the stimuli would indicate. If one ear should be more sensitive than the other, the two sound-waves, although one is stronger than the other, may be sensed as equal in intensity; or the ratio indicated by the stimuli may be reversed. Because, then, of the common occurrence of a natural difference in sensitivity between the ears, a method that attempts to measure the ratio of intensity of the sensations experienced by the ratio of intensity of the stimuli given, does not afford a safe basis for a correlation between the intensity of the sound as heard by the two ears and its localization. (3) Apart from the propriety of method, a third reason for continuing this line of attack is that the results reported from it have been too vague and uncertain to give much support to the intensity theory. For example, (a) the shell-shaped collectors in Thompson's pseudophone were assumed to collect more sound when given one direction than when given another; but there was no objective determination of how much they varied the intensity of the wave impinging upon the tympanum, or whether they varied it at all. No proper basis was laid even for a correlation of ratio of intensity of the two stimuli with the direction in which the sound was referred. (b) The method used for recording Thompson's localizations was indefinite, and his report of results is vague and uncertain. In short, a characteristic displacement of the sound toward the side receiving the more intensive stimulation is expressed (in the paper of 1879) as a matter of belief rather than as an established fact.²

(b) *Description of Method and Apparatus.* The writers were led to make this study by the results of tests they had been conducting on the relative sensitivity of the two ears in different people. The large number of subjects who were found to have a marked difference in sensitivity seemed to make possible a determination of whether or not the hearing of a

¹Fechner, investigating the relative sensitivity of the two ears (*Ueber die ungleiche Deutlichkeit des Gehörs auf linkem und rechtem Ohre*, Berichte der kgl. Sächs. Ges. der Wiss. Math.-phys. Classe, XII, 1860, 166-174), found that out of 215 persons examined only 51 had ears of approximately equal sensitivity.

²*Op. cit.*, pp. 388-390.

sound more strongly by one ear than the other leads to constant errors in localization. Assuming that the binaural ratio is a factor in localizing, there seem *a priori* to be two possibilities relative to this question. (a) The subject so affected may, in proportion to his defect, show a constant tendency to displace the sound towards the aural axis on the side of the more sensitive ear, or (b) this tendency may have been wholly or in part corrected in the subject's past experience, through the influence of the space reference of other sense organs, in such a fashion that the false ratio has shown a tendency to become associated with the true reference. If so, the amount of the constant error probably should sustain some ratio to the length of time the defect had existed. If, for example, it were congenital or contracted very early in life, one might expect less error in localization than if it were of recent occurrence. Unfortunately the subjects, up to the time of these tests, were not aware of their defect, consequently no data of the sort were available. The effect of recency of defect, however, came out strongly in the experiments in which the ratio was varied by artificial means. Differences in sensitivity, artificially produced, exerted a much more marked influence upon localization than did approximately equal differences due to natural defect.

Artificial variations were produced both upon defective subjects and upon subjects in whom the sensitivity of the two ears was approximately equal. In both cases the effect was marked and consistent. In case of the normal subjects, in turn first one ear was made more sensitive, then the other. In case of the defective subjects three variations were introduced. (a) The defect was exaggerated, *i. e.*, the difference in sensitivity was rendered greater by plugging the less sensitive ear.¹ (b) An effort was made to correct the defect by decreasing the sensitivity of the stronger ear. Our object here was to establish a ratio of sensitivity that should eliminate any approach to a constant tendency to displace the sound in either direction. This was a procedure involving many trials and much patience. Our first idea was that this result should be attained by equating the sensitivity of the two ears. This device, however, in case of the subjects

¹In all cases of plugging one ear, care was taken that monaural hearing was not produced. Before the observation began, both ears were firmly closed by the hands or some other effective means, until the stimulus used in the localizing experiments could no longer be heard, whatever position it might be given in the system used. The plugged ear was then uncovered, and the stimulus given at the most remote positions to be used in the experiment which was to follow. In no case was the observer unable to hear the sound.

used, overshot the mark. When the sensitivity of the stronger ear was decreased until it approximately equalled that of the weaker ear, a constant tendency to displace towards the normally weaker ear resulted. A compromise position then had to be sought. We finally succeeded in getting, with each subject, a ratio such that the error, roughly speaking, was apparently about as much and as frequently to one side as to the other of the true location. (c) A third variation was to plug the stronger ear until it became less sensitive than the weaker ear.

In all of our experiments, in order to guard against a wrong correlation of ratio with localization error, due to possible variations in sensitivity from day to day, or even from the beginning to the end of the experiment, sensitivity determinations were made at each sitting both immediately before and immediately after the localizing tests were made.

The ratio of sensitivity was obtained by comparing the limen of sound for the two ears. The observer was blindfolded and required to bite an impression previously made in a wax mouth-board. A wooden bar was supported in the line of the axis of the two ears, one end reaching as near as possible to the ear that was being tested. The other ear was carefully plugged. A watch was carried out along the bar until the limen was reached. An average of the results obtained by the method of approach and recession was taken as the final liminal distance, and the ratio of these distances was taken to represent the ratio of sensitivity of the two ears. To make sure that the plugged ear was not functioning in these tests, the watch was held as closely as possible to it without touching the lobe and the observer required to tell whether it could be heard or not.

The localizing experiments were carried on by means of the Titchener sound-cage. A Galton whistle set at 20,000 vibrations per second was used for the stimulus. As to devices for indicating the location of the sound, the pointing method, the chart method, and a combination of the two were used at different times. The authors have not made an exhaustive study of the relative merits of these methods but is inclined to prefer, on the basis of what they have done, a careful use of the pointing method alone. Any method involving the use of the chart has, in his experience, fostered a tendency on the part of the observer to delay the reference, to become uncertain and hesitating, to reason and debate with himself rather than to let whatever sensory mechanism for localizing with which nature may have provided him work itself out automatically. The errors arising from this tendency are, in our opinion, greater and more capricious than those from

wrong pointing, if proper care is exercised to make sure that the observer is pointing as he intends to point. The question of methods of recording, however, makes little difference for or against the validity of our demonstration; for (a) the constant displacement tendency appears whatever the method used, and (b) no method, however comprehensive its faults, could account for the consistent throw in opposite directions in case of different observers, or in the case of the same observer, when first one ear then the other is made more sensitive.

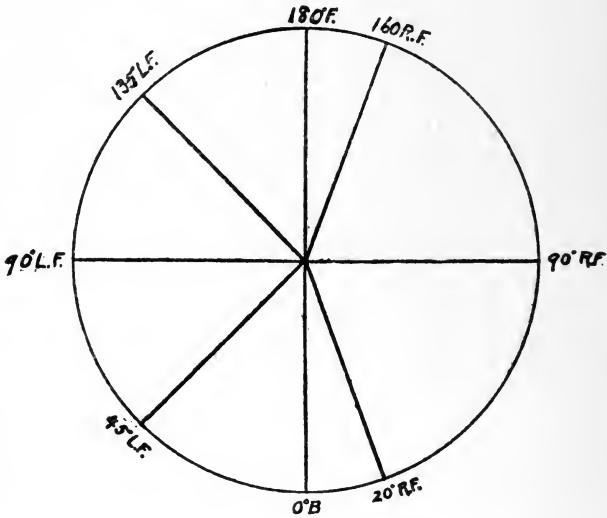


FIG. III

0° B.	180° F.
45° R. F. or L. F.	135° R. F. or L. F.
50° R. F. or L. F.	130° R. F. or L. F.
60° R. F. or L. F.	120° R. F. or L. F.
70° R. F. or L. F.	110° R. F. or L. F.
150° R. F. or L. F.	30° R. F. or L. F.
160° R. F. or L. F.	20° R. F. or L. F.

Thus far results have been obtained from ten observers in the investigation proper. In addition, the writers have roughly used, at one time or another for two years, all of the variations as a part of the drill course in his undergraduate laboratory.

(c) *Results.* The following tables have been compiled from the results of three of these observers, who were selected as representative: the Misses Friend (F.), Root (R.), and

Sharp (S.). Tables I-XII, inclusive, show the results of the experiments devised to demonstrate the importance of the binaural ratio as a factor in auditory localization. In all the tables given throughout the paper, locations in the horizontal plane are expressed in terms of the readings of the Titchener sound-cage. In this system of reference, the zero point is placed in the median plane, behind; the 90° points in the aural axis, right and left; and the 180° point in the median plane in front. It was found more convenient, however, in the vertical planes, to deviate from the scale of the sound-cage; the zero point was taken in the plane of the aural axis, and directions were read 90° up and down. Displacements to right or left were estimated from the actual position of the stimulus, or from its corresponding point front or back, as the case happened to be. For example, a stimulus given at 160° right front (R. F.) might be referred by the observer either to 160° R. F. or to its corresponding point, 20° R. F., without the reference being considered a displacement toward either ear. But if a stimulus were given at 160° R. F. and

TABLE I

Observer F. Showing the influence of the binaural ratio upon the localization of clangs. Natural sensitivity series. Liminal distance for right ear, 40 cm.; for left ear, 40 cm. Ratio, Left: Right = 1. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of right ear	Displacement toward axis of left ear
Horizontal	Vertical	Horizontal	Vertical		
45° R F	0°	50° R F	0° d	5°	
135° L F	0°	50° L F	8° d		5°
0° B	0°	0° B	5° d	0°	0°
135° R F	0°	125° R F	5° u	10°	
45° L F	45° u	50° L F	5° u		5°
180° F	0°	20° L F	0°		20°
135° L F	45° u	40° L F	20° d	5°	
45° L F	45° d	50° L F	40° d		5°
150° R F	0°	145° R F	0°	5°	
0° B	45° u	10° R F	0°	10°	
45° R F	45° u	50° R F	0°	5°	
180° F	45° u	0° B	40° u	0°	0°
135° R F	45° u	0° B	0°	0°	0°
150° L F	45° u	135° R F	30° u	0°	0°
0° B	90° u	35° L F	25° u		5°
70° L F	0°	20° R F	20° u	20°	
50° R F	45° d	70° L F	0°	0°	0°
70° R F	0°	120° R F	5° d	10°	
0° B	0°	80° R F	0°	10°	
		0° B	0°	0°	0°

Average displacement from median plane 2.1° (right).

were referred either to 140° R. F. or to 40° R. F., the reference would be considered a displacement toward the axis of the right ear. Fig. 3 shows a diagram of the horizontal plane of reference, with two pairs of corresponding points represented. Below it are given readings in degrees for the corresponding points used in the experiments.

Tables I, II, and III show the results for the natural difference in sensitivity of the two ears for Observers *F.*, *R.*, and *S.*, respectively. In each case, the liminal distance for the

TABLE II

Observer *R.* Showing the influence of the binaural ratio on the localization of clangs. Natural sensitivity series. Liminal distance: right ear, 18.5 cm.; left ear, 72 cm. Ratio, Left : Right = 4. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
45° R F	0°	25° R F	30° d	20°	
135° L F	0°	50° L F	30° d	5°	
0° B	0°	15° L F	50° d	15°	
135° R F	0°	40° R F	38° d	5°	
45° L F	45° u	60° L F	35° d	15°	
180° F	0°	35° L F	20° d	35°	
135° L F	45° u	70° L F	38° d	25°	
45° L F	45° d	55° L F	33° d	10°	
150° R F	0°	40° R F	42° d		10°
0° B	45° u	35° L F	35° d	35°	
45° R F	45° u	10° R F	45° d	35°	
180° F	45° u	30° L F	42° d	30°	
135° R F	45° u	15° R F	45° d	30°	
150° L F	45° u	40° L F	35° d	10°	
0° B	90° u	50° L F	10° d	50°	
70° L F	0°	70° L F	10° d	0°	0°
50° R F	45° d	15° R F	50° d	35°	
70° R F	0°	50° R F	38° d	20°	

Average displacement toward axis of stronger ear, 20.3°

ticking of a watch was determined for each ear both before and after the observations, and the ratio of sensitivity was computed from these distances. For observer *F.* this distance was found to be 40 cm. for each ear. Estimated in terms of these distances, then, the observer's ears were approximately equal in sensitivity. As the tables show, the ears of observers *R.* and *S.* were found to be of unequal sensitivity. It will be noted that a rough correspondence holds in each case between the ratio of sensitivity of the ears, and the observer's characteristic localizing tendency.

TABLE III

Observer S. Showing the influence of the binaural ratio on the localization of clangs. Natural sensitivity series. Liminal distance: right ear, 97 cm.; left ear, 33 cm. Ratio, Right : Left = 2.9. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
135° R F	0°	5° R F	5° d		40°
45° L F	0°	30° L F	0°	15°	
150° L F	45° d	10° L F	0°	20°	
0° B	45° u	0° B	45° u	0°	0°
180° F	45° d	5° L F	35° d		5°
45° R F	45° u	50° R F	35° u	5°	
135° R F	45° u	45° R F	52° u	0°	0°
160° L F	0°	0°	0°	20°	
0° B	90° u	30° R F	12° u	30°	
70° L F	0°	65° L F	0°	5°	
135° L F	0°	25° L F	0°	20°	
45° R F	45° d	10° R F	15° d		35°
0° B	90° u	25° R F	12° u	25°	
60° R F	0°	70° R F	18° u	10°	
150° R F	0°	50° R F	15° u	20°	
45° L F	45° u	0° B	2° d	45°	
60° L F	45° u	5° L F	30° u	55°	
160° R F	0°	25° R F	10° u	5°	
60° L F	45° d	40° L F	10° d	20°	
0° B	0°	0° B	5° d	0°	0°

Average displacement toward axis of stronger ear, 10.7°.

Tables IV and V show the effect upon localization for observers R. and S., produced by exaggerating the natural ratio of sensitivity of the two ears by plugging the weaker ear. It will be noticed that in each case the observer's tendency to displace the sound toward the axis on the side of the stronger ear is increased. For example, with the natural difference in sensitivity, Observer R.'s average displacement toward the axis on the side of the stronger ear was 20.3° (Table II); with the exaggeration of the natural difference, the average displacement became 31.6° (Table IV). For Observer S., the average displacement toward the axis on the side of the stronger ear, with the natural difference in sensitivity, was 10.7° (Table III); with the exaggeration of this difference, the displacement was increased to 17.2° (Table V).

Tables VI and VII show the effect upon localization, for Observers R. and S., produced by plugging the stronger ear until it became less sensitive than the weaker ear. The result in each case was to change the characteristic displacement to the opposite side—now the side of the stronger ear. For

example, for Observer *R.*, when the ratio was changed from left:right=4, to right:left=6.2, the average displacement of 20.3° toward the axis on the side of the left ear was changed to an average displacement on the side of the right ear of 34.4° . And for Observer *S.*, when the ratio right:left = 2.9 was changed to left:right = 3.8, the average displacement was changed from 10.7° toward the right to 34.6° toward the left.

TABLE IV

Observer *R.* Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Right ear plugged. Liminal distance: right ear, 3 cm.; left ear, 71 cm. Ratio, Left : Right = 23.6. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	30° L F	40° d	30°	
45° R F	0°	10° L F	35° d	55°	
180° F	0°	60° L F	10° d	60°	
135° L F	0°	55° L F	15° d	10°	
60° L F	0°	70° L F	15° d	10°	
0° B	45° u	40° L F	48° d	40°	
135° R F	0°	45° R F	40° d	0°	0°
180° F	45° u	40° L F	50° d	40°	
45° L F	45° d	55° L F	12° d	10°	
135° R F	45° d	40° R F	43° d	5°	
0° B	45° d	35° L F	25° d	35°	
180° F	45° d	30° L F	15° d	30°	
45° R F	45° u	25° L F	45° d	70°	
135° L F	45° u	50° L F	15° d	5°	
45° L F	45° u	60° L F	40° d	15°	
135° R F	45° u	15° L F	32° d	60°	
0° B	90° u	15° L F	15° d	15°	
45° R F	45° d	35° L F	18° d	80°	

Average displacement toward axis of stronger ear, 31.6° .

Tables VIII and IX show the effect upon localization made by producing artificial differences in the sensitivity of the ears for Observer *F.*, whose natural sensitivity is approximately equal for both ears. When the left ear was plugged until the ratio right:left=4.3 was obtained, the average displacement toward the axis on the side of the right ear was found to be 17° . And when the right ear was plugged until the ratio left:right = 4.1 was obtained, the average displacement toward the side of the left ear was found to be 18.2° . It will be remembered from Table I that the average of the localizations for this observer with natural hearing showed a displacement toward the right of 2.1° .

TABLE V

Observer S. Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Left ear plugged. Liminal distance: right ear, 97 cm.; left ear, 10 cm. Ratio, Right : Left = 9.7. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	45° R F	0°	45°	
45° R F	0°	35° R F	0°		10°
135° L F	0°	5° L F	7° d	40°	
180° F	0°	10° R F	8° d	10°	
135° R F	0°	20° R F	0°		25°
45° L F	45° u	5° L F	14° d	40°	
0° B	45° u	50° R F	15° u	50°	
135° R F	45° u	40° R F	8° u		5°
135° L F	45° u	25° L F	12° u	20°	
180° F	45° u	30° R F	20° u	30°	
45° R F	45° u	52° R F	30° u	7°	
45° L F	45° d	35° L F	8° d	10°	
0° B	45° d	20° R F	8° u	20°	
45° R F	45° d	60° R F	15° u	15°	
135° L F	45° d	45° L F	30° u	0°	0°
180° F	45° d	20° R F	15° u	20°	
135° R F	45° d	45° R F	10° u	0°	0°
0° B	90° u	40° R F	10° u	40°	
60° L F	0°	40° L F	10° u	20°	

Average displacement toward axis of stronger ear, 17.2°.

Table X shows the effect upon localization, in the case of Observer R., of an attempt to equate the sensitivity of the ears. The natural ratio of sensitivity for this observer (Table II) was left:right = 4. When this was changed by plugging the left ear until the ratio right:left = 1.02 (18.5 ÷ 18.2) was obtained, there resulted an average displacement 31.4° toward the axis on the side of the right ear. Results of this kind were obtained for all observers. A characteristic tendency to displace the sound to right or left cannot be corrected by equating the ratio of sensitivity of the ears. A value must be obtained somewhere between the natural ratio and equal sensitivity.¹

¹That the effect of equating the sensitivity should overshoot the mark is not at all strange. If one ear naturally hears more loudly than the other, an equal intensity of sensation has never been associated in the observer's experience with objects in the median plane, but always with some position displaced from the median plane toward the naturally weaker ear. Then when the stronger ear is plugged until it is of the same sensitivity as the weaker ear, sounds which can be heard as equally loud by both ears, *i. e.*, sounds coming from the median plane, will not be referred to that plane,

TABLE VI

Observer R. Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Left ear plugged. Liminal distance: right ear, 18.5 cm.; left ear, 3 cm. Ratio, Right : Left = 6.2. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
45° R F	0°	65° R F	0°	20°	
0° B	90° u	15° R F	42° d	15°	
180° F	45° d	25° R F	40° d	25°	
60° L F	0°	25° R F	40° d	85°	
135° R F	0°	40° R F	25° d		5°
45° R F	45° d	45° R F	18° d	0°	0°
135° L F	45° d	4° R F	40° d	49°	
0° B	45° d	30° R F	32° d	30°	
45° L F	45° d	55° R F	20° d	100°	
135° L F	45° u	5° R F	35° d	50°	
135° R F	0°	40° R F	20° d		5°
45° R F	45° u	50° R F	10° d	5°	
180° F	45° u	25° R F	38° d	25°	
45° L F	45° u	45° R F	42° d	90°	
135° R F	45° u	45° R F	45° d	0°	0°
0° B	45° u	45° R F	20° d	45°	
180° F	0°	25° R F	38° d	25°	
135° L F	0°	5° R F	28° d	50°	
0° B	0°	50° R F	40° d	50°	

Average displacement toward axis of stronger ear, 34.4°.

Tables XI and XII show the results of fairly successful attempts to correct this observer's tendency to displace sounds towards the left. In Table XII with a ratio left:right = 1.5 we find an average displacement of 2.1° towards the right; in Table XI with a ratio left:right = 2.1 we find an average displacement of 1.4° towards the left. The former tables thus show over-correction; the latter, under-correction.

B. THE RELATIVE IMPORTANCE OF INTENSITY AND TIMBRE AS FACTORS IN LOCALIZATION

In these experiments, tuning forks were used as the source of sound. The object was to find out to what extent the con-

but will be displaced toward the axis on the side of the weaker ear, because sounds of equal intensity have always had that connotation in the observer's past experience. Likewise, when the ears have been made equally sensitive by plugging the stronger, sounds which come from positions to either side of the median plane will always be displaced toward the naturally weaker ear, because now they are heard by the two ears with a relative loudness which, in the observer's past experience, has always connoted a position relatively nearer the weaker ear.

TABLE VII

Observer S. Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Right ear plugged. Liminal distance: right ear, 18 cm.; left ear, 70 cm. Ratio, Left : Right = 3.8. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	30° L F	5° d	30°	
180° F	0°	30° L F	20° d	30°	
45° R F	0°	10° L F	5° d	55°	
45° L F	45° u	60° L F	5° u	15°	
0° B	45° u	33° L F	22° d	33°	
135° R F	0°	20° L F	5° d	65°	
135° L F	0°	50° L F	15° d	5°	
180° F	45° u	30° L F	0°	30°	
135° R F	45° u	20° L F	18° u	65°	
135° L F	45° u	65° L F	5° u	20°	
0° B	45° d	25° L F	0°	25°	
45° R F	45° u	35° L F	10° u	80°	
45° L F	45° d	50° L F	15° u	5°	
60° L F	0°	6° L F	5° u		54°
45° R F	45° d	30° L F	12° d	75°	
180° F	45° d	40° L F	12° d	40°	
135° R F	45° d	30° L F	5° d	75°	
0° B	90° u	30° L F	5° d	30°	

Average displacement toward axis of stronger ear, 34.6°.

ditions obtaining in the former experiments influence the localization of simple tones. Three cases are possible. (a) These conditions may exert no influence at all. We should then have to conclude that, in the former experiments, the binaural ratio produces its effect wholly as a difference in the timbre of the sound as heard by the two ears. That is, since timbre depends upon the number and the proportionate strength of the overtones in the clang, in case one ear is more sensitive than the other, the timbre of the sound heard by one ear will differ from that heard by the other ear because of the different number of overtones present in the two cases.¹ (b) The conditions may exert some influence, but not

¹This view of the way the binaural ratio serves as a localizing clue was first advanced by Rayleigh in 1876, and later by Sylvanus Thompson (*Op. cit.*, p. 415) in 1882. Thompson says: "Judgments as to the direction of sound are based, in general, upon the sensations of different intensity in the two ears, but the perceived difference of intensity upon which a judgment is based is not usually the difference in intensity in the lowest or fundamental tone of the compound (or 'clang'), but upon the difference in intensity of the individual tone or tones of the clang for which the intensity difference has the greatest effective result in the quality of the sound It is completely open to doubt whether a pure simple tone heard in one ear could suggest any direction at all."

as much as was exerted upon the sound of the Galton whistle. In this case, we should have to conclude that differences of intensity both in the fundamental and in the overtones of the clang served as a localizing clue in our experiments. (c) They may exert an equal influence upon the sound of the tuning fork and upon the clang. This would indicate that differences in the intensity of the fundamental tone alone were operative as local signature.

TABLE VIII.

Observer *F.* Showing the influence of the binaural ratio on the localization of clangs, Artificial sensitivity series. Left ear plugged. Liminal distance: right ear, 39 cm.; left ear, 8 cm. Ratio, Right : Left = 4.9. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
45° R F	0°	70° R F	8° u	30°	
135° L F	0°	35° L F	0°	10°	
0° B	0°	35° R F	0°	35°	
135° R F	0°	50° R F	0°	5°	
45° L F	45° u	50° L F	0°		5°
180° F	0°	0° B	0°	0°	0°
135° L F	45° u	45° L F	30° d	0°	0°
45° L F	45° d	0° B	30° d	45°	
150° R F	0°	30° R F	5° d	0°	0°
0° B	45° u	10° R F	10° d	10°	
45° R F	45° u	45° R F	20° u	0°	0°
180° F	45° u	155° R F	10° u	25°	
135° R F	45° u	170° R F	20° u		35°
150° L F	45° u	35° L F	0°		5°
0° B	90° u	25° R F	0°	25°	
45° L F	0°	0° B	20° d	45°	
0° B	45° d	40° R F	10° d	40°	
180° F	0°	15° R F	10° u	15°	

Average displacement toward axis of stronger ear, 17°.

The stimulus was given as follows in these experiments. The observer, blindfolded, with head firmly clamped and ears tightly closed, sat in position in the sound-cage. A heavy unmounted tuning fork of 480 vibrations per second and a cylindrical resonator were used as the source of sound. These were substituted for the telephone receiver of the sound-cage. The fork was plucked by the fingers covered by a chamois glove, and was allowed to vibrate for a few seconds to allow possible high overtones, harmonic or inharmonic, to die out. It was then held over the mouth of the resonator.

As soon as the tone became audible, the observer's ears were uncovered and the sound was listened to for about one second, at the end of which time the fork was removed from the mouth of the resonator, and the direction in which the sound was heard was indicated by the observer. In no case were any of the noises attendant upon the stimulation of the fork heard; and a tone as simple as a tuning fork is capable of giving was obtained. The duration of the stimulus was roughly the same

TABLE IX

Observer *F.* Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Right ear plugged. Liminal distance: right ear, 10 cm.; left ear, 41 cm. Ratio, Left: Right=4.1. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
45° R F	0°	40° R F	0°	5°	
135° L F	0°	110° L F	0°	25°	
0° B	0°	15° L F	10° u	15°	
135° R F	0°	15° R F	20° u	30°	
45° L F	45° u	60° L F	5° u	15°	
180° F	0°	50° L F	25° u	50°	
135° L F	45° u	90° L F	35° u	45°	
45° L F	45° d	55° L F	10° d	10°	
150° R F	0°	120° R F	18° u		30°
0° B	45° u	25° L F	30° u	25°	
45° R F	45° u	25° R F	30° u	20°	
180° F	45° u	35° L F	32° u	35°	
135° R F	45° u	15° R F	35° u	30°	
150° L F	45° u	90° L F	5° u	60°	
0° B	90° u	5° R F	20° u		5°
70° L F	0°	75° L F	0°	5°	
50° R F	45° d	120° R F	15° u		10°
70° R F	0°	30° R F	15° u	40°	

Average displacement toward axis of stronger ear, 18.2°.

as that of the Galton whistle used in the earlier experiments, and care was taken to give the stimulus as nearly as possible the same intensity each time. The stimuli were all given at the level of the ears, and no account of vertical displacements was taken in recording the results, since these have no direct bearing upon the purpose of the experiment.

Tables XIII, XIV, and XV give the results of this investigation. These results, on the average, show that the ratio of sensitivity of the two ears affects the localization of simple tones almost, if not quite, as much as it does the localization of clangs of the degree of complexity of the Galton whistle.

TABLE X

Observer R. Showing the influence of the binaural ratio on the localization of clangs. Sensitivity of two ears equated. Left ear plugged. Liminal distance: right ear, 18.5; left ear, 18.2 cm.; Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of right ear	Displacement toward axis of left ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	50° R F	42° d	50°	
180° F	0°	35° R F	30° d	35°	
0° B	45° u	40° R F	30° d	40°	
180° F	45° u	50° R F	8° d	50°	
0° B	45° d	40° R F	25° d	40°	
180° F	45° d	30° R F	25° d	30°	
0° B	90° u	30° R F	42° d	30°	
0° B	0°	35° R F	40° d	35°	
180° F	0°	30° R F	44° d	30°	
0° B	45° u	20° R F	28° d	20°	
180° F	45° u	20° R F	32° d	20°	
0° B	45° d	10° R F	35° d	10°	
180° F	45° d	35° R F	35° d	35°	
0° B	90° u	15° R F	38° d	15°	

Average displacement toward axis of right ear, 31.4°.

TABLE XI

Observer R. Showing the influence of the binaural ratio on the localization of clangs. Attempt to correct localizing error. Left ear plugged. Liminal distance: right ear, 19 cm.; left ear, 40 cm. Ratio, Left : Right = 2.1. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of right ear	Displacement toward axis of left ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	10° R F	28° d	10°	
180° F	0°	20° L F	35° d		20°
0° B	45° u	0° B	32° d	0°	0°
180° F	45° u	10° L F	25° d		10°
0° B	45° d	0° B	35° d	0°	0°
180° F	45° d	5° R F	35° d	5°	
0° B	90° u	5° R F	50° d	5°	
0° B	0°	0° B	32° d	0°	0°
180° F	0°	0° B	20° d	0°	0°
0° B	45° u	10° L F	35° d		10°
180° F	45° u	5° L F	30° d		5°
0° B	45° d	15° R F	32° d	15°	
180° F	45° d	0° B	20° d	0°	
0° B	90° u	10° L F	42° d		10°

Average displacement from median plane, 1.4° (left).

TABLE XII

Observer R. Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Attempt to correct localizing error. Left ear plugged. Liminal distance: right ear, 19 cm.; left ear, 29 cm. Ratio, Left: Right = 1.5. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of right ear	Displacement toward axis of left ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	5° R F	30° d	5°	
180° B F	0°	5° L F	15° d		5°
0° B	45° u	10° R F	30° d	10°	
180° B F	45° u	0° B	30° d	0°	0°
0° B	45° d	0° B	15° d	0°	0°
180° B F	45° d	0° B	15° d	0°	0°
0° B	90° u	0° B	40° d	0°	0°
0° B	0°	5° R F	38° d	5°	
180° B F	0°	5° L F	22° d		5°
0° B	45° u	15° R F	30° d	15°	
180° B F	45° u	0° B	35° d	0°	0°
0° B	45° d	0° B	25° d	0°	0°
180° B F	45° d	0° B	25° d	0°	0°
0° B	90° u	5° R F	40° d	5°	

Average displacement from median plane, 2.1° (right).

But the effect is not nearly so consistent in the individual judgments. When the Galton whistle was used as stimulus, the sound was displaced toward the axis on the side of the stronger ear in a very large percentage of cases, and, relatively speaking, in not widely varying amounts. In the case of the tuning fork, however, a very large displacement of the sound toward the axis on the side of the stronger ear was frequently followed by one toward the axis on the side of the weaker ear, the variation in the individual judgments from the true position being, in general, very much greater than for the Galton whistle. It would appear then, in these experiments, that the binaural ratio has exerted its influence both as difference in intensity and as change of timbre.¹ For the sake of comparison,

¹A few words, further explaining and qualifying the above argument, are probably not out of place here. The tone of the Galton whistle set at 20,000 vibrations is relatively simple. The first overtone, for example, has a vibration rate of 40,000, and the second of 60,000, which is above the limit of audibility. Thus our argument that the above mentioned displacements have been made in terms of the intensity factor should not rest so much, probably, upon a correspondence of results when Galton whistle and tuning fork are used as sources of sound, as upon the fact that the large displacements observed took place both when a simple and a relatively simple tone were used as stimuli. To complete the investigation a comparison should

TABLE XIII

Observer *F.* Showing the influence of the binaural ratio on the localization of simple tones. Liminal distance: right ear, 40 cm.; left ear, 40 cm. Ratio, Right : Left = 1. Stimulus, tuning fork, 480 vibrations per second.

Set	Heard	Displacement toward axis of right ear	Displacement toward axis of left ear
135° L F	70° L F		25°
0° B	0° B	0°	0°
45° L F	60° L F		15°
135° R F	45° L F		90°
60° L F	60° L F	0°	0°
120° L F	120° L F	0°	0°
50° L F	80° L F		30°
180° F	0° B	0°	0°
150° L F	65° L F		35°
0° B	0° B	0°	0°
135° L F	90° R F	135°	
45° R F	45° R F	0°	0°
0° B	125° R F	55°	
60° R F	15° R F		45°
180° F	85° L F		85°
120° R F	60° R F	0°	0°
30° R F	130° R F	20°	
150° R F	135° R F	15°	
0° B	10° R F	10°	
45° R F	90° R F	45°	
180° F	20° L F		20°

Average displacement from median plane, 3.09° (left).

the same observers were used here that were used in the experiments with the Galton whistle. In order to show the effect of the binaural ratio, it was deemed advantageous, in both cases, to work with observers both of equal and of unequal sensitivity of the two ears.

Table XIV shows the results for Observer *R.* The ratio of sensitivity was chosen so that left:right = 2.3; the average displacement toward the axis on the side of the stronger ear was found to be 15°. A correlation of average displacement with ratio of sensitivity shows, roughly speaking, for this observer, quite as much tendency to displace the sound to the side of the stronger ear as was shown for the Galton whistle.

Results with the Galton whistle are brought forward from Tables II, IV, and VI for comparison. Table II shows for *R.*

be made further of the results obtained with these two sources of sound and one still more complex than the Galton whistle. This comparison will be included in the work on this problem still in progress in this laboratory.

a ratio of sensitivity left:right = 4, an average displacement toward left of 20.3° ; Table IV, a ratio of sensitivity left:right = 23.6, an average displacement toward the left of 31.6° ; Table VI, a ratio of sensitivity right:left = 6.2, and an average displacement of 34.4° toward the right.

Table XV shows the results for Observer S., with a ratio of sensitivity chosen so that right:left=1.9. With this ratio it was found that the sound was displaced, on the average, 7.5° toward the side of the stronger ear. When compared with a ratio left:right=2.9 and an average displacement of 10.7° toward the left ear (Table III), a ratio right:left=9.7 and an average displacement toward the right of 17.2° (Table V), and a ratio left:right = 3.8 with an average displacement toward the left of 34.6° (Table VII), these results also show probably as strong a tendency to displace the simple tone toward the stronger ear as was shown in the case of the clang.

TABLE XIV

Observer R. Showing the influence of the binaural ratio on the localization of simple tones. Liminal distance: left ear, 49 cm.; right ear, 21 cm. Ratio, Left : Right = 2.3. Stimulus, tuning fork, 480 vibrations per second.

Set	Heard	displacement toward axis of stronger ear	Displacement toward axis of weaker ear
45° R F	85° R F		4°
135° R F	30° R F	15°	
0° B	40° L F	40°	
60° R F	40° R F	20°	
180° F	0° B	0°	0°
30° R F	55° R F		25°
150° R F	40° L F	70°	
0° B	55° R F		55°
45° R F	55° R F		10°
135° R F	50° L F	95°	
0° B	85° L F	85°	
135° L F	35° L F		10°
0° B	35° R F		35°
45° L F	75° L F	30°	
0° B	50° L F	50°	
120° L F	55° L F		5°
50° L F	55° L F	5°	
180° F	60° L F	60°	
150° L F	40° R F		70°
0° B	45° L F	45°	
135° L F	70° L F	25°	
45° L F	85° L F	40°	

▲ Average displacement toward axis of stronger ear, 15° .

TABLE XV

Observer S. Showing the influence of the binaural ratio on the localization of simple tones. Liminal distance: right ear, 86 cm.; left ear, 46 cm. Ratio, Right : Left = 1.9. Stimulus, tuning fork, 480 vibrations per second.

Set	Heard	Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
135° L F	35° R F	80°	
0° B	25° L F		25°
45° L F	35° L F	10°	
0° B	40° L F		40°
60° L F	70° L F		10°
120° L F	40° R F	100°	
50° L F	65° L F		15°
180° F	45° R F	45°	
135° R F	0° B		45°
150° L F	10° L F	20°	
0° B	50° R F	50°	
180° F	10° L F		10°
135° L F	120° R F	105°	
45° L F	75° L F		30°
45° R F	10° R F	25°	
0° B	70° L F		70°
60° R F	75° R F	15°	
0° B	15° L F		15°
180° F	50° R F	50°	
120° R F	45° R F		15°
30° R F	60° R F	30°	
45° R F	45° L F		90°

Average displacement toward axis of stronger ear, 7.5°

C. INDIVIDUAL PREFERENCES

Of the individual preferences reported by von Kries¹ and Dunlap,² the writers find this much evidence. There is (1) a

¹The individual preferences mentioned by von Kries (*Ueber das Erkennen der Schallrichtung*, Ztschr. f. Psychol., I, 1890, s. 242-243) are confined to points in the median plane. The results obtained by us bear more specifically upon the preferences reported by Dunlap.

²Dunlap (*The Localization of Sounds*, Psychol. Rev. Monog. Suppl., 1909, Vol. X, No. 40, 1-16) says: "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 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 comparisons 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." The condition

tendency in case of a stronger ear, to refer the sound in the direction of this ear; and (2) in the case of his observers, and the relatively weak stimulus used, there seemed to be a fairly consistent tendency to prefer the back to the front locations; in fact, some observers never located a sound in front. The former tendency gave certain observers a decided right or left "preference," depending upon their defect; and this, combined with the second tendency, tended to limit the localizations to a single quadrant. That is, the back tendency operating to limit the sound to one hemisphere, and the right or left tendency to a hemisphere at right angles to this, would tend to confine the localizations to one quadrant for a given observer. But these tendencies can hardly be called capricious, as Dunlap apparently found them to be. To show that the one conforms to law, *i. e.*, is correlated with a definite sensory characteristic, has been the object of this paper. The other is still under investigation.¹

which discouraged further work on his problem may be summed up in his own words: "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 up in a box and dumped down on 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. 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. 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 produced no definite alterations in the preferred area. The Galton whistle gives practically the same results as the buzzer or the telephone receiver. So far I have not found a subject who does not localize in this preferential way. What the causes are, I cannot say. There are possible theories and nothing more. Meantime, how to conduct profitable experiments in localization before solving this problem is another problem."

The writers admit that the irregularity of Dunlap's results is discouraging. After a careful study of Dunlap's charts of results, they also admit that the factors underlying the evidences of individual preferences of their own observers fail utterly to solve "the puzzle." They can only repeat that in their contention for a lawful mechanism, they do not wish to go beyond the results and conditions of their own experiments; and merely suggest that Dunlap may have worked with the subjective type of observer, and may have fostered this subjective tendency by the use of the chart method for indicating directions.

¹It has been discussed at various times in the literature of auditory localization whether back reference may not have become associated with weak intensities of sound. (Thompson: *The Pseudophone*, Phil. Mag. (5) VIII, 1879, 385-390; Bloch: *Das binaurale Hören*. Wiesbaden, 1893, pp. 52-6; Pierce: *Op. cit.* 90-1. A reason for this association has been found in the shape of the external ear as a collector of sound (Pierce: *Op. cit.*, p. 90; Bloch: *Op. cit.*, pp. 25-52). But apart from the cause the writers are investigating the fact in the following way. Observers are selected who,

Moreover, no variation that could be called a consistent change occurred in these tendencies after a lapse of some four months. Certainly nothing has come out with regard to the right or left tendency, from time to time, that cannot be roughly correlated with the results of the accompanying sensitivity tests, as will be shown by the results given in the next section of this paper.

However, in contending for a lawful mechanism, and in suggesting an explanation, of what, on the surface, might be considered as capricious, the writer has no desire to go beyond the results and conditions of his own experiments. Each case must be tried out on its own merits.

Table XVI shows the preference for the back locations. This table was compiled from the results of Tables I-XV inclusive and from Table XVII. The number of readings given back and in front of the aural axis, the number of times the sound was localized in the correct hemisphere and the number of times it was displaced to the opposite hemisphere was determined from the tables; and the ratio of the number of backward displacements to the number of forward displacements was computed from these results.

with the comparatively weak stimulus used in the preceding experiments, show a marked tendency to locate the sound behind. A graded series of stimuli is then provided, ranging from very weak to very strong, and the regular localizing series is given for each stimulus. If, with an increase in the intensity of the stimulus, there is found to be a decrease in the percentage of back references, the intensity of the stimulus may fairly be said to sustain an associative relation to this direction reference. This method of procedure, the writers believe, offers better possibilities of getting results from which conclusions can be drawn than do the experiments of the kind performed by Bloch, even though Bloch's principle of working be carried out under laboratory conditions. Bloch selected a sound of such intensity that when it was given behind, it was localized behind by his observers; and then he tried the effect of an increase of intensity. He claimed thus to be able to cause a reversal of the localization, or the illusion of front. Bloch's method of experimenting was extremely crude. The experiments were made in the open air in a court enclosed on three sides. "The observer stood 5 meters from the end wall and pebbles were thrown on the stone pavement in front or behind him. The result was that when his face was turned towards the wall, the legitimate influence of the pinna was merely increased and the localizations were mostly correct. When, on the contrary, the back was turned towards the wall, sounds coming from behind were apt to be falsely located in front, since now the reflection of the sound waves by the wall produced an unwonted intensity in the sound." Applying this principle of working under laboratory conditions in various ways, the writers have always failed to get anything like consistent results. Any attempt to confirm the association of the back or front reference with the intensity of the sound, based upon individual judgments, they believe is doomed to failure. If conclusions are to be reached at all they must be reached from a comparison of averages got by a systematic variation of intensity.

TABLE XVI

Showing, with the comparatively weak stimulus used, the preference of our observers for back locations.

Tables from which data are taken	No. of readings given back of aural axis	No. localized in correct hemisphere	No. displaced in front of aural axis	No. of readings given in front of aural axis	No. localized in correct hemisphere	No. displaced back of aural axis	Ratio of displacement back to displacement front
Table I	11	10	1	8	3	5	5 : 1
Table II	10	10	0	8	0	8	8 : 0
Table III	12	12	0	8	0	8	8 : 0
Table IV	10	10	0	8	0	8	8 : 0
Table V	10	10	0	9	0	9	9 : 0
Table VI	10	10	0	9	0	9	9 : 0
Table VII	10	10	0	8	0	8	8 : 0
Table VIII	9	9	0	9	2	7	7 : 0
Table IX	10	9	1	8	4	4	4 : 1
Table X	8	8	0	6	0	6	6 : 0
Table XI	8	8	0	6	0	6	6 : 0
Table XII	8	8	0	6	0	6	6 : 0
Table XIII	11	9	2	10	3	7	7 : 2
Table XIV	13	13	0	9	0	9	9 : 0
Table XV	13	13	0	9	1	8	8 : 0
Table XVII	9	9	0	8	0	8	8 : 0

D. THE QUESTION OF CHANGES IN THESE PREFERENCES WITH LAPSE OF TIME

Experiments were conducted to find out whether any considerable change occurred in the observer's tendency to localize during the course of several months, or, more especially, to determine whether there occurred any change that could not be correlated with a corresponding change in the ratio of sensitivity of the ears. No change of any significance was found to have taken place in any of the cases examined. Table XVII shows the results obtained for observer R. three months later than those given in Table II. These results may be taken as representative. In Table II, the ratio left:right = 4, and the displacement is 20.3° toward the left; and in the table given below, the ratio left:right = 3.7, and the displacement is 23.3° . This comparison shows a slight increase in the observer's tendency to refer the sound to the side of the stronger ear, but in a field where the results show such a large mean variation the writers have not con-

sidered, either here or elsewhere in the work, that so small a change in results is at all significant.

By comparing the results of Table XVII with Tables II, VI, VII, X, XI, XII, and XIV, in Table XVI, it will be found also that no significant change has occurred in the observer's preference for the back locations.

TABLE XVII

Observer R. Showing that no change of any consequence has taken place in the localizing tendency of our observers after a lapse of three months. (Compare with Table II.) Liminal distance: right ear, 19 cm.; left ear, 71 cm. Ratio, Left:Right=3.7. Stimulus, Galton whistle, 20,000 vibrations per second.

Set	Heard	Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
0° B	25° L F	25°	0°
180° F	50° L F	50°	
45° L F	65° L F	20°	
130° L F	65° L F	15°	
70° L F	70° L F	0°	
135° L F	80° L F	35°	
0° B	15° L F	15°	
135° R F	15° R F	30°	
45° R F	25° R F	20°	
150° R F	20° R F	10°	
60° L F	75° L F	15°	
135° R F	10° R F	35°	
60° R F	10° R F	50°	
50° L F	60° L F	10°	
130° R F	25° R F	25°	
180° F	20° L F	20°	
0° B	20° L F	20°	

Average displacement toward axis of stronger ear, 23.2°.

III. SUMMARY OF RESULTS.

(1) Subjects having a natural difference in the sensitivity of the two ears show a constant tendency to displace the sound toward the axis on the side of the stronger ear; and, conversely, subjects without this difference in sensitivity do not show this tendency. The greater number of subjects examined showed a difference in sensitivity.

(2) Changes in the ratio of sensitivity, produced by plugging either ear, were followed by corresponding displacements of the sound toward the axis on the side of the stronger ear. Differences in sensitivity, artificially produced, apparently

exerted a greater influence upon localization than did approximately equal differences due to natural defect. This is probably because, in the case of a natural defect, the localization error has been partly corrected, in the past experience of the subject, through association with the direction reference of other sense-organs.

(3) In the case of observers who showed a characteristic right or left tendency, it was found possible to change the ratio of sensitivity so that the error in localization was corrected. This result was not accomplished by equating the sensitivity of the two ears. The desired ratio was always found to have a value somewhere between equal sensitivity and the old ratio.

(4) The average results showed that changes in the binaural ratio affected the localization of simple tones almost, if not quite, as much as it did the localization of clangs of the degree of complexity of the Galton whistle. The individual judgments, however, showed a much larger variation from the true position in the case of the simple tones. It would appear, then, that in these experiments the binaural ratio exerted its influence both as difference in intensity and as change of timbre, but predominantly as difference in intensity.

(5) The writers find this much evidence of individual preferences in localization (von Kries', Dunlap). (a) There is a tendency in case of a stronger ear to refer the sound in the direction of that ear. This gave certain observers a decided right or left tendency, depending upon the kind and amount of their defect. (b) With the relatively weak stimulus used, there seemed to be a fairly constant tendency for the observers to prefer back to front locations. But these tendencies cannot in any sense be called capricious. One is directly traceable to the binaural ratio; the other is still under investigation, and is probably an effect of the intensity of the stimulus used.

(6) No changes of any consequence in these tendencies were found during the course of several months, as occurred in the case of Dunlap's observers,—certainly none that could not be correlated with a definite change in the localizing clue. For example, a cold, or what not, was sometimes found to produce a change in the observer's right or left tendency, but tests of the sensitivity of the two ears always disclosed a corresponding change in the binaural ratio.

In this study, nothing was undertaken bearing upon the later aspects of the intensity theory brought out by the papers of Rayleigh¹ and Wilson and Myers.² The writers, however,

¹Rayleigh: *On our Perception of Sound Direction*, Philos. Mag. (6), XIII, 1907, pp. 214-32.

²*Op. cit.*

have begun experiments upon three points relative to these aspects. (1) It will be determined whether tones of 128 vibrations or less per second have a larger j. n. d. of direction than tones of higher pitch. This the intensity theory would seem to require, according to Rayleigh's calculations of the relative intensity of the waves received by the two ears. Rayleigh's tests of this point were as rough as possible. They consisted, it will be remembered, in determining whether stimuli of both low and high pitch, given in the region of the aural axis, could be judged as right or left without mistake. Now, working under these conditions, a considerable difference in direction-sensitivity might obtain for the two kinds of tones, and still no mistake be made in either case. The positions chosen give the largest possible binaural ratio, and the judgments required are the most general that could possibly be made. In short, a less sensitive method for detecting small differences in power to discriminate direction could hardly have been devised. The size of the j. n. d. is obviously the proper criterion to apply. (2) The series of experiments used in this paper will be repeated, using forks of low and high frequency. If there is found as much tendency to displace the tones of low pitch as those of high pitch, the results should argue that the localizing clue for low tones is the binaural ratio, instead of the power directly to detect phase differences; because (a) the change in the sensitivity of the ear does not affect the phase of the sound wave, and (b) it could not affect the detection of the phase differences by the ear in such a manner as to displace the sound toward the stronger ear, for by this hypothesis, ratio of effect has nothing to do with localization. At least, it cannot be assumed that the binaural ratio, which is computed in terms of intensive difference, could be translated directly into terms of recognition of phase difference. Apparently the only effect that could follow a decrease of sensitivity of one ear would be a proportionate confusion and uncertainty of localization, not a definite and characteristic displacement toward the axis on the side of the stronger ear. (3) The settings given to the stimuli in Wilson and Myers's experiments will be repeated under ordinary localizing conditions, in order to see whether the transfer of the sound from one side of the median plane to the other takes place when the direct paths of transmission to the two ears are changed by the amounts they used. If the transfers do not take place, some evidence, at least, will be afforded that the experiments they describe and the conclusions they reach do not bear directly upon the phenomenon of localization as it ordinarily occurs, but only upon a special

phenomenon created by their conditions, which favored bone conduction.

The writers present this report with the hope that their results establish a more definite correlation between the binaural ratio and direction-reference than has previously been attained, and that the experiments described will provide an easily available means of clearly demonstrating this correlation in the teaching laboratory.

A REPLY TO PROFESSOR SAFFORD

By F. M. URBAN, University of Pennsylvania

Professor Safford published in the January number of this *Journal* a little note containing a criticism of my theory of psychophysical measurements. His objections are two in number. The first refers to the number of decimals which have been retained in my tables; he is of the opinion that the computation should have been carried to the third decimal only rather than to the fourth. The second objection is of a more complicated nature, and refers to my use of Lagrange's formula. Professor Safford's ideas are very interesting and I am glad to have an opportunity to explain some considerations, at which my original articles merely hint. My book would have become very voluminous, had I undertaken to present all the chains of reasoning which I later found to be wrong, or to describe all the considerations which decided me to adopt a certain manner of procedure. Both of Professor Safford's objections occurred to me in the course of working out my data, and I may be allowed to state the reasons why I believe that they are erroneous.

Before answering the first objection I want to say that in computing data of this sort it is customary to express one's results in four places of decimals. The majority of statistical investigations, however, do not use relative frequencies, as I did, but percentages calculated to the second decimal. Percentages are found from relative frequencies by multiplying by 100; two decimals in percentages, therefore, correspond to four decimals in relative frequencies. Saying that an event has the relative frequency 0.4422 is the same as saying that this event occurs in 44.22 per cent. of the cases. The latter form is, perhaps a little more familiar to the eye; but relative frequencies have the advantage over percentages of being the more primitive notions.

The relative frequencies of the different judgments form the starting-point of my exposition of the theory of psychophysical measurements; and I naturally gave much thought to the question of how many decimals should be retained. The mere physical labor of carrying out the computations—including the unavoidable wild goose chases—was very considerable indeed, and it seemed highly desirable not to increase the task by carrying too many decimals. Not being satisfied with the reference to custom and being acquainted with the theory of physical measurements, my mind naturally drifted into the channels pointed out by Professor Safford. I found out very soon that there does not exist a universal agreement as to the number of decimals which should be retained in the result, and that the rules explained by Professor Safford are not the only way of approaching the problem, for no less an authority than Gauss advocates the rule that the computations should be carried so far that the final result should enable one to calculate the actual data of observation with their original precision.

Professor Safford says that it is customary to use the average deviation as a measure of precision and to retain two significant figures of it. There does not exist any such general custom. In Germany and Austria the mean error is in almost exclusive use, while English, American and some French textbooks on the method of least squares recommend the probable error. Something may be said in favor of each one of these quantities, but this is not the

topic of the present discussion, where we only want to see whether there exists a universal agreement as to the quantity which is to be used as a measure of precision. Neither does there exist an agreement as to the number of significant figures to be retained in the measure of precision. I open the chapter on the adjustment of observations in Czuber's text-book of the calculus of probabilities and find on pages 293, 294 and 298 examples in which the mean error is calculated to one, two, three and four significant figures. Some of these examples are taken from authoritative sources, so that one cannot possibly say that there exists a general rule as to the number of decimals which should be retained in the result. Two or three significant figures in the measure of precision seem to be most frequently used.

There exists a fundamental difference between the data of statistical observation and the results of physical measurements, which Professor Safford entirely overlooks. The results of physical measurements are exact within one-half of the last significant figure. Thus if we put down 29 inches as the length of a line, this result means that the line is not longer than 29.5 nor shorter than 28.5 inches. When using this result in a computation, one must not add digits to it, because the following figures are entirely unknown. The case of statistical observation is different. If we observe that an event takes place 29 times in 100 cases, both these numbers are absolutely exact. The figure 29 does not indicate a result which may vary between 28.5 and 29.5; but it means exactly 29 and we may add as many zeros as seems necessary. The number of decimal places retained is merely a question of convenience, and one cannot be accused of publishing a misleading result if the accuracy of the determination accompanies it.

It took me some time to see that the theory of physical measurement is not the most direct way of determining the precision of my observations. The original data of my experiments are determinations of the probabilities of the different judgments. The most direct way of finding the precision of these observations is given by Bernoulli's theorem. This theorem refers to observations, in which a chance event A occurred n times in a total number of cases N ; and it gives the most probable value of the unknown probability of this event and the limits of the accuracy of this determination. This is exactly the case of my experiments and I chose the probable error determined by Bernoulli's theorem as the measure of precision. A table of these probable errors is printed in the *Archiv f. d. ges. Psychologie*, 1909, Vol. 15, p. 287. The probable errors in the determination of the probabilities of the "greater" judgments for Subject I on the comparison stimuli 84, 88, 92, 96, 100, 104 and 108 were found to be 0.0015, 0.0044, 0.0090, 0.0132, 0.0157, 0.0097 and 0.0076. Admitting that two or three significant figures in the measure of precision is a conservative accuracy, I decided to retain four decimals in the tables of the relative frequencies of the judgments.

It was the traditional custom in statistics and psychophysics to use the methods of physical measurement uncritically. Lexis and his followers have shown how statistical data must be treated, and I tried to develop the theory of psychophysical measurement. That this can be done has been shown. At present we have a number of psychophysical methods which may stand alone on their own merit. Neither did I decide hastily, in breaking away from the old notions. It may be that there is a connection between the theories of physical and of psychophysical measurement; but at present we know only little about it, and the only statement which one could make with any kind of confidence is, that the theory of physical measurement must be based on that of psychophysical measurement. The so-called law of the distribution of errors of observation has resisted all attempts at a purely mathematical demonstration. Innumerable attempts have been made to explain this law—some of them by the cleverest mathematicians the world has known—but all have failed in so far as their proofs necessitated the introduction of some one assumption which is equivalent to the propo-

sition to be proved. It is only natural to suppose that this law contains some supposition of non-mathematical nature. It seems that it depends, in some way, on our method of making observations, and that the so-called Gaussian coefficient of precision is closely related to the threshold of difference, a fact which would justify Gauss in putting this quantity directly proportional to the accuracy of observation. The threshold of difference is an object of psychophysical investigation, and for this reason I believe that the theory of physical measurement ought to be based on that of psychophysical measurement.

We now turn to Professor Safford's second objection. His criticism of my use of Lagrange's formula of interpolation is twofold: first that the calculation is carried entirely too far, and second that the interpolation should have been effected by the graphic method. Before entering upon the discussion of these objections I want to say something about the general purpose of interpolation. It frequently happens that the values of a function are given for a certain number of values of the independent variable, and that one wants to know something about the values of the function for intermediate values of the argument. Every procedure which serves this purpose is called a method of interpolation. In the graphic method one plots the results on millimeter paper and connects these points by a smooth curve. Every point of the curve corresponds to a certain value of the function, which in many cases may be read off with sufficient accuracy. This is the method which Professor Safford thinks I should have employed. I have done so as a matter of fact; but I did not consider such results of sufficient interest to publish, and only published a notice in the *Archiv f. d. ges. Psychologie*, 1910, Vol. 18, p. 410, that the charts are at the command of every scientific investigator, who may be interested in them. The chief objection against graphic interpolation is that it is too arbitrary; and for this reason I relied upon numerical interpolation alone.

There is a great variety of methods of numerical interpolation; but the essential feature of all these methods is that an algebraic expression is given, which may be fitted to the course of any function. Two of the best known methods are known as Newton's method of differences and Lagrange's formula of interpolation. Both methods are essentially identical, since they both suppose that the function may be represented by an algebraic function of degree n . The greater the number of observed values is, the more reliable is the result of interpolation; and it is, therefore, desirable to have as many observed values as possible. In a scientific investigation, however, one has to take into account that the time and energy of the observer are limited and that it is better to have a few carefully made observations than a mass of not very dependable results. In planning an investigation one has to strike a happy medium, which gives as many carefully made observations as possible. Experience shows that in the study of the psychometric functions seven values of the comparison stimulus are as much as can be handled easily and effectively. I may support in this respect my own opinion by the authority of G. E. Mueller. It is obvious that one cannot make observations for every intensity of the comparison stimulus, and that one has to fall back upon interpolation, if one wants to know something about the intermediate values. I, therefore, cannot see the reason why Professor Safford should find fault with my tables, because only seven entries were original results, a fact which could not fail to be noted by anybody who read the text. Tables of interpolated values are plentiful in physics and astronomy, and no one objects to them, if they are properly pointed out as such.

I now want to call attention to a small error in Professor Safford's text and a slight inconsistency in his position. He says on p. 97 that the seven ordinates were treated as absolutely exact. This is not quite correct; the abscissæ were so treated. He, furthermore, objects to the actual set-

ting up of the equations by Lagrange's formula on account of the great number of decimals which must be retained in the coefficients, but does not raise the same objection against the interpolation without setting up the equation. The advantage of this formula is that the interpolation can be effected without setting up the equation, but the use of the formula nevertheless implies the equation. To be consistent Professor Safford should have objected to the use of Lagrange's formula in any shape, but this would have precluded the use of Newton's formula, which Professor Safford favors, because it gives the same result "and requires about one-tenth of the labor." I beg to differ on this score. I tried both methods and found that the number of figures to be written down to effect the interpolation for one intermediate value and all my seven subjects was smaller for Lagrange's formula than for Newton's method of differences.

Professor Safford's clever-criticism of the equation set up for the psychometric function for Subject *I* is likely to carry the most conviction to the reader. The discrepancy between the amount of work spent in setting up the equation and the result obtained is so great that one cannot possibly help being struck by it. In this part of the work I had the good fortune to obtain the services of a professional computer, who first set up the equation with six decimal places of the coefficients only. The results of interpolation by this formula were fantastic, because the curve did not follow the actual results at all. The necessity of retaining as many decimals as were actually used later on, began to dawn upon me only after I had reasoned out that each one of these coefficients had to be multiplied by high powers of numbers around 100. I then realized the necessity of carrying out all divisions to the bitter end, and incidentally won an insight into the nature of Lagrange's formula. This formula is a merely artificial construction, the coefficients of which have not immediate physical significance at all. The formula of interpolation is a means of achieving a certain purpose and he who wills the purpose must will the means.

It is very interesting to analyze Professor Safford's criticism of my statement that the use of Lagrange's formula does not imply a definite hypothesis about the psychometric functions. The meaning of this expression, which I explained at some length elsewhere, is this. The psychometric functions give the dependence of the probabilities of the different judgments on the intensity of the comparison stimulus. We do not know anything about this dependence, but we have to make some hypothesis about it for the purpose of interpolation. This can be done in two ways: by assuming a function which fits any kind of results, or by assuming a definite law of distribution. Lagrange's formula belongs to the first class, because the degree of the function depends on the number of observations only. The form of the function is, therefore, different in different cases. A further difference between these assumptions and a definite hypothesis about the psychometric functions consists in the fact that the latter admits of an extrapolation, whereas the former as a rule, do not.

I may illustrate this distinction by the following example. Suppose that a table be given, which we know contains the values of either the sine or tangent for small angles but we do not know which. We may use Lagrange's formula for interpolating in this table, and we may represent the course of the function in this interval by this formula, but we fully realize that this hypothesis is not definitive but subject to correction and that the formula will not represent the course of the function outside the interval. If, however, we possess some further information, which leads us to believe that the tables contain the values of the function sine, we make a definitive hypothesis about the function. My monograph on statistical methods contains only a casual mention of this distinction, but the articles in the *Archiv* leave no doubt as to the meaning which I wanted to convey by these words.

Professor Safford's criticism is this: "Lagrange's formula gives the equation of a curve through n points, whose degree is not greater than n (this is not correct, it should read $n-1$; remark of the writer), and the n points determine the curve completely." There is an infinity of curves of the same type and Lagrange's formula merely has the merit of being the simplest, and it is therefore useless to spend much energy upon it. This is the standpoint of the mathematician, whose interest lies in the study of the properties of whole groups of curves. The standpoint of the practical calculator is different. He attempts to reach his goal by the shortest possible route; and I, for one, refuse to consider a method of which I know beforehand that it has no merit over another excepting that it is more complicated. Whether in a scientific investigation energy is spent uselessly or not, depends upon the importance of the results obtained; and this can be judged by the specialist alone. I do not believe that my energy was wasted in this case, for I am willing to go through all the trouble of working out my data merely for the sake of finding the result, that the maximum of the psychometric function of the equality judgments must be related to the threshold of difference.

Professor Safford sees a further objection to the use of Lagrange's formula in the fact that it excludes at once all probability curves, symmetrical as well as asymmetrical. In my opinion, this is one of the greatest advantages of direct interpolation that it fits the actual data of observation without making the assumption of a definite law of distribution. If the data follow one of these functions, the interpolation by Lagrange's formula will follow it closely enough. I may remark that I am at this point in perfect agreement with W. Wirth, who in his latest publication employs the method of direct interpolation. One of the most distressing features of the history of psychophysics is the endless discussion as to the applicability of a definite law of distribution. It was my purpose to get away from this discussion and to see how the curves would look, if the data were not adjusted according to a definite law of distribution.

Professor Safford's last objection is based on the fact that the results of direct interpolation do not agree very well with what he calls the theoretical curves. I may remark here, that I made it a point to speak of hypothetical curves and not of theoretical curves. There is, of course, only little difference between a theory and an hypothesis, because many theories should be called hypotheses; but it seems to be a pretty general rule that one does not call a doctrine an hypothesis, unless one wants to emphasize the hypothetical element in it. The supposition that the psychometric functions belong to a certain type has the character of an hypothesis in a very high degree. There is no possibility of deciding beforehand whether the data will fit such a curve; and for this reason one ought to insist on calling such an assumption an hypothesis and not a theory.¹ The fact that the actual distribution does not coincide with the hypothetical one, is very

¹I may be allowed to state here the reason which prompted me to choose the term $\phi(\gamma)$ hypothesis instead of the customary name of Gaussian distribution. It seems to me that this term should be restricted to the distribution of errors of observations exclusively. In the work of Gauss there is no passage known, to me, which could lead us to believe that Gauss would have applied this law to empirical distributions of all descriptions. Considering the practical turn of Gauss's mind, it seems very unlikely indeed that he would have favored such an unwarranted generalization. One, therefore, ought to speak of a distribution according to the probability integral or to the $\phi(\gamma)$ function. To call this function by the name of Kramp-Laplace, as Opitz has done, makes the name a little clumsy, and is not entirely justified from the historical point of view. It is entirely inadmissible to call this function by the name of Gauss; and the chances are that Gauss himself would have been very much surprised by this honor. In the *Theoria Motus Corporum Coelestium (Werke, Vol. vii, p. 238)* the integral is referred to Laplace, but as a matter of fact this integral was already known to Euler. Gauss was acquainted with this fact, as may be seen from a letter dated February 10, 1810, and from a manuscript note to the *Theoria Motus*. To call the function by the name of its inventor, one has to take one's stand in a complicated historical question, for the decision of which the complete material is not yet at hand.

interesting in view of the above mentioned discussion; but it is not so much an argument against the use of direct interpolation as one against the use of some hypothetical probability curve.

After paying me some compliments as to the mathematical treatment of my problems, of which, as coming from an authoritative source, I am highly appreciative, Professor Safford remarks that my data are hardly sufficient to warrant an extensive treatment. Of course I agree that it would be eminently desirable to have a more extended material, and that it would be possible to improve upon my results; but I must insist that it is at present the most suitable material for testing the different psychophysical methods. Other statistical sciences make very extended use of mathematical methods, and their material is not always as good as that of my experiments. The work of constructing mortality tables requires efforts compared to which my work—as being done by one individual—shrinks into insignificance; but the data used do not always show the high degree of stability of those found in my experiments. This fact may readily be seen from the values of the coefficients of divergence: But few statistical investigations deal with data of a similar degree of stability. I do hope that we may soon possess an experimental material still more extended than my own; but until then my data are the best available for the specific purpose of testing the different psychophysical methods. And if one wants to make such a test one has to make the best of the material at hand.

Professor Safford has presented his criticism with admirable clearness and precision; but I am convinced that his position is untenable, and his argument is unsound. The methods of physical measurement cannot be taken over bodily and applied directly to the problems of statistics and of psychophysics. And while we are always grateful for the mathematician's interest in psychophysical discussions, yet it is a truism that every science is obliged to develop its own methods, and to grapple with its problems in its own way. That the methods and the problems of the theory of observations and of psychophysics are the same cannot be maintained; whether, indeed, any intimate relationship obtains between these two fields of scientific endeavor still remains to be determined.

BOOK REVIEWS

Dogmatism and Evolution. By THEODORE DE LAGUNA and GRACE ANDRUS DE LAGUNA. New York, The Macmillan Co., 1910. pp. iv, 259.

The term dogmatism, as employed in the title of this work, is intended "to denote the body of logical assumptions which were generally made by thinkers of all schools, before the rise of theories of social and organic evolution. Its application is therefore wider than common usage would warrant. The empiricism of Berkeley and Hume, as well as the rationalism of Descartes and Leibniz, is included in its scope" (Preface). This usage of the term is sufficiently justified by the presentation. The authors aim to make prominent the fact that empiricism and rationalism, in spite of their wide divergences, are founded upon a common basis, and that this identity of assumption is more significant for present-day philosophy than are the differences. The fundamental dogma behind both standpoints is also present in the philosophy of Kant, and in absolute idealism. The pragmatic movement, moreover, which is principally a protest against these earlier philosophies, is to some extent misdirected, since it perpetuates in a measure this self-same dogma. It suffers further from the inevitable extravagances and overstatements pertaining to doctrines that express the irritation of a reaction; and hence, while it represents an important truth, it requires re-interpretation and correction.

In presentation the first two parts of the book in particular are not only compact and closely-reasoned, but coherent and lucid. The treatment of the subject-matter can lay claim to originality, and is splendidly stimulating and suggestive. Historical rationalism and empiricism differ in that the one makes an appeal to mathematics as its ideal of knowledge, while the other relies upon introspection. In spite of this contrast, however, they both assume that experience presents us with certain unanalyzable elements, which serve as the foundation for all further knowledge. In the case of rationalism, these simple elements are universal propositions possessing intuitive certainty; in the case of empiricism, they are particulars which are capable of entering into various combinations. Hence both encounter the same difficulty, for both are committed to the view that relations are external to their terms. This is obviously true of empiricism; but it is no less true of rationalism, for the relation of inclusion cannot obtain between simple concepts, and hence rationalism must take as its starting-point, not concepts, but judgments which are indemonstrable and synthetical. Spinoza, indeed, attempts to start with a concept in which all other concepts are implicit, viz., that of substance. But the exposition runs smoothly merely because the concept is at once completely simple (or indeterminate) and infinitely determined. Rationalism fails to justify either the synthetic character of its most fundamental judgments or the passage from a system of universal truths to the 'infinite determinations' of fact in which this system finds embodiment.

This result sets the problem for Kant. The fact, however, that 'synthetic *a priori* judgments' present a problem at all is due to the assumption, which Kant shares with his predecessors, that analysis must yield final elements. "No proposition could be determined as synthetic, unless a complete definition of its terms had exhibited their ultimate disparateness" (p. 73). In other words, Kant proceeds on the assumption that pure thought supplies to experience certain universal modes of relationship to

which every experience must be subject. This assumption persists in absolute idealism. At first sight, the contrast between rationalism and absolute idealism is as great as could well be imagined. The former made relations external; the latter asserts that "the essences of things are wholly constituted by their relations" (88). The procedure of rationalism is a descent from first premises; that of absolute idealism is an ascent of which the fundamental principle of the entire scheme is the goal. Yet absolute idealism, like rationalism, is committed to the proposition that "the order and connection of thoughts and the order and connection of things are the same" (108). It necessarily depends upon an inner dialectic for the movement of its self-contained system of thought, and hence it has to choose between the claim of being able to account antecedently for all the contingent facts of history, or else to accept existing irrational facts, and thus to admit an irreconcilable contradiction in its theory of actuality.

It appears, then, that rationalism, empiricism and absolute idealism are all dogmatic, in that they all proceed upon the basis of an untested assumption with regard to the 'simple elements' or constituents of experience. On the other hand, pragmatism bases itself upon evolution and endeavors to give a functional interpretation of logical and psychological problems. With this endeavor the authors profess themselves in sympathy. Their argument, however, is intended neither as a defense of pragmatism nor as an attack upon it, but as a justification of the charge that current pragmatism is "only half-free from the grip of the traditions which it openly repudiates," and thus untrue to the deeper spirit of its own standpoint.

As a matter of presentation it is unfortunate that the authors do not connect the discussion of pragmatism more closely with the results of the preceding exposition. As they themselves admit in the preface, this omission detracts appreciably from the unity of treatment. The basis for the charge against pragmatism, it seems, is the fact that the latter has formed entangling alliances with immediatism. While pragmatism is right in its emphasis upon functionalism, its most prominent advocates have all professed their adherence to some form of immediatism. This creed is not only inessential to pragmatism as such, but is incompatible with its deeper meaning, for it introduces once more the attempt to base our thinking upon a 'simple element' or 'given.' A starting-point of this kind necessarily leads to perverted notions regarding the nature of thought. In effect it means that relations once more become external, as appears most strikingly in Professor James's contention that "the self-same piece of experience taken twice over in different contexts" is equivalent to the distinction between knower and known. The relations are treated as merely additive, *i. e.*, as exerting no influence upon the character of the experience. Essentially the same criticism applies to the pragmatic treatment of concepts. While it is true that concepts necessarily have reference to conduct, it does not follow that the nature of the concept is exhausted in any direct and 'external' relation of the given experience to a specific form of conduct. The relation of the concept to conduct is more indirect and equivocal. "From the standpoint of biological utility it is clear that the object, so far from meaning a definite type of behavior, is recognized as an object only as it is associated with important diversity of behavior in characteristically different situations" (p. 168). In other words, the concept is of necessity more inclusive than any given type of behavior. The concept cannot be identified with any conscious process, however complex, for "the group of associations which constitutes the concept may never in its entirety be present to consciousness in any single experience" (p. 170). It follows, furthermore, that "apart from this reference of thought to conduct, that is to say, in the limitless interrelations of concepts with each other, thought has as distinctive a form as any abstractly considered entity whatsoever" (p. 207).

The gist of the matter, then, as regards current pragmatism, seems to lie in the proposition that functionalism may be divorced from immediatism. On just this point, however, the position taken in the book does not seem to be altogether consistent. In the discussion of J. S. Mill's theory of objectivity (pp. 173-185), it is pointed out that Mill's fundamental mistake lies in the fact that he takes simple elements of sensation as his starting-point. These elements are held together by connections which Mill regards as 'real' but as inexplicable. The alternative proposed by the authors is that sensation is a scientific construct, that the distinction between sensations and relations is simply a matter of logical analysis. In other words, the relations fall within the experience quite as much as do the sense-elements. This is only another way of saying that objects are immediately presented—however we may see fit to interpret objectivity. Earlier in the chapter, however, the distinction between the 'given' and its relations is drawn in quite as hard and fast a way as was ever done by Mill. A passage was quoted in the preceding paragraph to the effect that the concept is never in its entirety present to consciousness in any single experience. So far as the exposition goes, there is no ground for the belief that it is ever present to any degree or in any intelligible sense whatever. On page 171 the question is raised: "How, indeed, can given conscious contents 'represent' or 'mean' or 'point to' other possible contents not given?" The answer which is suggested is that there is a tendency on the part of the associated experiences to rise to clear consciousness, and that "such inhibited tendencies to revival may affect in a distinctive manner the qualitative tone of the existing content." Such an explanation obviously fails to explain. We must either identify the concept with these nascent associations, which is incompatible with the general account given of the concept, or we are forced to recognize that the tendencies in question can, at most, effect a change in the quality or structure of what is presented or experienced, a change which may perhaps be interpreted as corresponding to the function, but which is in no sense identical with it. In other words, the relations or functions, regarded as such, necessarily fall outside the experience. That this is the intention is evidenced by the general tenor of the book, and in particular by the avowed agreement on this point with Berkeley and by the assertion that the 'real' is never experienced but always remains ideal.

The implication of the foregoing, it is evident, is that *Dogmatism and Evolution* is itself in bondage to the tradition which it accuses pragmatism of perpetuating. It is committed to the very opposition of universal and particular which it charges against the "immediate empiricism" of Professor Dewey (pp. 244, 246). In the end, the denial of immediatism is purely verbal, for the relations or meanings which are necessary to constitute things are opposed to what is 'given.' Hence we have the assertion that the 'real' is "never immediately experienced at all; it is always ideal" (p. 245). This charge, however, is significant, for it seems to indicate the source of the trouble. The immediatism attributed to Professor Dewey is essentially that of the older empiricism—the immediatism which constitutes a contrast to all forms of interpretation or mediation. It is urged, for example, that immediate experience can contain no uncertainty and doubtfulness; also that the Zöllner lines cannot be immediately experienced as convergent,—the reason in the latter case being that convergent lines are lines which when extended meet in a point, whereas the lines in question do not meet unless *conceived* as extended. In other words, if the lines are conceived as extended, the experience is held to be no longer immediate. The point of Dewey's contention, however, is that both the immediate and the mediate of ordinary philosophical usage are enveloped in a wider immediacy, and that this immediacy is meant when the assertion is made that things are what they are experienced as. Moreover, as Pro-

fessor Dewey says, this proposition is not identical with the platitude that experience is experience, but has the significance of a method of philosophical analysis. If mediation is itself immediately experienced, the proper way to find out its nature is to observe its operations as they occur, instead of applying the mediation *ab extra*, as has been done so frequently in the past.

In brief, then, it would seem that if the function and content of concepts are not immediately experienced, we are back at the standpoint of Mill, and left to derive what comfort we can from the classification of a contradiction as an 'ultimate mystery.' On this ground, moreover, we seem compelled to choose between the alternatives offered by Professor Royce, viz., a validism of Mill's type, and a world which is the embodiment of an "absolute system of ideas." At all events, we can hardly be content to say merely that the real is always ideal. On the other hand, if mediation is directly experienced, there seems to be no ground for identifying the real with something beyond experience or with any particular kind of experience. One experience is then, to all appearances, as real as another.

In conclusion the reviewer may be allowed to say that since limitations of space do not permit comment upon the many excellent discussions contained in the book, the foregoing criticism may well seem disproportionate, in view of the many solid merits of the work. But disagreement, even though pretty fundamental, is entirely compatible with sincere respect and appreciation. Readers who remain unconvinced by the third part will nevertheless find the work one of distinct and unusual ability, a work that will abundantly repay a careful reading.

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Manual of Mental and Physical Tests. By GUY MONTROSE WHIPPLE, PH.D.
Baltimore, Warwick and York, 1910. pp. ix, 534.

This manual includes a description of the apparatus and method of administration of fifty-four tests or groups of tests, a series of accounts of the chief results obtained by those who have used them, and a corresponding series of bibliographies. There is also a summary of the formulæ and tables useful in calculating central tendencies, variabilities, reliabilities and correlations from the obtained measures.

Eighty pages are given to means of measuring height, weight, head-shape and size, breathing capacity, and muscular strength, speed, precision and steadiness. The next ninety pages concern tests of sensory capacity. Under the headings 'Tests of Attention and Perception' and 'Tests of Description and Report,' we have, in the next ninety pages, carefully elaborated forms of tests in perceiving letters, words, etc., exposed by the tachistoscope, in cancelling words, letters, etc., printed amongst others, in counting dots, in reading, in adding a one place number to three given numbers in succession, in simultaneous reading and writing, and in describing and passing a detailed examination upon objects. A fourth portion of the same length covers tests of association (thinking of a word, of a word to fulfill certain requirements, and of the facts needed for simple computations), learning (to copy drawings seen in a mirror and to translate certain characters into numbers with the aid of a 'dictionary' printed on the blank), and memory (of series of digits, letters and words, and of passages). Finally, in somewhat over a hundred pages we find tests of suggestibility, imagination, invention, intellectual equipment, and developmental diagnosis. These include, often in improved forms, the size-weight illusion, Binet's other tests for suggestibility, tests of the effect of suggested warmth, Dearborn's ink-blot test, the familiar school tasks of including given words in a sentence, completing sentences and writing compositions, word-building from given letters, the Ebbinghaus 'Combination' test, Swift's interpretation of fables, Kirkpatrick's test of knowledge of the meaning of words,

Whipple's test of range of information and the De Sanctis and Binet-Simon tests of intelligence.

The tests have been chosen on the basis of thorough knowledge of the work that has been done in the field and with due appreciation of the uses to which they will be put. Although probably no student of the subject will agree with Mr. Whipple's selections in every case, all will admire their general worth and timeliness. And those who find the most to disagree with will perhaps appreciate their general worth most fully. Any one who plans to measure intellectual abilities of whatever sort, should as a first step become familiar with the tests recommended in this Manual.

The chief desiderata in means of measuring mental traits are that something of importance be measured, that the resulting quantity be objective or verifiable by any competent observer, that the precision or freedom from variable error attainable from a given expenditure of a subject's time be reasonably great, that the time and energy of the experimenter be economized, and that the results be commensurate with those hitherto obtained in measurements of the mental trait in question. There results a balance of goods in the selection or invention of a test in the case of almost every mental trait. Consequently, a practically infinite amount of ingenuity can be expended in devising tests to satisfy best these desiderata. A standard test, in the sense of an unimprovable one, probably does not now, and will not for a long time, exist in the case of any mental trait.

Professor Whipple has all these facts in mind, but, I think, in two ways does not quite maintain the most serviceable balance amongst them. In some cases he perhaps imposes too great a burden upon the experimenter in order to make too slight a gain in objectivity, precision or comparability with previous work. He is also too modest in recommending a test which happens to have been used by some one a few times, instead of devising a far better one himself. For example, counting dots (Test 27), adding a given number, say 3, in succession to three numbers and continuing with the sums thus obtained (Test 29), amenability to oral suggestions from the experimenter (Test 43), and the interpretation of fables (Test 49) have been very seldom used and could easily be very much improved.

The very difficult task of giving instructions in the administration of all these tests is well done. Often the desirable plan of printing exactly what the experimenter shall say is followed. A mass of minor information hitherto acquired at great cost of time by imitation, can thus be put into the student's hands once for all. If mental measurements are to be made by others than trained experts, such detailed instructions (possibly even still more detailed and rigorous instructions) must be accessible in print.

It would have been a great addition to the usefulness of the manual if Mr. Whipple had given approximate measures of the number of trials with each test necessary to secure a given degree of reliability. For individual diagnosis and prognosis, for measurements of change and for measurements of the relations between mental abilities, it is of very great importance to reduce the unreliability of the average or median ability found for an individual to a small per cent. Investigators commonly err by dispersing their time over too many individuals, not measuring each one precisely enough to allow straightforward inferences about anything save group averages.

Where the author does announce the number of trials to be made, I fear that he gives too few. For example, in measurements of the delicacy of sensory discrimination, he commonly requires, after a brief preliminary series, only ten judgments with the difference chosen, ten with one a little greater and ten with one a little less. It would seem that if sensory discrimination is to be measured by the per cent. of right judgments, at least fifty judgments of a given difference should be taken. If only ten are to

be taken, an arrangement to use the average error made by the subject seems preferable in many cases.

I may note also that to give only differences and permit only judgments of more . . . or less . . . relieves the experimenter from very annoying elements in the latter calculations and on the whole seems better than to allow judgments of 'equal.' The author's instructions vary on this point.

The chapter on statistical methods gives the standard formulæ with illustrations of their calculation. It is made specially useful by including the later short methods of calculating correlations. I regret that the author accepts Pearson's speculative assumption that to compare the variabilities of two series each gross variability should be divided by the corresponding central tendency. No one method of rendering the variabilities of the same group in different traits or different groups in the same trait comparable is universally valid, and certainly not the method of dividing by the central tendency. Dividing by the square root of the central tendency will be more often and more nearly right.

The summaries of work done and the bibliographies accompanying them represent a scholarly heroism all of whose sins of commission and omission will readily be pardoned by any one who has tried to do the like. The only serious fault, I think, is in quoting as measures of correlations, figures got before the effect of the variable errors of the original deviation-measures in reducing the obtained correlation from the true correlation toward zero had been discovered by Spearman. The obtained correlations of Aikins, Thorndike and Hubbell and Wissler were thus necessarily far too low. Mr. Whipple's interest in the generally neglected subject of correlations also leads him to mislead the ordinary reader by quoting resemblances of related individuals in the same trait along with the resemblances of a person's degree of ability in one trait to his ability in another. The former should be carefully explained if quoted at all in such connection.

It is to be hoped that this book and the reports that are being issued by the American Psychological Association's committee on tests will be studied and used by every investigator of human intellectual performance here and abroad. The earlier expectations from tests of human faculty on the basis of the faculty psychology, being too great, were destined to disappointment, but now that the complexity, variability and relative independence of mental functions are being understood and allowed for, we may hope for a revival of interest in inventories of individual intellects, in measuring the changes which they undergo by growth and training, and the causes of their original capacities. If Professor Whipple's work did nothing more than stimulate other investigators to measure the reliability of his tests, their susceptibility to practice effect and their value as symptoms of more general conditions, and so to amend or even replace them, it would have abundantly justified itself. It will do much more than this.

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The Phenomenology of Mind, by G. W. F. HEGEL. Edited, with an introduction and notes, by J. B. Baillie. London, Swan Sonnenschein & Co., 1910. 2 volumes. Vol. i, pp. xlv., 427; vol. ii, pp. viii., 429-823.

In Professor J. B. Baillie's recent translation we have now before us Hegel's *Phänomenologie des Geistes* in an adequate English dress. The Teuton has so far failed to make anything worth while out of this unique intellectual product from a fellow-member of his race. It is now handed over to a more distant relative, but perhaps none the less close still to the central intellectual tendencies of the Germanic races for the *rapport* necessary to its understanding, in a form to make it more readily accessible, and so to give the Anglo-Saxon a turn at its interpretation. As is, in part, implied in these two sentences, we shall probably have to approach the translation,

which as adequately represents the original as this is possible in a translation, on the assumption that it is only a means to an end, for two reasons. First, the genius of language, reflecting the habits of thought of a race, is sufficiently diverse in the English and the German to require a closer adaptation of the methods of presentation than a mere translation affords. Secondly, Hegel's presentation is, in fact, inadequate to begin with, and naturally so because he had undertaken a task to which there were no established precedents other than mere vague effort; and the great bulk of modern scientific work giving a training in accurate formulation has all come after him, so that his work needs first of all to be modernized. But the present translation will now unite the efforts of interpretation over a greatly extended area.

In a brief appreciation of such a book as the present there are perhaps essentially two things of interest to the reader. First comes the question as to the purpose of the book, and as to whether its preparation is adequate to the purpose. Second, and of equal importance would, in the present instance, probably be additional suggestions calculated to enhance the value of the book. The first has probably been sufficiently considered above; to the second we might now give a little more attention. In general, the cynical aspersion on Hegel's work of its being an intellectual travesty cannot satisfy an impartial mind upon a glance at the table of contents and a mere superficial perusal of the text of the *Phänomenologie*. Particularly, great emphasis is everywhere put on the unity of all matter of experience, and on the need of taking fully into account the presuppositions implied in our expositions in science through this unity of all matter of experience. This receives practically no attention in present-day science, while its recognition would bring about a far reaching revolution in science. To get into Hegel's analysis in the present work, a very close study of his Introduction, with all the implications, is advisable for the necessary point of view. This point of view is not that experience as a whole must be explained in terms of some single definite conception, as idealism, monism, materialism, psychophysical parallelism, and so on; but that these very conceptions themselves are all elements in the form of stages in this experience, while the single fact of experience as a distinct conception is nothing for us. Perhaps due largely to its subject-matter, the English rendering of the Introduction is particularly good, so that not even an original reformulation in the English could improve it much. One suggestion might here be made, which has reference more especially to the genius of the English language and the Anglo-Saxon habit of mind. If in place of the abstract noun representing completed action the present participle of continuing action were more generally used, or if the reader will more generally supply the sense of a continuing for that of a completed action, the representation of the original would be more exact; as for instance near the top of page 78, by putting for *die Darstellung des erscheinenden Wissens*, "the presentation of a developing knowing" instead of "the exposition of knowledge as a phenomenon." This will make awkward English, but it more adequately represents the continuous flux of conceptions as necessary to understand Hegel, contrasting with the fixedness or permanent demarcation of conceptions in the English.

It might be to the point to consider this flux of conceptions in Hegel's method a little farther; and the real nature of this flux is best shown by its formal recognition in the antinomy. In the antinomy, we have a contrast of two mutually exclusive or incompatible appearances of the same matter of experience of such a nature that one of them complete and distinct, but only one, is necessarily present while the other is impossible for the time being. Upon close examination and analysis of the appearance present, however, it literally dissolves, as if by magic, under our very eyes; and its contrasting opposite appears and immediately takes its place. Moreover, only at the completion of the actual perversion are the full meaning and all

implications of each of the two forms of appearance of the experience wholly understood. Classic among such antinomies are those regarding the nature of motion in our environment developed among the ancient Greeks; regarding the innateness of ideas, developed by ways of reaction between Descartes and Locke; regarding the nature of the reality of the matter of our experience, developed by Berkeley and Hume; and the cosmological antinomies of Kant. Besides these particular instances, the antinomy can, however, be found permeating our experience everywhere. Through it is revealed an actual single inner movement and unity, in the universe as it surrounds us, totally different from the cosmic motion of the Copernican system and not recognized in present-day science but which alone will account for such perplexing phenomena as the action of force at a distance. The fixedness of conception, then, as most characteristic of the Anglo-Saxon mind, here invariably leads to confusion and a sense of loss of reality with the passing of the forms present, because the continued presence simply of the fixed forms is taken to be the reality, while philosophical analysis invariably shows that the forms cannot be so retained. The Introduction, from page 82 to 89 of the translation, read in the light of this suggestion, may prove to be of more value, reference being had not only to the fact of flux, but also to the mode of flux, of conceptions.

Furthermore, Hegel in his analyses exhibits a kind of pedagogical ungainliness, which borne in mind will explain and clear up no small part of his obscurity. So the following summary may serve to guide and elucidate. The "Meaning" in the section heading on page 90 of the translation should be understood in the sense of "Supposing;" for this beginning section, subordinated under the general head of Consciousness, is a discussion of the *sinnliche Gewissheit eines gemeinten Dasein*, the sensations or sensuous awareness of a supposed this as apart by itself, over against the knowing, which is the beginning of all conscious knowledge. The next sub-section under the head of Perception (page 104) examines the development and nature of the unities in things, under another aspect also called universals, as essentially forming the basis of our conceptions of the nature of things. The third sub-section under the head of Understanding (page 124) discusses the formation of a continuous scheme of things constituting our conception of the nature of things and consisting of two fundamentally distinct parts or elements, the physical occurrence or fact of experience, and the apperceptive content or metaphysical part in an Aristotelian sense. Following these three sub-sections under the general heading of Self-consciousness (page 163) is shown the mode of appearance of a persisting self possible only through a succession of antinomies or course of antinomic dialectic. The preceding sub-sections show the essential elements in ordinary knowing in their more or less independently distinct character; in this section we have these elements, mutually dependent, forming an indissoluble system. The rest of the work, beginning with a section under the head of Reason (page 220), then shows how out of the movement of successive reactions between the antinomic moments, in a single movement, of a persisting self knowing and an other than the self known, develops the comprehensive structure of science, the conventionalized conscious content of our experience. A closing section under the head of Absolute Knowledge (page 800) then considers various characteristic incidents of the independent absolute form attained in our experience by the foregoing mode of analysis.

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De l'origine et de la nature mnemoniques des tendances affectives. Par E. RIGNANO. Estratto da "Scientificia": Rivista di Scienza. Vol. 9, Anno 5 (1911). N. XVII. 1. Traduit par le Prof. J. Dubois. 35 p.

In this article the author outlines a genetic theory of the affective states. The term affective is restricted to the special category of organic tendencies

which manifest themselves subjectively in man as desires, appetites or needs, and which objectively, in both man and animals, are translated into non-mechanized movements. Admitting this definition, the author reduces the entire series of the principal affective tendencies to the single fundamental tendency of the organism toward its own physiological state of equilibrium. This tendency may be observed in all unicellular organisms, *e. g.*, hunger which is the most fundamental affective state is reducible to a tendency to maintain or re-establish in the nutritive internal *milieu* the qualitative and quantitative conditions which permit a stationary metabolic state. This tendency toward a state of metabolic equilibrium has become, in the course of phyletic evolution, a tendency to accomplish all the acts necessary to procure food. The hydra and sea anemone, for example, react positively to food only if the metabolism is in such a state as to require more material. The localization of hunger in the higher animals is a secondary development, and merely one of the multiple aspects of the part functioning vicariously for the whole which characterizes all the physiological mnemonic processes. It is the same with thirst, which though localized in the glands of the throat, is a need of the entire organism. Similarly, the need of elimination of substances produced by general metabolism, which the organism is not able to utilize, whether in the simplest infusoria or the most complicated vertebrate, follows the same general rule. In this category of affective eliminative tendencies, the author places the sex instinct. To this fundamental property that every organism possesses, *i. e.*, the tendency to conserve the equilibrium of its own normal physiological state or to re-establish it if it has been disturbed, must be added another which in its turn becomes the source of new affectivities. When the original state cannot be re-established then the organism tends to pass into a new static condition adapted to the new external or internal *milieu*. In this way, the whole series of the phenomena of adaptation is produced. The experiments of Dallinger and others on the acclimation of lower organisms have shown conclusively that this secondary state once established tends to perpetuate itself. This tendency is of a purely mnemonic nature and implies for the different elementary physiological states, forming altogether the general physiological state, the faculty of leaving behind a specific accumulation or mnemonic residue susceptible to revival or recall. The extension of this faculty of specific accumulation to all physiological phenomena in general is in harmony with the hypothesis which posits nervous energy as the basis of all vital phenomena.

With the extension of this mnemonic faculty to all the elementary physiological processes, we arrive at a somatic or visceral theory of fundamental affective tendencies. Naturally in organisms endowed with a nervous system there would gradually be developed along with the affective tendencies whose origin is purely somatic, the tendency, sometimes co-operative and sometimes vicarious, "represented by the corresponding mnemonic accumulations, deposited in that special zone of the nervous system which is directly connected with respective points of the body." In man, this zone would be the *Körperfühlsphäre* of Flechsig, to which is added in certain cases the frontal zone. These mnemonic cerebral accumulations once established under direct somatic action are able, even after communication with the body has been severed, to represent the original affective tendency in which they originated, *e. g.*, Sherrington's "spinal" dog showed the same repugnance to dog flesh in precisely the same way as a normal dog. Finally affective tendencies owe their subjectivity to their mnemonic, physiological origin, from the fact that the organism finds itself endowed with specific affective tendencies in accordance with the particular environmental history of the species or individual. In support of the foregoing hypothesis thus briefly sketched, the author cites various examples from the higher animals and man, *e. g.*, he finds the origin of maternal love in

the principle of elimination, the need of being nursed. Homesickness is due to the disturbance of fixed paths of habituation. As a further confirmation of the hypothesis of the mnemonic nature of the affective tendencies Ribot's principle of *transfert* is utilized.

In accordance with this principle, in itself of mnemonic origin, all affectivities not directly traceable to a mnemonic source are derived from those which are thus referable, and are therefore of indirect mnemonic origin, *e. g.*, secondary sex affectivities, cruelty as an end in itself, derived from the original tendency of tearing prey to satisfy hunger, the desire of victory for itself, originally self defence, the desire of amassing wealth, which is a transfer from the original simple impulse to satisfy hunger plus the intellectual element of foreseeing its recurrence.

Emotions according to this theory "are only sudden and intensive modes of putting in action those accumulated energies, which constitute precisely the affective tendencies." Emotions and affective tendencies are distinguished from each other by the fact that the same affective tendency may, under different circumstances, give rise to very diverse emotions; to emotions of different intensities or even, in some cases, to no emotion at all in the proper sense of the word, *e. g.*, the affective tendency of a dog for his piece of meat may be translated, according to circumstances, into flight, anger, or merely a search for a quiet place in which to enjoy it. As all affective tendencies result in movement, external or internal, the theory is here in accord with that of Ribot and the Lange-James theory.

The will is only an affective tendency inhibiting or impelling to action like every other affective tendency. As to pleasure and pain, the theory is in accord with that which interprets pleasure as the subjective accompaniment of unimpeded activity and pain as due to its inhibition.

THEODATE L. SMITH

A Text Book of Psychology. E. B. TITCHENER. Macmillan Company, New York, 1910. pp. xx + 565.

Professor Titchener in the present volume has given us more than a text-book of psychology. The book comes fairly close to being a brief, systematic psychology—an earnest, certainly, of what the author will give us when his more complete study is ready. At the present time, in English, we have at our disposal many elementary texts on psychology, and many elementary laboratory manuals; but we are poverty-stricken for advanced works in general, systematic psychology (based step by step on experiment), and for advanced experimental manuals on the various sense-fields,—attention, association, etc. Titchener's book, while supposed to be for elementary students, is far from being an easy text. Indeed, the author's own way of thinking has become so much more complex since the writing of the *Outline* that I doubt if he himself clearly realizes just how much of his more recondite reflections have become incorporated in the book. If I were seeking a quarrel with the Text-book I should find the grounds for it on the score of too great complexity. It is a little heavy for the average junior or senior. But psychological classes differ greatly in the different institutions. In some, psychology is required; in others, elective. In some the "quarter" system is in vogue, and only one quarter is allotted to psychology; in others, psychology runs the year through. It is doubtful whether Titchener's book can be adapted to meet the requirements of a short course. In institutions where the elective system is in operation, and where a full year can be given to psychology, I know of no text better to use than the one under discussion. In view of the fact that the author introduces experiments everywhere and that he discusses methods and results the book lends itself easily both to systematic and to experimental presentation. Any student going carefully over the work with a competent instructor will come out at the end of the year with an increased respect for psychology and with the

ability to think along psychological lines and to read and follow the future progress of psychology even if he carries his training no further.

In a book which is so full of factual material, we cannot hope in a review to discuss chapter and verse in any adequate way. Certain interesting points of view developed by the author, alone can be discussed. In the first place, Titchener's series of chapters on sensation is excellent—by far the best treatment we have. For, in addition to the full treatment of the ordinary laws and principles involved in sensation, we have the more recondite phenomena touched upon. Much additional material over that treated in the *Outline* appears. For example, we have a fuller treatment of color-theories; of the vestibular and ampullar senses; of the sensitivity of the abdominal tissue; of sensations arising from the digestive and urinary systems; and from the circulatory and respiratory system.

In discussing the attributes of sensation in general, the author tells us that there are four distinct attributes; quality, intensity, clearness and duration. The reviewer is puzzled by the attribute *clearness*. We all admit clearness as an attribute of complex conscious experience, but not as a fundamental aspect of the sensation-process—not in the sense in which duration and intensity are attributes. He says, "Clearness, again, is the attribute which gives the sensation its particular place in consciousness; the clearer sensation is dominant, independent, outstanding; the less clear sensation is subordinate, undistinguished in the background of consciousness." This is certainly to be admitted, but surely what Titchener is writing of here is an attention-state, in which a given "sensation" is focal, while others appear in what James calls the "fringe." In other words, clearness is one of the descriptive words which we apply to perceptual, ideational and other complex mental states. With this given as an attribute of sensation one would expect to find it taken account of somewhere along with the other attributes of sensation. But in his chapters on the special senses he speaks only of the usual attributes of each group, introducing certain changes in terminology, to be sure, as for example, he speaks now of the qualitative attributes of a color as being hue, tint and chroma.¹ And further, in audition, he speaks of size and diffusion as an attribute of tone. It would seem in places that he means to use this attribute of clearness in the same sense as we should use *clearness* in describing a perceptual state; but this would carry with it the inference, it seems to me, that sensation is something more than an abstraction—something that can actually present itself. Furthermore, in order to realize the conditions for the appearance of clearness, we should have to have at least two such "sensation processes" attempting to run their courses simultaneously. But this is certainly the process which we know as perception. The confusion, if I understand Titchener's statements, is similar to that found in James where sensation is at times discussed as an abstraction and at others as a process correlative with perception.

It is interesting to note that he treats of the sense-image under the general chapter heading of synæsthesia; since the image is the normal process, and synæsthesia the anomalous one, we should suppose that the traditional order of treatment were best. One would hardly begin a chapter on color vision for elementary students with a discussion of red-green blindness. His early discussion of imagery is rather disappointing. Only two pages are given over to it. One finds there few statements concerning the experimental mode of investigating the image, and very little of individual differences. This lack of emphasis of the image in an early place would seem to be a real limitation in the use of the book as a text. The average undergraduate rarely wakes up to real introspective interest in psychological problems until he has learned that he has imagery and can stand

¹ His introduction of the words chroma and tint are of doubtful value, since the word saturation, now in common use, seems adequate.

off and look at it, as it were, in the absence of a perceptual world. A brief study of the image awakens him far more rapidly than does a much longer drill on sensation-processes. Later on in the book, however, the author completes the treatment of imagery under the headings, association, memory and imagination. Here the treatment is full and adequate.

Following the chapter on synaesthesia is one on the intensity of sensation, which includes a discussion of mental measurement, liminal and terminal stimuli, just noticeable differences, and Weber's law. The chapter is concise, but clear, and since these topics are valuable to the student, such a chapter is a real contribution on the pedagogical side.

Then follows the chapter on affection. He stands by his position stated in the *Outline*. "The writer holds that there is an elementary affective process; a feeling element. . . ." "He holds further that there are only two kinds or qualities of affection, pleasantness and unpleasantness." Although the reviewer thinks he finds himself in another 'camp,' it gives him a sense of security to find a psychologist of Titchener's eminence who admits his position so frankly on such a vexed question as that of affection. On page 228, in a discussion of the relation between sensation and affection, he again enumerates the attributes of sensation—*quality, intensity, clearness, duration*. Those of affection on the other hand, are *quality, intensity, duration*. On page 231, he uses clearness as the distinguishing criterion between sensation and affection. "Pleasantness and unpleasantness may be intensive and lasting, but they are never clear." This is due, in short, the author says, to our inability to *attend* to an affection. "The lack of the attribute of clearness is sufficient in itself to differentiate affection from sensation. . . ." Again, this attribute of clearness attaching to sensation and not to affection, and further the fact that we cannot attend to affection, make the author reject the view that affection is really a complex or fusion of the accompanying organic sensations. While there is no time to argue the question, I cannot see that Titchener makes his point against this latter view. If we should grant his premises, namely, that affection lacks clearness, and that it cannot be attended to, we should be forced to admit his point. But these are just the questions at issue.

He discusses two methods of investigating affection: that of "paired comparison," and the method of "expression." He devotes about six pages to the method of expression, but is in agreement with the majority of psychologists in denying any great usefulness to it. It seems like a useless luxury in a text-book to treat so at length of a method which has absolutely nothing to recommend it. The tridimensional theory of feeling is well and critically discussed.

Space does not permit of even a brief review of further chapters; attention, perception, association, memory and imagination, action, emotion, and thought. The chapters on attention and on thought are striking and are both readable and teachable.

In the chapter on action it is with a shock that one again meets with his earlier expressed view that the first movements of organisms were conscious movements (agreeing thus with Wundt, Ward and Cope). According to this view, voluntary action degenerates into *ideo-motor* or *sensory-motor* action, and then into the reflex. But in animal life we find two kinds of responses, in every organism, beginning with the protozoa (as shown by the recent work of Gibbs and Smith, of Bentley and others): the one type being fixed and definite; the other diffuse, leading itself to habit-formation. Certainly I should agree with Titchener that consciousness is as old as life, but I should certainly connect consciousness with the diffuse type of response. I should say further that *the very first organism started with both types of response*. Surely nothing is gained, and confusion is introduced by the conception of Titchener that all movement was first a voluntary acquisition, and that only later do we begin to find fixity

in the responses of organisms. There is not a scintilla of evidence that *fixed* and automatic reactions do not appear with the first appearance of organisms. And there is abundant reason to say that each new species as it appears, *e. g.*, by mutation (see the work of Tower *et al.*), gives evidence of a reflex repertoire and of a plastic repertoire. Titchener argues that the reason we do not see this complete plasticity (which would be called for on his theory) in the unicellular form to-day, is that the protozoa have travelled all the way from plasticity to fixity.

Looking at the book as a whole, it seems to the reviewer that in many places Titchener has adhered too rigidly to the introspective method. Surely in his treatment of *meaning* he could have leaned advantageously a little way toward the functional side, without giving up the guiding principles of the book. Nevertheless in this day when, if I can read the signs aright, the pendulum is swinging another way—toward a study of life-situations as a whole, and the adequacy, permanency and different types of adjustment which such situations call forth—Titchener gives us an enviable example of a man unafraid of his own views of the problems of psychology, and of his own methods of solving them. JOHN B. WATSON

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L'année psychologique, publ. par A. BINET, avec la collaboration de LARGUIER DES BANCELS et Dr. TH. SIMON, etc. Seizième année. Paris, Masson et Cie. 1910, pp. IX, 500.

The introduction reviews the progress of psychology in 1909, treating especially the work on thought and on pathological states, and the work in experimental pedagogy and judicial psychology. The first original contribution, by A. Binet, is entitled "The physical signs of intelligence in children." Greater or less degrees of correlation are found between intelligence and size of head, the so-called signs of degeneracy (abnormally shaped head, ears, etc.), facial expression, and hands. The habit of biting the finger-nails is found to be without significance in this respect. The correlations found hold in general only for the group, not always for the individual. The physical signs are useful for confirming, rather than for making, estimates of intellectual level. Next in order is an examination of the art of Rembrandt, by A. and A. Binet. The authors attempt to show how, by avoiding extremes of contrast and by accentuating unity of lighting, Rembrandt has succeeded in giving those impressions of distance, of unity, and of light which characterize his work. "Tachistoscopic Researches," by B. Bourdon, is an investigation of the times of choice-reactions made by observers to whom colors, rectangles of different lengths and figures, have been tachistoscopically exposed. The writer measures the time of reactions involving judgments of resemblance, identification, localization, comparison, or combinations of these processes. The eight following papers, by A. Binet and Th. Simon, are concerned with defining the various mental derangements. Up to the present, the authors believe, definitions have been too inclusive and general, have failed to show the *essential* characteristic which marks off the disorder, and have been couched in terms only partly psychological. They themselves classify the derangements as (1) hysteria, (2) derangement with knowledge (fears, impulses, etc.), (3) manic-depressive insanity, (4) systematized insanity (paranoia), (5) the dementias (general paresis, senile dementia, dementia præcox), and (6) subnormality. They consider the history of the conceptions of the various disorders, the theories propounded and the attempts at definition. They also review the characteristic mental states, symptoms, and attitudes of patients, both as reported by others, and as shown by the new data here published. They compare the special derangement under consideration with the other types of derangement, and finally arrive at a conclusion as to its essential character. Of hysteria, they find character-

istic, separation; of derangement with knowledge, conflict; of manic-depressive insanity, domination; of paranoia, deviation; of the dementias, disorganization; of subnormality, arrest of development. The psychological significance of these terms is discussed and explained at length, and an attempt is made to bring them all into relation. "Judicial Diagnosis by the Association-method," by A. Binet, argues against unlimited confidence in the method for application in practice. The writer reviews the experiments of Henke and Eddy and of Yerkes and Berry, pointing out chances for error, and showing on both theoretical and practical grounds that the method, as used in the laboratory or classroom, is not suited to the conditions of the criminal court. The psychological literature of 1909 is reviewed by Beaunis, Binet, Bovet, Larguier des Bancels, Maigre, and Stern, under the headings of physiological psychology, sensations and movements, perceptions and illusions, associations, attention, memory and images, language, feelings, æsthetics, thought, suggestion, individual psychology, child psychology and pedagogy, animal psychology, judicial psychology, pathology, dreams, treatises and methods, and philosophical questions.

W. S. FOSTER

Il sentimento giuridico. GIORGIO DEL VECCHIO. Seconda Edizione. Roma: Fratelli Bocca, 1908. pp. 26.

Professor del Vecchio, of the University of Sassari, who has previously published several articles on kindred topics,—*L'etica evoluzionista* (1903), *Diritto e personalità umana nella storia del pensiero* (1904),—discusses in the present monograph the "feeling (or sense) of justice" in man, its origin and development. From the time of Aristotle down this "sense of justice" has been attributed to man, but the philosophers have disputed much as to its primary or derived character (these arguments the author briefly summarizes). According to Professor del Vecchio, "the origin and nature of the sense of justice is essentially a problem of the metaphysical order" (p. 12). This, however, does not prejudice in any way the analysis of the psychic datum and its proper functions. The "sense of justice" is thus "primary and normal datum of the ethical conscience, an element or an aspect of it; and its nature is affective and, at the same time, ideological." A fundamental and distinctive characteristic of the "sense of justice" is its independence of all exterior sanction,—that is just, which is right independent of all positive historical sanction. Thus justice and law differ. No prescription of law can destroy this original faculty of conscience to oppose itself, as supreme principle, to the authority of constituted law (p. 23), this, Hobbes to the contrary notwithstanding. The philosophy of law is rooted in the "juridical vocation of conscience." The "sense of justice" is "the anthropological exigence of law, its primary indication, and the psychic expression of its human necessity."

ALEXANDER F. CHAMBERLAIN

Sulla Craniologia degli Herero. DOTT. SERGIO SERGI. Roma, 1908. pp. 10. (Estr. dal *Boll. d. R. Accad. Med. di Roma*, Anno XXXIV, Fasc. I).

Contributo allo Studio del Lobo frontale et parietale nelle Razze umane. Osservazioni sul Cervello degli Herero. Pel DOTT. SERGIO SERGI. Roma: Fratelli Pallotta, 1908. p. 107, 1 pl.

In the first of these studies Professor S. Sergi gives the results of his examination (description, measurements) of 6 male crania of the Herero (a Bantu people of Damara Land, German Southwest Africa), now in the collection of the Anatomical Institute, Berlin. The skull-capacities range from 1,315 to 1,590 ccm. (4 are 1,500 or over); the cephalic indices from 67.5 to 72.9 (4 below 71). The Herero have a skull-capacity approaching that of the Kaffirs of the S. E. coast,—it is a curious fact that the Bantu peoples of the S. W. and S. E. coasts have a cranial capacity greater than that of those of Central Africa and the region of the upper Congo. The cephalic index of the Herero ranks them among the more dolichocephalic

Bantu. Previous to this paper, but two Herero skulls have been studied (one by Fritsch in 1872, the other by Virchow in 1895).

In his monograph on the brain of the Herero Professor Sergi treats in detail of 14 young adult brains (male 11, female 3) in the Anatomical Institute of the University of Berlin, with special reference to the frontal and parietal lobes. A few of these brains had been previously investigated in a general way by Waldeyer in 1906. Anatomical description and measurements are exhaustive: fissure of Sylvius, fissure of Rolando and the relative development of the frontal lobe, sulci of lateral surface of the frontal lobe, sulci of the orbital surface, fronto-parietal median sulci, sulci of the cranial surface of the parietal lobe, etc. Comparisons are made with similar data for other races, and the 8 figures in the accompanying plate demonstrate well the anatomical facts, by reference to the text-descriptions. The weights of the fresh brains range from 1,146 to 1,470 gr. (the 3 female are all below 1,200; 6 of the male below 1,300 and 2 above 1,400),—the Herero are said to average 1,800 mm. in stature, with head relatively small as compared with the body. Intellectually the Herero are inferior to the Hottentots, whose skeleton and musculature are of a finer structure (their average height is 1,700 mm.). Both Herero and Hottentots belong to the Bantu division of the Negro Race. Some of the facts brought out show how dangerous it is to generalize, *e. g.*, for "all Negroes," as Parker does with respect to the direction of the Sylvian fissure. In the method of termination of this fissure the Herero show 17 simple and 11 bifurcate, a proportion close to that of the Polish brains studied by Weinberg (Javanese, Swedish, Lett and Esthonian brains show a large majority the other way). The development of the upper and lower frontal lobe is more variable in males; and in both males and females more variable on the right than on the left. The absolute development of all the frontal lobe is greater in males than in females. The fissure of Rolando is more irregular on the right in male brains, on the left in female. In male brains left rami, in female right rami predominate. As has been shown for the *Hyllobates*, the facts indicate, according to Professor Sergi, the existence in the human frontal lobes of two distinct zones, an upper and a lower, which follow different laws of development. Of these "the upper left has in female brains reached its proportional development with respect to the other parts of the brain, while in male brains has still a considerable evolution to undergo" (p. 40). In the greater frequency of the separation of the inferior frontal sulcus from the precentral and the less frequency of a close anastomosis between them, the Herero brains differ from those of all other races so far examined. In the Herero the upper and lower frontal sulcus shows more divisions than in the European. With respect to the varieties of disposition of the retrocentral sulcus the Herero brains "reveal neither a condition of ontogenetic arrest, nor a phylogenetic record" (p. 83). While not venturing to draw any dogmatic general conclusion from the facts recorded, the author feels authorized to make this statement (p. 103): "The more rational analytical method for the determination of the variability of the cerebral sulci is still in its infancy waiting for the aid of microscopic research; and at present it can be asserted that we do not know a single morphological characteristic of the cerebral sulcature, which belongs exclusively to a given human race. But the frequency of determinate variations indicates sometimes the tendency toward oscillations and divergences, which, with certain limits, seem to be proper to a given human group; but more noteworthy still is the tendency toward the persistence of certain morphological characteristics of the cerebral sulcature in relation to sex independently of all ethnic differences."

A complete analytic study of all the Herero brains here considered will be found in Professor Sergi's more extensive monograph *Cerebra Hererica* to appear in "Ergebnisse einer zoologischen Forschungsreise in Südafrika mit Unterstützung der Kgl. Preuss. Akademie der Wissenschaften zu Berlin von Dr. Leonhard Schultze." ALEXANDER F. CHAMBERLAIN

BOOK NOTES

- Das Bewusstsein*, von JOHANNES REHMKE. Heidelberg, Carl Winter, 1910. 250 p.
- Philosophes et penseurs. Buchez (1796-1865)*, par G. CASTELLA. Paris, Bloud, 1911. 64 p.
- Institut de Sociologie, Bulletin Mensuel*. No. 1, Janvier, 1910. Instituts Solvay, Parc Léopold, Bruxelles.
- Philosophes et penseurs. Léonard de Vinci*, par LE BARRON CARRA DE VAUX. Paris, Bloud, 1910. 62 p.
- Kant and Spencer. A study of the fallacies of agnosticism*, by PAUL CARUS. 2d. ed. Chicago, Open Court Pub. Co., 1904. 107 p.
- Kant's Prolegomena to any future metaphysics*, edited in English by PAUL CARUS. Chicago, Open Court Pub. Co., 1902. 301 p.
- The fundamentals. A testimony to the truth*. Vol. 4. Compliments of two Christian laymen. Chicago, Testimony Publishing Co., n. d. 128 p.
- The concept of method*, by GERHARD R. LOMER. Published by Teachers College, Columbia University, New York City, 1910. 99 p. (Contributions to Education, No. 34.)
- Subconscious Phenomena*, by HUGO MÜNSTERBERG, THEODULE RIBOT, PIERRE JANET, JOSEPH JASTROW, BERNARD HART and MORTON PRINCE. Richard G. Badger, Boston. 1910. 141 p.
- On the genesis and development of conscious attitudes (Bewusstseinslagen)*, by WILLIAM FREDERICK BOOK. Reprinted from the Psychological Review, November, 1910. Vol. XVII, pp. 381-389.
- Transactions of the Congress of American Physicians and Surgeons*. Eighth Triennial Session, held at Washington, D. C., May 3rd and 4th, 1910. New Haven, Conn., published by the Congress, 1910. 456 p.
- The influence of complexity and dissimilarity on memory*, by HARVEY ANDREW PETERSON, PH. D. Dissertation, University of Chicago; Monograph Supplement, No. 49, of the Psychological Review. n. d. 87 p.
- Im Kampf um die Tierseele*, von J. VON UEXKÜLL. Separat-Abdruck aus Ergebnisse der Physiologie, 11. abt., hrsg. von L. Asher in Bern und K. Spiro in Strassburg I. E. Wiesbaden, Bergmann, 1902. 24 p.
- The first principles of heredity*, by S. HERBERT. London, Adam & Charles Black, 1910. 199 p.
- This compend on heredity has sections on the germ cell, theories, inheritance of acquired characters, of diseases, Mendelism and biometrics, with its conclusions.
- Text-book of nervous diseases for physicians and students*, by H. OPPENHEIM. Authorized translation by Alexander Bruce. Edinburgh, Otto Schulze & Co., 1911. 2 v.
- This is the fifth, enlarged and improved edition with 432 illustrations in the text and 8 plates.

Report of the committee of the American Psychological Association on the standardizing of procedure in experimental tests. Committee: CHARLES HUBBARD JUDD, WALTER B. PILLSBURY, CARL E. SEASHORE, ROBERT S. WOODWORTH, JAMES R. ANGELL, Chairman. Published by the Association. The Psychological Monographs, Dec., 1910. Vol. 13, No. 1. 108 p.

The evolution of mind, by JOSEPH MCCABE. London, Adam & Charles Black, 1910. 287 p.

The writer discusses the lowest and earliest forms of life, appearance of the brain, development of the fish, invasion of the land, insects and intelligence in them, mind in the bird, growth of the mammal brain, law of heredity, and advance in man.

Die innere Werkstatt des Musikers, von MAX GRAF. Stuttgart, Ferdinand Enke, 1910. 270 p.

This work treats of the unconscious, how it has affected romantic and classical productions in the field of art, the creative mode, artistic conception, outer impulse and inspiration, critical work, the sketch, technique, the classical and the great style.

Heredity in the light of recent research, by L. DONCASTER. Cambridge, University Press, 1910. 140 p.

The writer considers variation, its causes, a statistical study of heredity, Mendelian heredity, disputed questions, heredity in man, historical summary of theories, the material basis of inheritance. The work is all it claims to be, a simple introduction to the subject.

Vom Selbstgefühl, von ELSE VOIGTLANDER. Leipzig, R. Voigtlander, 1910. 119 p.

After a general characterization of self-feeling, the author gives its types, vital, self-conscious, etc. Then she discusses mirror-consciousness and its various forms. The writer's point of view is original and naïve. She goes to nature rather than to books for data.

Das vorgeschichtliche Europa, Kulturen und Völker, von HANS HAHNE. Monographien zur Weltgeschichte, herausgegeben von Ed. Heyck. Bielefeld, Velhagen und Klasing. 1910. 130 p.

This is a very interesting and comprehensive compend, with illustrations on nearly every page, the whole designed to give the beginner a general survey of the results, up to date, of the investigations into prehistoric times in Europe.

A study of association in insanity, by GRACE HELEN KENT and A. J. ROSANOFF. Reprinted from *The American Journal of Insanity*, 1910. Vol. LXVII, Nos. 1 and 2, 142 p.

This work, on the background of association in normal subjects, passes to that of a number of insane people, giving stimulus and reaction-words, and making careful generalizations from a large number of cases, and finally analyzing out certain symptoms.

Hereditary characters and their modes of transmission, by CHARLES EDWARD WALKER. London, Edward Arnold, 1910. 239 p.

This is an interesting text-book beginning with the cell and passing to instinct, theories of evolution, mutation hypothesis, continuity between species, protective coloring, law of frequency, immunity to disease, Galton's theories, environment, trypanosomes, ants and bees, Mendel's experiments, breeding, sex determinants, etc.

Zeitschrift für Psychotherapie und medizinische Psychologie, von ALBERT MOLL. Ferdinand Enke, Stuttgart, 1909. 1 Band, 384 p.

In this first volume we have a very imposing array of articles by eminent experts making original contributions to the subject. The references, too,

and the record of sittings, with a miscellaneous section, make a good and very interesting and attractive collection of view-points in a subject which at present is rather unusually lacking in harmony.

The age of mammals in Europe, Asia and North America, by HENRY FAIRFIELD OSBORN. N. Y., Macmillan, 1910. 635 p.

The writer has here brought together a very valuable report of the state of the topic under discussion, together with his own investigations which have been comprehensive. He divides his work by geological periods, eocene, oligocene, miocene, pliocene, and pleistocene, discussing under each the characteristics forms found in different countries. He does not enter the field of man.

The book of the animal kingdom; Mammals. By W. PERCIVAL WESTELL. London, Dent, 1910. 379 p.

Perhaps the best thing about this book is its many and excellent illustrations from life, too often, alas! life in captivity, of the many mammals described. A number of the best colored cuts are reproductions of extinct forms of life. Special attention is given, too, to the rarer and remoter forms. The work is rather popular, and approximates what a boy's animal book ought to be.

Individualism, by WARNER FITE. New York, Longmans, Green & Co., 1911. 310 p.

This book is four lectures on the significance of consciousness for social relations, given in 1909 at the Summer School in Chicago, but here very greatly developed. The author's general position is insistence upon individualism, *versus* the present emphasis laid by men like Dewey and Royce upon social relations. Indeed, the book is in part a friendly criticism of the views of these authors.

L'état mental des hystériques, par PIERRE JANET. Paris, Félix Alcan, 1911. Deuxième édition. 708 p. (Travaux du laboratoire de Psychologie de la Clinique à la Salpêtrière—Cinquième série.)

This is simply a reprint of the first edition of the first volume of the author's work which was published in 1893 and 1894 and it is here reprinted almost exactly, without change, because the author found that he must choose between this method and that of radically reconstructing his work, and because much of this volume is devoted to plain descriptions of cases valid under any theory.

Moto-sensory development: Observations on the first three years of a child. By GEORGE V. N. DEARBORN. Baltimore, Warwick & York, Inc., 1910. 215 p. (Educational Psychology Monographs.)

This is a study of the author's own child from birth up to the 152nd week. The frontispiece is the baby itself, and there are notes on observation, which constitute the bulk of the book; certain inductive considerations; a chronological epitome of observed development which presents salient facts in a condensed way; and lastly, the various first appearances are alphabetically arranged.

Mentally deficient children, their treatment and training, by G. E. SHUTTLEWORTH and W. A. POTTS. 3d ed. Philadelphia, Blakiston 1910. 236p.

This is a new edition of an almost standard work and contains some additional material. After an historical retrospect there follows a characterization of feeble-minded, degenerate and epileptic children; then comes a description of the instruction they require, pathological classification, etiology, diagnosis and prognosis, mental examination of children requiring special instruction, treatment, intellectual, industrial and moral training, recreation, with results and conclusions. An appendix lists institutions in England and America, gives speaking exercises, and a bibliography is appended.

Modern theories of criminality, by C. BERNALDO DE QUIRÓS. Tr. from the Spanish by Alfonso de Salvio. Boston, Little, Brown, and Co., 1911. 249 p.

This is an admirable survey and begins with origins, laying special stress upon Lombroso, Ferri and Garafalo. Then the writer discusses theories of anthropology, degeneration, pathology, sociology, parasitism, criminal tendencies. The book was written to furnish Spanish scholars and jurists with a general survey of what is being done in this field. This commission which has been given the author has been admirably executed by him.

Questioned documents. A study of questioned documents with an outline of methods by which the facts may be discovered and shown. By ALBERT S. OSBORN. Rochester, N. Y., The Lawyers' Co-operative Pub. Co., 1910. 501 p.

This is a comprehensive and excellent work on modes of testing handwriting and other topics therewith connected, describing how questioned documents of various classes have been cared for, the standard of comparison, photography, the microscope, alignment, pen position and pressure, writing instruments, variations in style, forgeries, disputed letters, ink, paper, folds, erasures, age of documents. The work contains over two hundred illustrations and is written mainly from the legal point of view without signs of much acquaintance, even in the brief bibliography appended, with the recent voluminous studies in this field.

Introduction to philosophy, by WILLIAM JERUSALEM. Authorized translation from the 4th edition by Charles F. Saunders. New York, Macmillan, 1910. 319 p.

This translation endeavors to help all who have a real interest in philosophy to an acquaintance with its language and its problems, and thus to stimulate independent reflection. The author's watchwords throughout have been "objectivity, perspicacity and brevity." The Germans have shown their appreciation by the fact that the book went through four editions in ten years. Its second aim is to examine the problems themselves and to make contributions toward their solution. The author's philosophy is characterized by the empirical view-point, the genetic method, and the biological and social mode of interpreting the human mind. He first treats preparation, principles, then criticises knowledge and epistemology, passes then to metaphysics and ontology, then to the methods of aesthetics, and finally to those of ethics and sociology.

Studies in the psychology of sex. Erotic symbolism; the mechanism of detumescence; the psychic state in pregnancy. By HAVELOCK ELLIS. Philadelphia, F. A. Davis Co., 1906. 285 p.

In this book the phenomena of the sexual processes are discussed before the attempt is made in the concluding volume to consider the bearings of the psychology of sex on social hygiene. Under erotic symbolism the author includes all the aberrations of the sex instinct although some have been deemed important enough for special volumes. Much stress is laid upon sexual equivalents. The mechanism of detumescence brings us to the final climax for which the earlier and more prolonged stage of tumescence is an elaborate preliminary. The art of love is that of preparation. The author, too, has treated at some length the psychic state of pregnancy, where the whole process in a sense reaches its goal. Woman in this state is "the everlasting miracle which all the romance of love and all the cunning devices of tumescence and detumescence have been invented to make more manifest." This is "the supreme position" which life has to offer and has its own problems. The book is full of suggestiveness.

Die Philosophie der Gegenwart; eine internationale Jahresübersicht. Herausgegeben von ARNOLD RUGE. Band 1 (Doppelband), Literatur 1908 und 1909. Heidelberg, Weissche Universitätsbuchhandlung. 1910. 532 p.

This is an extremely serviceable book. It divides philosophical literature into 12 departments, viz., (1) journals, creative works and dictionaries; (2) texts, translations and critical works; (3) history of philosophy; (4) general philosophy; (5) logic and theory of knowledge; (6) moral, social and legal philosophy; (7) philosophy of history, language and culture; (8) natural philosophy; (9) philosophy of religion; (10) art; (11) psychology; (12) more popular works, aphorisms and essays. Two reflections are suggested here. One is that this division of subjects is far too elaborate. In looking for some special work or article, the writer of this notice had to look through several of these rubrics before finding what he wanted, but the chief criticism of the arrangement is that psychology is given so small a place and that so many works one would naturally expect to find under this caption are found under philosophy.

The Journal of Animal Behavior. New York, Henry Holt & Company. Vol. 1, No. 1, January-February, 1911.

This is a new journal in a new topic. The editorial board is composed of Madison Bentley, of Cornell University, Harvey A. Carr, of The University of Chicago, Samuel J. Holmes, of The University of Wisconsin, Herbert S. Jennings, of The Johns Hopkins University, Edward L. Thorndike, of Columbia University, Margaret F. Washburn, of Vassar College, John B. Watson, of The Johns Hopkins University, William M. Wheeler and Robert M. Yerkes, of Harvard University. It is published by Henry Holt and Company, of New York, and the first number contains 77 pages. The first article is an experimental study of the turtle, by D. B. Casteel. Then follow articles on The Reactions of Mosquitoes to Light in Different Periods of their Life History, by S. J. Holmes; A Study of Trial and Error Reactions in Mammals, by G. V. Hamilton; A Note on Learning in Paramecium, by Lucy M. Day and Madison Bentley; and a note by Robert M. Yerkes on Wheeler on Ants. Save the last, there is nothing approaching a book review. We are glad to know that the Journal is to have a book-review department.

Spiritism and Insanity, by MARCEL VIOLETT. Swan, Sonnenschein and Co., 1910. pp. 134.

In the presence of spiritistic facts men react diversely. Sceptics deny everything *en bloc*; serious savants endeavor to apply their scientific methods, but up to the present their efforts have remained barren; the rest are essentially believers. An imperious need inclines them to accord a supernatural origin to what they cannot understand naturally. Such a method is risky, but where is the science which risks nothing?

At a spiritistic meeting the air fairly vibrates with mystery, and all believe; but when they leave the seance most become preoccupied with everyday affairs, and the belief has little practical importance. Far otherwise is the case with certain ones of instable mental equilibrium. Here we find those of congenitally weak intelligence, for whom life is at best difficult, who seek consolation in spiritism, and find there only new tortures. Here, too, are the paranoiac temperaments, those suspicious of others, inclined to beliefs in persecution, whose weariness of life leads them to spiritism. Here are the self distrustful and melancholiac, and especially the hysterical and neuropathic, who tend to become subjects. Here some bring actual insanity, senile decay, minds weakened by excesses, all of which give the best of soils for spiritism to grow in.

For the very core of spiritism is the mystery of its facts; not what the facts are, but that they seem to be without natural cause. But this is closely analogous to hallucination. Both appear abruptly, without transition, without progression, preparation or natural explanation, and as the hallucination tends to produce automatism in its subject, so does the spiritistic phenomenon produce it in the sitters, already predisposed, selected in many cases as we have seen, from the instable.

To the spiritist, evoking a spirit means to bring back the perispirit, which is the mean between body and soul and the Intermediary between us and the invisible volitions about us. The idea of this constant *entourage*, the uncertainty as to the power of these spirits, of their intentions towards us, of our possible displeasing of them, of our weakness and defenselessness against them—all this is hypothesis, but hypothesis which offers no barriers, which can never be refuted, and which opens to infinite other hypotheses the more it is considered. Its guarantees are the senses of the spectators and their unanimous consent to the dogma and doctrines. But it may be compared to delirium in these respects: it originates, like delirium, in a miraculous fact, and the consequences drawn from this fact are purely hypothetical. Whether it be actual delirium or not, it constitutes a vast culture infusion for all errors, disequibrations and madness.

So we get two classes of spiritistic insanities: 1, those evolving among the predisposed whose attack is referable to spiritistic preoccupations; and, 2, those who would have gone insane in any case, but to whom spiritism has given its own coloring.

Under the first come those exterior mediumnpaths, who are tormented by wicked spirits outside themselves; interior mediumnpaths, when the demon has taken possession. This possession may vary greatly in degree, the torments of the victim increasing until complete possession is attained, when depersonalization is complete, the delirium of greatness sets in, and the case passes into mediumnomania. The patient now considers himself a medium and is glad to be one, the spirit praises him, he has dreams of establishing a new religion, etc. All sorts of hallucinations may develop, and in the extreme stages the person loses all memory of his former self, perhaps even the notion of the existence of his body, becomes immovable and silent, but shows by the happy and calm expression, the ecstasy at which we can only guess. But some always remain melancholic.

In all these cases there are hallucinations, but in others there are none. Such cases rest their beliefs on illusions and delirious interpretations, especially the latter, and will evolve, according to temperament, towards melancholia, the delirium of persecution or of greatness. The last two are usually combined, the intelligence is intact on other points, and so the patient systematizes his delusion with much subtlety. His delusion becomes his curse, he is the persecuted victim, and he must defend himself by all means, often, in the end, by physical violence or even murder.

Spiritism may give a coloring to dementia præcox in its various forms, to general paralysis, to senile dementia, but these diseases would have evolved in any case.

In view of these facts a word of warning should be spoken, especially to the spiritists themselves: Sift your seances. Keep out the degenerate and unbalanced, and thus spare them possible madness and spiritism the discredit, danger, and fraud involved in having them for supporters.

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THERMAL INTENSITY AND THE AREA OF STIMULUS¹

By SARAH E. BARNHOLT and MADISON BENTLEY

At present the formulation of a general rule expressing the relation of sensational intensity to the area of stimulus seems to be impracticable. The obvious reason is the lack of uniformity in the facts; and this lack appears to arise from the diversity of organic conditions within the several senses. The spread or multiplication of a tactual stimulus upon the skin is different from the areal increase of light upon the retina, and both offer a mode of attack upon the organism which is again unlike that sustained under the numerical increase of tones or noises. The problem itself may indeed be given a single formulation, namely: does an areal or numerical increase of stimulus produce an intensive increment in sensation? But the solution must be sought separately in the individual departments of sense. And first of all, we must distinguish between those cases where the increase of stimulus leads to qualitative increase in sensation (*e. g.*, the musical chord) and those where it produces a like quality of different locality or different extent (*e. g.*, the spread of a tactual or gustatory stimulus). The problem under consideration falls within the second group of cases.

Stumpf² found occasion to discuss the matter in his analysis of tonal fusion. He divides the question into two parts, which he states as follows: first, 'is the intensity of a tone affected by the presence in consciousness of other tones?' and secondly, 'is a tonal complex stronger than each of its constituent tones?'

¹From the psychological laboratory of Cornell University.

²*Tonpsychologie*, ii, 1890, 416 ff.

In answer to the first question, Stumpf asserts that the individual tone is diminished, not augmented, by the presence of other tones; and that the decrease in intensity is a true weakening, not a phenomenon of attention. Heymans has since maintained that intensive 'inhibition' is general among sensory processes.¹ In answer to the second question, Stumpf denies the intensive summation of different tones. Two tones of like strength may be fuller, richer, or more 'voluminous' than one, but not stronger.

In vision, the matter is more complicated. Here one must inquire (a) whether binocular intensity is greater or less than monocular; (b) whether two disparate retinal areas, either of the same or of different eyes, produce an interactive effect upon intensity; and finally, (c) whether the enlargement of a single stimulus-area affects the strength of the resultant sensation. The facts are further complicated for vision by the peculiar relations of intensive to qualitative change. The dependence of visual sensation upon area appears in such problems as the spatial limen for colors and the sensitivity of peripheral vision, and in the facts of contrast and induction.

The intensity of pressure is obviously dependent upon the size of stimulus. But the mechanics of deformation plays an important part in determining the excitation of the tactual organs.² Von Frey found that the stimulus limen for moderate areas is approximately proportional to the surface affected and he lays it down that the neural excitation is a function of hydrostatic pressure. Brückner, who stimulated neighboring pressure-organs with the blunt end of a fine needle, found evidences of physiological (central) summation under simultaneous stimulation, even where the two points were discriminated.³

As regards temperature, the traditional view correlates the size of a heated or cooled area upon the skin with the intensity of the resulting warmth or cold sensation. This view is frequently supported by the observation that the finger plunged into warm or cold water gives a weaker sensation than the immersion of the whole hand or the entire body.⁴ The experience itself is undeniable. But whether the observation may safely be interpreted to mean that the higher

¹G. Heymans: *Untersuch. ueber psychische Hemmung*, *Zeitschr. f. Psych. u. Physiol. d. Sinnesorgane*, XXI, 321; XXVI, 305; XXXIV, 15; XLI, 28, 89.

²V. Frey, M.: *Untersuch. u. d. Sinnesfunction. der menschlichen Haut*, 1896.

³A. Brückner: *Die Raumschwelle bei Simultanreizung*, *Zeitschr. f. Psych. u. Physiol. d. Sinnesorgane*, XXVI, 1901, 33.

⁴E. g., E. H. Weber in Wagner's *Handwörterbuch der Physiol.*, iii, 2, 553. Weber explains in terms of cerebral summation. Cf. Stumpf, *Tonpsych.*, ii, 1890, 445.

intensity rests directly upon the exposure of a larger area is doubtful. It may rest upon (1) adaptation, (2) an augmentation of sensory feeling, (3) the presence or absence of such organic accompaniments as shiver, goose-flesh, or visceral displacement, (4) the addition of highly tuned temperature-organs, (5) the confusion of extent with intensity, (6) the confusion of pressure with temperature, or finally (7) the difference in cutaneous conduction over large and small extents.

The four methods used in our experiments were designed to test these various possibilities.

METHOD I. IMMERSION

The observer's eight fingers were ringed with indelible ink $\frac{1}{2}$ inch and 2 inches from the tips. The forearm was supported, and the hand was allowed to hang down in a natural position. Water was kept at a constant temperature (45° C.) in a small vessel, and the vessel was raised by the experimenter until the water reached either the first or the second ring upon a single finger. The observer was not required to move his hand or fingers. Immersion lasted one second. After it, the finger was dried gently and without friction, by applying an absorbent cloth. Then the same or another finger was immersed in the same way up to the second or first ring. The usual precautions against the constant errors of space and time were taken. The observer was asked to report which sensation was the stronger. Preliminary trials were made in which the danger of confusing extent and intensity was impressed upon him. In all but two out of forty-eight experiments, the two trained observers (H. and S.) reported an intenser warmth from immersion of the larger surface. This result confirms common observation; while, at the same time, the method makes it clear that the judgment of difference rests neither upon the repeated use of a common part-area (adaptation) nor upon the temporal order of comparison. Furthermore, the introspections indicate that the judgments were true comparisons of intensity, not of area. It is possible, however, that the finger-tips are uniformly less sensitive to warmth than the rest of the hand, and that the results reached under Method I. are to be referred to this difference. To eliminate this possibility, and also the strong suggestion of degree that arises from gradual immersion, a new method was devised.

METHOD II. CIRCULAR AREAS

An area 3.5×4.0 cm. was laid off on the palm of the hand or on the volar forearm and stimulated, under the procedure of paired comparisons, by a graded series of five brass cylinders, all of the same weight, and of diameters which ranged from

1.4 cm. to 3.3 cm. The cylinders stood at 5° C. (cold stimulus). Their flat circular ends were set down at all parts of the chosen rectangular area mapped upon the skin, so that the same temperature organs should be brought into function, in the course of the experiments, by both large and small cylinders. Altogether, 24 series of 10 comparisons each were made with three observers (H., S., and F.). Of the 240 comparisons, 170 (70%) gave an intenser cold with the larger areal stimulus (72%, 72%, and 67%, by observers).

The methods of immersion and circular areas have reduced our list of possibilities as follows. (1) Adaptation is eliminated by the successive stimulation of different areas, (2) affective indifference is secured by the use of small areas and of moderate intensities of stimulus, (3) organic complexes are avoided by the same means, (4) inequalities of "tuning" are partially compensated by the distribution of stimuli over a common field of exploration (method of circular areas), and (5) and (6) the confusion of thermal intensity with extent and with pressure is avoided by practice-series carried out under definite instructions.

The fourth possibility is only partially provided against; for the chances that a large stimulus will strike a highly tuned region exceed the chances for a small stimulus. And the greater physiological efficacy of a large stimulus through conduction (7) may account for the greater intensity of the large sensation. Let us first consider the fourth possibility.

METHOD III. WEAK AND STRONG AREAS

The direct comparison of areas of high and low sensitivity seemed to us the simplest way of arriving at the value of tuning in areal stimulation. So we selected a region upon the upper arm which gave, under the cylinders, a bright lively cold and an intensive warmth, and we then instituted a one-to-one comparison between it and a dull region on the ulnar side of the palm. Forearm and forehead were similarly compared. The smallest of the five cylinders was used upon the place of high sensitivity,—designated I_1 (cylinder-1 on intensive area),—and all the cylinders were compared with it, in haphazard order, upon the weak area (designated W_1, W_2, \dots, W_5).

The number of intensive judgments in which the *small-strong* area was sensed as colder (or warmer) than any one of the *large-weak* areas is set down in Table I. The initials of the Obs. are written at the top. Each place in the columns represents a total of six judgments. Thus I_1 was pronounced "warmer" than W_1 (the same stimulus set on a weak-warm area) 47 times out of a total of $6 \times 9 = 54$; I_1 "colder" than W_1

TABLE I

WARM	H		S			F		Total	COLD	H		S			F		Total		
I ₁ > W ₁	4	6	6	4	3	6	6	6	47	I ₁ > W ₁	6	6	4	6	6	6	5	5	44
I ₁ > W ₂	4	6	3	2	3	5	5	3	37	I ₁ > W ₂	6	6	1	3	5	3	3	4	31
I ₁ > W ₃	4	2	2	1	2	4	5	3	26	I ₁ > W ₃	2	5	0	2	3	5	2	2	21
I ₁ > W ₄	3	3	2	0	0	4	4	4	22	I ₁ > W ₄	3	2	3	0	3	4	2	4	21
I ₁ > W ₅	2	1	2	0	0	2	4	2	15	I ₁ > W ₅	1	2	3	1	1	2	2	2	14

(weak-cold area) 44 times out of a total of $6 \times 8 = 48$. But $I_1 > W_5 = \frac{1}{5} \frac{5}{4}$ (warm) and $\frac{1}{4} \frac{4}{8}$ (cold). Although equality-judgments were discouraged by the instructions and therefore appeared but rarely, it will be seen from the Table that subjective equality lies just above W_3 ; $\frac{2}{5} \frac{6}{4}$ and $\frac{2}{1} \frac{1}{8}$; *i. e.*, the thermal intensity from a cylinder 1.4 cm. diameter set in one certain region of high sensitivity is approximately equal to the thermal intensity from a cylinder of 2 cm. diameter applied to a certain other region of low sensitivity.

Thus the fourth item in our list of possibilities is accounted for by the discovery that a large area of low tuning can actually be made equal for sensation to a small area of higher sensitivity.

The seventh item is left. Size of stimulus seems clearly to be translatable into intensity of sensation. It remains, however, to be seen whether the translation is due to a process of summation or to the different conduction through the skin of large and small stimuli. The obvious recourse is to the individual sense-organ for cold and warmth.

METHOD IV. TEMPERATURE SPOTS

Three separate organs for cold were found within the radius of a few millimeters upon the volar forearm. A like group of warm-organs was also identified and mapped in the same general region. The organs in the cold group were designated as *a*, *b*, and *c*, those in the warm group as *a'*, *b'*, and *c'*. They were explored with blunt pointed temperature-cylinders held at approximately a constant temperature (water at 5° and 45° C). Again, we proceeded with paired comparisons, taking one organ with one, and one with two. Thus the intensity of each sensation was compared with that from the other two organs, taken singly and also together. The simultaneous stimulation of two spots was effected by the use of a double v. Frey stimulator, in which the bristles were replaced by smooth blunt copper wires of 1-mm. diameter. Adaptation was avoided by taking each organ only once in any comparison;

thus a was compared with b and c , never directly with a or ab or ac .

The following Table (II) gives the results of 515 experiments from three observers (initials at the left) for cold and warmth. The columns show the number of times each of the

TABLE II

COLD					WARM				
	$a > b$	$a > c$	$b > c$	Totals	$a' > b'$	$a' > c'$	$b > c'$	Totals	
H	20 10	25 5	23 6	89	15 8	22 2	14 9	70	
S	20 12	28 4	21 11	96	12 4	13 2	10 6	47	
F	15 9	21 2	15 8	70	9 7	15 1	13 3	48	
	55 31	74 11	59 25	255	36 19	50 5	37 18	165	
%	64 36	87 13	70 30		65 35	91 9	67 33		
	$ac > b$	$a > bc$	$ab > c$		$a'c' > b'$	$a' > b'c'$	$a'b' > c'$		
H	4 2	3 3	5 1	18	4 1	2 4	4 2	17	
S	3 3	3 3	5 1	18	4 0	2 2	4 0	12	
F	3 3	4 2	4 2	18	2 2	3 1	4 0	12	
	10 8	10 8	14 4	54	10 3	7 7	12 2	41	
%	56 44	56 44	78 22	309	77 23	50 50	86 14	206	
								515	

three temperature organs in the group returned the intenser sensation. Thus in thirty a - b comparisons, H. judged a colder twenty times, b colder ten times. In twenty-three a' - b' comparisons, H. judged a' warmer fifteen times, b' warmer eight times. Totals, recorded both in numbers and in percentages for each comparison, are added at the bottom. The last half of the Table gives the results for the one-to-two comparisons. Thus H. judged ac taken together colder than b four times out of six, and $a'c'$ warmer than b' four times out of five.

The relative tuning of the sense-organs is indicated by the following results; a was pronounced colder than b or c (taken singly) in 75.5% of the comparisons made, and a' warmer than b' or c' in 78%. The corresponding numbers for the other spots stand; $b = 53\%$, $b' = 51\%$, $c = 21.5\%$, $c' = 21\%$. Thus it becomes evident that the differences in tuning are considerable. The following order of sensory intensity may therefore be set down; a (or a') $>$ b (or b') $>$ c (or c').

Now we are in a position to discover whether reinforcement of one sense-organ by another occurs. If it does occur, we might expect to find $ac > a$ when both are compared with b , $bc > b$ when compared with a , etc. The one-to-one and the one-to-two comparisons are brought together in Table III.

TABLE III

COLD	% DIFF.	WARM	% DIFF.
A > A (b)	9	A' > A' (b')	5
A > A (c)	8	A' (c') > A'	12
B (c) > B	8	B' (c') > B'	15
(A) b > b	8	(A') b' > b'	19
(B) c > c	31	(B') c' > c'	41
(A) c > c	26	(A') c' > c'	44

The upper half of the Table shows the result of adding a second, *weaker* sense-organ to a first, stronger; the lower half, the result of adding a second *stronger* organ to a weaker first. For the sake of clearness, the stronger component is indicated by a capital letter, and the third letter added—in the one-to-two comparisons—is enclosed in parentheses.

It will be seen (upper half) that b or c (weak spots) added to A , or c added to B , decreases as often as it increases (3 to 3) the relative intensity of the cold or warmth; but that, on the other hand, the addition of a more highly tuned organ (lower half) to b or c invariably raises the intensity of the sensation. We are led to infer, therefore, that under the given conditions no sensible process of summation takes place; that the intensity of the temperature sensation is determined, instead, by the most highly responsive component in the excitation. This law may not obtain, of course, where two or more thermal areas are separately localized. Even within the narrow limits of a cylinder-area (Method II) the observers noted at times a plurality of colds or of warmths of unequal degree.

The apparent summation under our cylinders is very likely a matter of conduction. The excitation-value of a 1.5-cm. cylinder is different from that of one 3-cm. in diameter. In the first place, the thermal gradients from centre to periphery are unequal; and in the second place, the difference in temperature between the skin and the stimulus will naturally be

more quickly reduced with the stimulus of smaller area.¹ Our punctiform stimulus of constant area (Method III) eliminates both of these complicating factors.

Temperature-sensations seem to stand, then, as regards intensity, in the same case as tones. Without analysis, that excitatory factor which possesses the highest valence determines the intensity of the sensation. With (local) analysis, it seems probable that each mental constituent appears in approximately its own proper strength. Whether mutual 'inhibition,' however, tends slightly to blunt the constituent intensities under analysis, our results do not clearly inform us.

SUMMARY. The common observation that large surfaces are sensed as colder or warmer than small suggests that thermal intensity may be a function of the *number* of temperature-organs stimulated. But in working with individual end-organs we found no evidence of summation; we found, instead, that the strength of sensation is primarily determined by the most highly tuned of the organs involved. Tuning does not, however, wholly explain the common observation. After the consideration of six other possible factors, we conclude that the high intensity of the 'large' sensation is also owing, in part, to the more favorable conditions afforded by the stimulus of great area for conduction from the surface of the body to the true organs for temperature.

¹In order to demonstrate the difference of conduction we proceeded as follows. An ordinary thermometer reading to $\frac{1}{10}^{\circ}$ C. was laid upon a flat horizontal surface. A sheet of cardboard with a thickness slightly in excess of that of the lower end of the thermometer was cut to receive the bulb. Then a strip of pliable lamb's leather was stretched over both and tacked in place. The mercury bulb represented the organs of temperature and the leather the cuticle of the skin. The thermometer was brought to the temperature of the room (23.5°) and the five cylinders used in Methods II. and III. were brought to zero, and then applied in succession to the leather sheet just over the bulb. Each cylinder remained upon the leather for one minute, during which thermometric readings were taken every five seconds. The rate of drop in temperature accorded with the size of the cylinder, as is shown by the final readings, which stand for cylinders 1 (smallest) to 5 (largest): 1.6° , 2.1° , 2.7° , 2.9° , and 3.0° . The whole course of the twelve readings for each cylinder (every 5 sec. for 1 min.), when platted, also showed in a very striking way the difference in *physiological efficiency* of the five brass areas standing at one and the same temperature. A repetition of the observations indicated that the differences fell well within the probable error of observation. To make sure that the thermal gradient from centre to periphery is likewise a function of the size of stimulus, we took similar sets of readings with the bulb set directly under (1) the centre, (2) the middle of a radius, and (3) the margin of cylinder 5, and under (1) the centre, and (2) the margin of cylinder 1. The averages of two sets of final readings (initial temperature 21.0°) were the following: for cyl. 5, (1) $1.85^{\circ} \pm .15$, (2) $1.7^{\circ} \pm .10$, (3) $1.15^{\circ} \pm .05$; for cyl. 1, (1) $1.2^{\circ} \pm 0$, (3) 1.0 ± 0 . Thus it appears, even from our rough method of determination, first, that the total conductivity of the different areas is different, and secondly, that conduction within one and the same stimulus-area varies from centre to margin.

CONSCIOUSNESS UNDER ANÆSTHETICS.

By EDMUND JACOBSON

As yet it has been for the most part left to the surgeons to describe the nature of experiences under anæsthetics. They have worked out a semi-popular psychology, and thereby meet the demands of their practice with such success as a common-sense method can be expected to secure. But sometimes their problem becomes very delicate, and invites the subtlest refinements of scientific consideration,—for what they have to deal with, and what life itself depends upon, is the employment of tests for the presence or absence of psychoneural functions. Now objective tests for consciousness should, when possible, be confirmed with introspections from the subject, and in so important a matter as the present it is desirable that those introspections be as accurate and as detailed as possible. That the psychologists are commencing to assist in this introspective task is shown by the fact that three articles, recounting personal experiences and attempting to make generalizations, have recently appeared in their journals. That many more will be needed is evident; and therefore we add the present report to their number. Certainly the most startling thing we have to describe is that the patient was conscious under the anæsthetic at the very height of the operation.

The case to be cited occurred at Wesley Hospital of Northwestern University in October, 1910, upon the occasion of an operation for the removal of the appendix under the influence of nitrous oxid and air. The anæsthesia lasted sixteen minutes. With reference to the patient's physical and mental condition at the time, it is important to remark that he had not recently been ill, so that at the outset both physical and mental condition were normal, except in so far as he was stimulated by the serious and novel situation. Hence attention was unusually keen, and the subsequent memories clear and detailed; so, for illustration, the *minutiae* of the conversation between two of the operators concerning the movements of the meniscus of the sphygmomanometer which they were preparing for use are still, after several months, vividly recallable. It should be added that the patient had previously half jestingly remarked that he might take subjective observations;

for this virtually constituted an *Aufgabe*, and hence was one of the factors which determined his mental attitude toward the experience.¹

With some abridgment and alteration to suit present purposes, we shall quote from memoranda which were not written down until from four to six weeks after the operation, but which nevertheless are, as we believe, accurate. Here and there we shall interrupt the account with explanatory comments. The experienced reader will readily distinguish the points at which we are unable to give direct descriptions of the psychic processes, and therefore are obliged to substitute impressionistic indications or statements of meaning.

"After I had been placed upon the operating table and while preparations were under way, I remember that I conversed with one of the surgeons, that he inquired whether I was nervous, and that I answered 'No.'

"The time came for the application of the gas. I was told that air would be given first, and then the gas gradually mixed with it, but that I should not be able to detect the latter, since it was odorless. Then the bell was put on, leaving my eyes partially or wholly exposed. My left arm was extended, and rested in the blood pressure apparatus, but the hand grasped the wrist of one of the surgeons, Dr. P. I breathed and waited. Nothing happened for a short time, and I squeezed the wrist to show that I was fully awake. Presently I detected the oncoming of the gas, and I squeezed the wrist again, and silently thought that they were mistaken in saying that the gas had no odor. I breathed in the gas for a little time and worried slightly because it did not seem to affect my consciousness. Then the gas began to operate, and I reacted by squeezing the wrist. There was no sense of suffocation, nor again of giddiness. Objects commenced to slip from the mental grasp, and there was a sort of blurring of those that remained in consciousness. My central thoughts remained perfectly firm. Then came a striking experience. My eyes were open, and though the bell occupied the centre of the visual field, yet it did not obscure the rest of the room. Now gradually the sight began to alter. The outlines of things became blurred, and at the same time the perspective began to disappear, until the field became perfectly flat, and what with the four or five heads of the operators, as they appeared arranged around the bell, the whole (apart from the blurring) looked like a picture of the early Florentine school; though of course I make this comparison now and did not then think

¹Habits of continual self-observation, also, will have to be taken into account.

of it. A moment later and this also was gone and I saw no more; excepting that very nearly colorless lights danced outward accompanied by a dull buzzing. I said to myself, 'I don't care; I don't care.' This was in some measure true and spontaneous; but also, in part, it came as a deliberate auto-suggestion, and was followed shortly after by a fleeting and half-developed realization that the suggestion was not working negatively.

"At one time, when consciousness was perceptibly affected, the hand of an assistant was pressing heavily, and I muttered a protest; and when some one said 'Keep quiet,' I laughed in a guttural manner to show that I was so. At another time—I do not know precisely when—after vision had gone, the surgeon laid his hand on the body, and I cried out, 'Not yet! I am awake!' Again, after I was no longer able to squeeze the wrist, I tried to show that I was awake by muttering 'um—um' with each expiration. While I was able later to recall that I had squeezed the wrist and had laughed, the facts of having muttered and of having exclaimed that I was awake did not recur to me until I was told of them.

"There were other thoughts during the administration of the anæsthetic which now escape recall. I remember with great clearness, however, the way in which the gas seemed to affect consciousness, for I was deeply interested in it at the time, and described the processes to myself even while they were occurring, though not always in verbal terms. So that finally, when things had almost all fallen away, I said to myself, very, very slowly, 'Dim-in-ish-ing, dim-in-ish-ing con-scious-ness!' That was the last word, and I knew nothing more; but as these words appeared there occurred an intellectual process, which contained no distinct verbal images, and the meaning of which was, 'Your personality must be psychological at its core, if you think of such things at this moment'."

There came a break in consciousness here. The experiences which we are about to describe are more or less disordered and confused. Subsequently, in recalling them, they appear without temporal setting, and as more or less disconnected from the experiences during the waning of consciousness.

"'Are you ready, Doctor?' 'Just a minute, please.'"

"Possibly there was a dull whirring and buzzing; voices moved to and fro; my ideas were confused and troubled. I did not rightly know where I was, nor what was happening. One thing stood out clear; there, in the right side,—the pain!

¹When later I repeated the remark to Mr. B., I learned that it was addressed to the anæsthetist; and therefore it doubtless meant that I should soon be in the state of deep anæsthesia. I was not able of myself to recall when I heard these words.

It was sharp, griping; it seemed to be drawing the whole body to that spot. It was agony. I have never endured, never before even imagined such intense torture. I groaned again and again, in helpless, uncomprehending protest.

"I had many troubled dream experiences, which afterwards I could not recall, yet knew that I had had them. The pain lasted long, long, and around it my dreams centered. Suddenly—I do not know just how suddenly—I realized it all: This agony I cannot escape; I am being operated upon! I am here on the operating table! And I am conscious!

"Conscious! I tried voluntarily to suppress the pain and seemed in some degree to succeed. I stopped groaning.¹ I was thinking now, perhaps with breaks and disordered interruptions, but yet in fair measure logically and coherently. I am under the anæsthetic and I am conscious! It is secondary consciousness! Amnesia will follow. I must try to remember what is happening in order afterwards to relate it and prove that I was conscious. At about this time I spoke aloud to those about me, exclaiming, 'I've made a discovery; I've made a discovery! The secondary consciousness—.' According to my subsequent memory, I said three words more before I stopped without completing the sentence. It seemed as if I had said, 'The secondary consciousness is the primary consciousness—' and I intended to go on and say that the same *I* was present in both, and perhaps to say something else, which I have since forgotten; but I ceased, owing to the difficulty of putting the matter into words and owing to lack of strength.²

¹In this connection it is interesting to note that Dr. C. reported, 'There was a time when you (the patient) seemed to reconcile yourself to it, and you stopped groaning.' I report this voluntary suppression because I have a memory of it; it is not, however, in accord with my waking experiences, since I am not ordinarily successful in suppressing pain in this way.

²Those present state that I never mentioned 'primary consciousness' at all, but that I repeatedly said 'secondary consciousness' in rapid succession. In subsequent conversations with the surgeons I stated that I had used the term 'secondary' not in the usual sense (*i. e.*, of double consciousness), but rather to signify a type separated from primary or waking consciousness by amnesia. I trace the associative source of this idea to reading certain passages in Bramwell a few weeks before, though that author of course observes the customary usage. (Especially p. 390, *Hypnotism*. London, 1906.)

I had never had any anticipation that I should be conscious during the operation. For the sake of completeness a trivial incident may be mentioned in this record; a week before the operation a layman had asked another in my presence whether anæsthetised people ever felt anything and had got the answer, 'No.' The incident passed out of my mind at once and did not recur to me until after the operation, when I tried to recall what remarks about anæsthetics I had recently read or heard. The incident is entirely negligible, I believe, and I mention it only because the record requires.

“‘Stop the anæsthetic!’ It was probably just a little after this was said that marked changes took place. The griping pain, sharply located in the region about McBurney’s point, was giving place to a very mild pain, different in quality and referred to the region about the umbilicus. The latter was quite tolerable, and was the kind of pain which in stronger form remained more or less continuously for about thirty-six hours after the operation. I was ‘waking up,’ and as audition was present, I commenced to make a series of remarks which ceased when vision began to come back. Most striking was the reappearance of the visual field,—at first like a flat (Florentine) picture, and then gradually regaining perspective and clearness,—just the reverse of the initial experience. It was this that prompted me to say aloud, ‘It all ends just as it began!’”¹

About two minutes after the stopping of the anæsthetic, the patient was fully awake and rational. He entered into conversation about his experiences at once, suggested to the internes not to make the bandages too tight, etc. Though talking was very difficult, this conversation was for a while continued after the patient had been put to bed. Temperature and pulse fairly soon became normal, and recovery was very rapid. The case had no subsequent history, though it may be mentioned that during dozing in subsequent nights, there occurred three local nervous spasms, which were painful and accompanied by slight psychical disturbance.

Excepting where otherwise specified, the report quoted is based upon the memories of the patient, and there is evidence that they are on the whole satisfactorily faithful. In support of this may be mentioned that he repeated to the surgeons that he had laughed and had periodically squeezed the wrist he held; although it came as news to him that eventually the fingers had closed about the wrist with a vise-like grip. At various places in this paper we specify things not recalled at all, and also things recalled only after others had first mentioned them. Most important is it to remind the reader that, while the operation was going on, the patient determined to remember what was happening, in order later to be able to prove that he had been conscious. This was virtually an auto-suggestion to remember, and in the light of current knowledge, we should expect it to have efficacy; for it is well known that the amnesia which characteristically follows deep hypnotic states can be prevented, if suggestion to remember be given while the state is in course; and again, in normal psychology it has been experimentally verified that the intention to remember and to relate psychological experiences has a similar efficacy. (Messer, A. *Experimentell-psychologische Untersuchungen über das Denken*. *Arch. f. d. ges. Psy.*, 8, 1906, 20. Also, Ach, N. *Ueber die Willenstätigkeit und das Denken*. Göttingen, 1905, p. 11.) Therefore it is particularly interesting to state that immediately after waking, the patient was able to enter into conversation about “secondary consciousness,” “discovery,” and the like, without needing to be informed of having spoken aloud of these things. Furthermore, his memory of the question “‘Are you ready, Doctor?’” and the answer “‘Just a minute, please’” was corroborated by Mr. B., who was

¹This accords with the statement of Hewitt, “‘When the administration is discontinued and fresh air admitted to the lungs, a kind of retrogression in the patient’s symptoms commences.” *Anæsthetics and their Administration*. London, 1893, p. 327.

present during the operation. But the most interesting verification of all was the repetition of the groan to Dr. C. and Mr. B., who were able to recognize it without a doubt because of its very peculiar character. It was the kinæsthetic-auditory memory-image of this groan and the auditory image, "Are you ready, Doctor?—Just a minute, please," that the patient found associated with the memory of his conscious determination to remember.

We have some brief notes kindly dictated to us by Dr. G. T. Courtenay and Mr. J. R. Buchbinder, both of whom assisted in the operation, but unfortunately not given until three weeks after the operation and therefore somewhat inaccurate and incomplete. We give them, however, since they are all that we have; for the sake of clearness we have somewhat altered their style.

Dr. C.—Before the incision the patient said, "I am not asleep," again and again. Dr. R. put his hand on the abdomen and he said, "Not yet! I am awake!" He was not thought to be under as yet. He had been muttering. The incision was made. During this time, and up to the moment when the operator came to the appendix, there was no muttering and nothing was said, but there was muscular rigidity. Then the patient cried, "Oh—h!"—groaning. As I remember it, he then cried loudly, "I am awake!" and repeated this several times. While the skin-clips were being put in he exclaimed, "It's the secondary consciousness! I have made a discovery!" (See also p. 336.)

Mr. B.—While the gas was being given to induce anæsthesia, the patient said nothing. After two minutes Dr. R. said, "Are you ready, Doctor?" (When the writer reminded Mr. B. that the answer came, "Just a minute, please," he recalled the remark and supplied the information that it was addressed to the anæsthetist.) Before the incision there occurred inarticulate muttering. Probably during or after the incision (I cannot positively say which) the patient said, "I am conscious." A minute or two elapsed, during which the operator was getting the tip of the appendix and the patient was quiet. While the cæcum was being manipulated he said, "I have made a discovery;—an important psychological discovery!" This was repeated many times, each one faster than before. The appendix was cut off and the patient squeezed the wrist of Dr. P. with a very strong grip. Then he repeated "important discovery" and "secondary consciousness" a few times. (It will be recalled that the report of the patient shows no remembrance of the phrases having been thus repeated. In this respect Mr. B.'s report supplements the former, and is without question correct; but we doubt that the terms "important" and "psychological" were used.) After the first skin-clip had been put on, the administration of the anæsthetic was stopped and the suggestion given, "You can wake up." During the last few minutes the patient was quiet, and after waking said to me, "I've made some interesting observations. Did you take notes?" (Mr. B. amended the above account after the patient purposely repeated the groan for him. Like Dr. C., he was able to recall with certainty that it occurred, but unlike him, was not perfectly sure when it occurred, adding, however, that the balance of probability seemed in favor of its having occurred after incision.) During the operation the blood-pressure, as indicated by the sphygmomanometer, remained constant.

Now let us proceed to general matters.—It is more or less the custom, in treatises on anæsthesia, to include an account of the order of disappearance of the functions of the nervous system. So, for instance, Patton says, "There is irritation, depression, and finally paralysis of the nervous system. The cerebral cortex, the cerebellum and ganglia of the base, the

sensory tracts and centres of the cord, the cerebrospinal motor tracts and centres, and the respiratory and cardiac centres seem to be affected in the order mentioned."¹ Experimental and clinical observations have led to formulæ of this sort, which therefore must have at least a certain rough validity. Everyday clinical experience makes it familiar that circulation and respiration almost always remain when other functions have failed; and it is equally certain that the loss of the conjunctival reflex is a useful indication of the loss of various other nervous functions. There are other gross correlations of this sort, the validity of which is fairly beyond challenge. The principle of such correlations is, therefore, true and useful—if not carried to the extreme. But the current formula that the nervous functions disappear in hierarchical order cannot, we believe, be completely relied upon. It appears that sometimes, at least, a higher function may remain even when certain lower ones have gone; that the absence of certain lower functions is not an invariable guarantee of the absence of all higher ones. In evidence of this may be adduced our own case as a fair example of the highest psychoneural function, namely intellection, remaining present even when lower ones such as vision were in abeyance. Nor does this observation seem to be exceptional, for the same thing is reported, with respect to the period of the waning of consciousness, not alone in the psychological papers of Jones, Johnston, and Hill, but also in Hare's article in *Keen's Surgery*². Therefore we seem warranted in generalizing, at least with respect to persons who make habitual use of the higher intellectual functions, that *generally these persist even after vision and other psychical and physiological lower functions have gone.*

As to the psychological situation, there seem to be some strange misunderstandings (or else carelessnesses) in certain

¹Patton, J. M.: "Anæsthesia and Anæsthetics." Chicago, 1905, p. 30.

²"After all sensations were damped down completely there still remained an inner consciousness which for the most part was perfectly normal. Memory seemed pretty accurate, and the reasoning powers only slightly deficient." Jones, E. E. *The Waning of Consciousness under Chloroform.* *Psy. Rev.* 16, 1909, 53-54. "The special sense organs become inactive long before general consciousness is lost." (Speaking of the process of recovery of normal consciousness, the same writer says, "Feeling is first reinstated. Purely intellectual activity—is next in order." Johnston, H. J. *The Rôle of Sensations and Feelings under Ether.* *Jour. Abn. Psy.* 4, 1909, 29. "—there —remained to the final fading out of conscious experience an awareness of personal identity." Hill, Prof. and Mrs. D. S. *Loss and Recovery of Consciousness under Anæsthesia.* *Psy. Bull.*, 7, 3, 81, "—chloroform, after a brief quickening of the pulse and of respiration, causes a gradual decrease in the activity of the perceptive portions of the cerebrum, followed or accompanied by a similar obtunding of the intellectual activities." Hare, H. A. *Keen's Surgery*, Philadelphia, 1909, V, 1019. Similarly in the case of ether, 1027.

of the surgical works. For example, in speaking of the second degree of general anæsthesia, Patton states that it is a "stage of unconscious reflex activity."¹ Here, as he says, he is following Hewitt, and therefore we shall quote the views of that writer.² "Second Degree or Stage.—(Ether) Loss of consciousness takes place abruptly. The patient passes into a condition in which, although memory, volition, and intelligence are abrogated, he will readily respond to stimuli. The response may have all the appearance of conscious response. (N. B!) Questions may be answered; but the answers will be nonsensical."

If we understand the above passage rightly, it means that a time comes at which the patient is to be considered unconscious, yet at which he will give answers to questions as well as *simulate* conscious response in other ways. This view is so naïve that the psychological reader will not demand that we argue the matter. Hewitt also says, "Laughing, struggling, shouting and singing may be met with at the commencement of this stage if the administration be slowly conducted." We scarcely believe that he wishes to consider these reactions also as unconscious. But without making assumptions in this regard, we may state in reply that, by virtue of analogy with our general psychological experience, there need be no doubt that the patient is conscious when such things occur as answering questions, shouting, singing, talking, laughing, or true groaning.³ It is absurd to call a stage which may be characterized by the presence of such reactions one of "unconscious reflex activity." Such a use of the term "unconscious" is lamentable, for it contains the confusion, sometimes popularly made, between absence of intelligent response and absence of conscious response.⁴

¹*Op. cit.*, 33.

²*Op. cit.*, 153.

³We use the expression *true* groaning in order to exclude stertor and also expiratory noises due merely to occlusion of the air and vocal passages by tongue, mucus, or other foreign substance.

⁴When a reaction is nonsensical, this indicates the presence of disorganization in consciousness; the processes do not function as usual, relatively to each other. Complete nonsense would mean utter disorganization of conscious processes, but it would be incorrect to take it to mean complete absence of conscious process. This fact being clear, there is left an inviting problem for investigation by psycho-analysis as to whether the utterances of patients under ether or chloroform delirium have not a "latent meaning" under their apparent absurdity. Apropos of this, the writer recalls the deep significance possessed by his groan. It meant uncomprehending and helpless protest against the pain. Physiologically it was the development or consummation of the process of uttering "um—um," for it was made with the same laryngeal adjustment, except that instead of an almost constant pitch it had a large rise and fall, was much higher, and much more prolonged. The former utterance had the meaning "Not yet! I am awake!"

In the light of these considerations, and if even highly organized consciousness has the tenacity which we have indicated, it is apparent how great ought to be the caution in judging that the patient is unconscious on the ground that certain reflexes are absent and notwithstanding that other ones are present. To be sure, with sufficiently deep *ether* or *chloroform* anæsthesia, a condition may be attained which is suggestive of natural sleep; respiration and circulation are the chief visible activities that remain; there is quiet (except for stertor) and relaxation, and the spinal reflexes are largely absent and inelicitable. When things are so, it seems reasonable enough, on grounds that we shall formulate later on, to assume unconsciousness; for although this conclusion cannot be proved with absolute certainty, yet at any rate it has high probability.¹ But there are statements in the surgical books which go farther and assume the absence of consciousness even when such quiet and absence of function are not attained. So, for example, the *International Text-Book of Surgery*, in arguing for a sparing use of ether in prolonged operations, says: "A few whiffs of ether now and again will keep him free from pain, anxiety and fright. As he knows little or nothing, a moderate amount of involuntary struggling unattended with suffering does no harm."² We submit, however, that it is not safe generally to affirm that when a "moderate amount of involuntary struggling" is present the patient nevertheless "knows little or nothing." It is a very delicate matter indeed to say when struggling occurs unaccompanied by the conscious functioning of the higher nerve-centres. For if, on the one hand, it is fairly certain that such functions as answering questions are always accompanied by higher consciousness, while, on the other, it is also fairly certain that such functions as circulation are accompanied at most by only a very low form of consciousness, yet the most that it seems fair to concede with regard to struggling is that it can upon occasion belong to either class of experiences. Why this is so will be made clear in the following paragraph.

Jactitation is a phenomenon likely to occur when nitrous oxid is given without oxygen or air and is described, for example, by Luke, as consisting of "clonic muscular contractions commencing in the orbicularis palpebrarum and extending to the limbs."³ If, as a test case, one is looking for reasons why

¹In judging depth of anæsthesia the surgeon is guided by observation of some or all of the following: the respiration, the occurrence of swallowing movements, the lid-reflex, the state of the eye and pupil, the pulse, the color of the face and lips, the rigidity of the skeletal muscles (Hewitt).

²Phil., I, 1902, 448.

³*Guide to Anæsthetics*, Phil., 1906, 19.

this activity should be considered unconscious or conscious, the important fact must be noted that under normal conditions it cannot be consciously initiated (at any rate not in the absence of a preliminary learning process). By analogy with normal experience, therefore, there is no compulsion to assume that jactitation is a conscious phenomenon. And in general it may be said that *there is no logical requirement to assume consciousness in case of activities which under normal conditions are incapable of conscious initiation.*¹ Again, if one is considering another class of functions, those, namely, of the autonomic nervous system,—circulation, respiration, secretion, etc., what needs to be taken into account is that under normal conditions these do not require the attendance of consciousness in order that they may go on. By analogy with normal experience, therefore, there is also no compulsion to assume that these phenomena under anæsthesia are accompanied by consciousness. Or, to put this matter also generally, *there is no logical requirement to assume consciousness in case of activities which under normal conditions may go on without conscious attendance.* It is now clear why it is permissible to assume that deep states of ether and chloroform narcosis are probably unconscious; for all the activities that are observably present in these states are either such as may under normal conditions go on without conscious attendance or else such as are normally incapable of conscious initiation. On the other hand, the phenomenon of struggling does not fall into either of these two categories; for under normal conditions it can be consciously initiated and it never occurs without conscious attendance. Therefore there is an element of hazard in calling it unconscious at any particular time; and at all events, when it occurs concomitantly with shouting, true groaning, or the like, it should—like these reactions themselves—be regarded as conscious.

The mere fact that the patient is subsequently without memory of his reactions must not be assumed to prove that consciousness was absent. For his psychophysiological state is so disturbed—not alone during the administration of the anæsthetic, but also usually for some time thereafter—that memory may be expected to be deficient. So it is in the case of Hill, whose record shows that there was a period following the first signs of awakening during which he did such conscious things as calling for air and chattering more or less irrationally about his experiences, but of which he subsequently had no recollection.² The fact of forgetfulness was noted by Buxton in his discussion of chloroform narcosis.

¹*I. e.*, voluntary initiation.

²*Op. cit.*, p. 79.

"In the second stage [Buxton recognized five stages] the mental powers are impaired although not suspended. . . . As a rule struggles or experiences of pain which show themselves at the time are not subsequently remembered."¹ A further reason for presuming that experiences during gas, ether, or chloroform narcosis might not be subsequently recalled, even if conscious at the time, is that amnesia characterizes kindred psychophysiological states, namely deep hypnosis, deep alcoholic intoxication, and the dream states of natural sleep. This tendency toward amnesia, which we must therefore recognize, is an obvious hindrance to proving that the patient was unconscious. One must give him a fair chance to remember: Follow the example of the workers on deep hypnosis: experimentally produce a state of ether narcosis in which involuntary struggling, groaning, and the like occur; during this state or previous to it, get *en rapport* with the patient and suggest to him that he will remember all that occurs; next, quickly bring him back to normal consciousness, avoiding post-anæsthetic disturbance as much as possible, and learn whether the patient then retains any memory!² If he does not, this is evidence that he had no high form of consciousness; not an absolutely conclusive test, however, since a low or disorganized state of consciousness might be present and yet fail to respond to suggestion. But at any rate, in the absence of experimental tests of this kind, it is a risky hypothesis to assume that such things as struggling and groaning in anæsthesia are other than what they are in normal activity, namely, signs of unpleasant consciousness.

In this connection, attention may be called to the fact (recognized, for instance, by Hewitt) that nitrous oxid does not give that complete freedom from reflex movement and phonation which characterizes the third degree of ether or chloroform anæsthesia.³ Or again, as A. D. Bevan recently put it,—“(Nitrous oxid) anæsthesia is not as profound as that of ether or chloroform, and the occasional talking of the patient may be disconcerting to one not familiar with the method.”⁴

¹Buxton, D. W.: *Anæsthetics*, London, 1888, 69.

²Anæsthetics characteristically produce a state of heightened suggestibility. So in clinical work it is found advisable not to do anything to or say anything before the patient during the waning of consciousness that might act as a harmful suggestion. The close relationship which anæsthetic narcosis bears to such a state of heightened suggestibility as hypnosis is shown on the one hand by the fact that chloroform, for instance, may be used as a decided aid to suggestion in inducing hypnosis (*e. g.*, Bramwell, *op. cit.*, p. 45), and on the other hand by the fact that suggestion may be used as a decided aid to chloroform in inducing surgical anæsthesia (*e. g.*, Munro, H. S. *Influence of Suggestion as an Adjunct in the Administration of Anæsthetics*. *St. Louis Med. Rev.*, Nov., 1908.)

³*Op. cit.*, p. 110.

⁴*Jour. Am. Med. Ass.*, 1907, 49, 3, 197.

What seems to us the probable significance of the presence of such reactions has already been sufficiently indicated. Talking does not possess the distinguishing marks that belong to activities which may permissibly be considered as unconscious; for under normal conditions it is capable of conscious initiation and it never occurs without conscious attendance. Therefore the conclusion that it is unconscious when it occurs during anæsthesia is unwarranted. If nitrous oxid anæsthesia is characterized by the presence of such reactions, it does not seem warranted to believe it to be a state of continuous unconsciousness. Nor, as previously indicated, should the assumption of unconsciousness be made with regard to chloroform or ether anæsthesia unless these drugs be given in sufficient quantity to suppress "involuntary struggling" and the like.

It goes without saying, however, that the psychologist can have no opinion on questions concerning the choice of an anæsthetic for a given operation, or again, concerning the advisability of permitting slight consciousness in prolonged operations in preference to running the dangers of exhaustion and collapse which prolonged and deep anæsthesia involves. Matters of practice concern only the surgeon. The sole interest of the psychologist is to analyze the mental situation.

Before closing the paper, a few words of discussion may be added in regard to the psychological articles that have recently appeared. Jones presents the record of three experiences under chloroform, two of which were produced for observational purposes alone, and with the aid of simple laboratory devices. His account is therefore more full than could otherwise be the case, and is important because of its qualitative descriptions of sensory processes and its tests as to the order of disappearance of mental functions. That order was: hearing, touch, gross muscular movement, highly specialized movement (fingers), vision, reasoning, memory. To be sure, Jones fails to state whether this order was precisely maintained in all three cases, and one is frequently at a loss to know whether a given phenomenon that he describes occurred in only one of his experiences or in all three. It is to be regretted that he made the methodological error of failing to be clear and full as to what were their similarities and differences. Hill is especially interested in marking the similarities and differences between his experiences and those of Jones. If we have not misunderstood the latter's paper, he was awake—able to reason, and remember—after muscular control had disappeared.¹ If so, then Hill scarcely seems justified in saying that "the

¹*Op. cit.*, p. 53-4.

persistence of motor ability in the observation of both Jones and myself as witnessed in the waning of consciousness attests its fundamental position, and also that artificial sleep as well as natural sleep. . . is most closely related to the cessation of voluntary motor ability." If one is seeking a key to the explanation of sleep, and regards 'persistence' as the road to finding it, why select 'voluntary motor ability' in preference, say, to voluntary memory,—which in Jones' case was the more persistent? It is relevant to mention the case of the patient described by Johnson, who becomes awake (to the extent of having various sensations and feelings) at a time when the limbs were not under control.¹ To be sure, if voluntary motor ability is absent, one would *ipso facto* not expect normal consciousness to be present. But if anything further than this is to be established, and if voluntary motor ability is to be exalted over its brother functions, a wider range of evidence should be offered than that given by Hill.

Johnson's paper is valuable—among other things—for its description of the waning of voluntary inhibitory power, wherein it resembles that of Hill, but differs from that of Jones and our own since the loss is not reported by the latter two. The order of disappearance of functions, also, differs from that of Jones: vision went before touch.² Johnson terminates his paper with a discussion of the nature of feelings, imageless activity, and the like; but these are general matters, better left for laboratory investigation. Efforts should be focused on the attempt to understand the anæsthetic experience itself, with special endeavor to describe minutely the conscious events in their temporal order, and in addition, when possible, to state the physiological concomitants.³

¹*Op. cit.*, p. 23.

²This suggests the matter of individual differences, which vary greatly with (1) the anæsthetic used, and (2) the temporary and permanent psychophysiological conditions of the patient. An adequate account of individual differences cannot yet be written, although the surgical books furnish some material. Of particular practical importance would it be to determine how frequently the pain sense persists during the second and third stages.

³An excellent list of problems has been published by Jastrow. *Am. Med.*, Philadelphia, 1905, X, 202. Same also in *Pacific M. J.*, San Francisco, 1906, XLIX, 140.

ON THE INTENSITY OF IMAGES¹

By ALMA DE VRIES SCHAUB

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INTRODUCTION

Until recent years the experimental investigation of images has been comparatively neglected, and even at the present time the subject does not seem to receive either the extended or the detailed study that is given to sensation. Thus, while there have been studies of the general nature of mental imagery, of the memory image, and of the image of imagination, these have treated the subject mostly from the point of view of recognition and recall, and a specific investigation of the attributes of the image has been neglected. Especially meagre in the existing experimental work on imagery is reference to its intensive aspect, a subject that has played an important rôle in the study of sensation. Our aim in the present investigation has, therefore, been to attack the problem of images from the point of view of their intensity. More specifically, we seek to answer two questions: (1) Do images possess the attribute of intensity? and, if so, (2) Is this intensity comparable with that of sensations, and in how far? A brief review of the mention of this subject in psychological literature and in experimental investigation will serve not only to introduce the problem but also to show the urgent need for its careful examination.

¹From the Psychological Laboratory of Cornell University

(a) *Historical*

Though almost all writers agree in accepting intensity as one of the attributes of sensation, there is by no means such agreement with regard to the intensity of images. Indeed, many of the earlier writers, and a few of the more recent as well, seem to find in intensity the main difference between images and sensations. They have either denied this attribute altogether to certain images, or, at best, have granted them but a small measure of it. The clearest and most concise expression of this view, perhaps, is that of Hume. "All the perceptions of the human mind," he tells us, "resolve themselves into two distinct kinds which I shall call Impressions and Ideas. The difference betwixt these consists in the degree of force and liveliness with which they strike upon the mind. . . . Those perceptions which enter with most force and violence we may name *impressions*; . . . By *ideas* I mean the faint images of these in thinking and reasoning."¹ That Hume regards difference in intensity as the distinguishing mark of the image, appears also in such statements as the following: "The first circumstance that strikes my eye, is the great resemblance betwixt our impressions and ideas, in every other particular except their degree of force and vivacity."² "That idea of red, which we form in the dark, and that impression, which strikes our eyes in the sunshine, differ only in degree not in nature."³ While Bain is not so explicit in regarding intensity rather than quality as the essential difference between image and sensation, he, nevertheless, calls it the most obvious point of difference.⁴ Hobbes⁵, John Stuart Mill⁶, and Hamilton⁷, barely touch upon the subject, yet indicate their assent to this view. Spencer undoubtedly confuses intensity with clearness or vividness, but on close examination his doctrine appears to agree with that of Bain. He divides feelings into "those primary and vivid feelings produced by direct excitation, and those secondary or faint feelings produced by indirect excitations," adding that he wishes to emphasize "not difference in kind, but difference in degree."⁸

Of the German writers, Lotze seems to deny absolutely that images possess the attribute of intensity. There has been some discussion with regard to his statement that "the idea of the brightest radiance does not shine, that of the intensest noise does not sound, that of the greatest torture produces no pain."⁹ Such a statement, in the opinion of Titchener, reflects a form of the stimulus error.¹⁰ Another statement of Lotze's which must be regarded as doubtful and which, by the way, is not consistent with the sentence quoted above, is the following: "Only sensations of moderate intensity allow of a reproduction that is in some measure faithful."¹¹ Ebbinghaus, while not actually denying intensity to images, grants them only a scant allowance. In one passage, indeed, he says: "The imaged sun does not shine and its imaged heat with its thousandfold degrees gives no warmth; the last spark of a flickering match is far more effective in both respects."¹²

¹ David Hume: *A Treatise of Human Nature*, ed. by L. A. Selby-Bigge, 1889, 1.

² *Op. cit.*, 2.

³ *Op. cit.*, 3.

⁴ J. Mill: *Analysis of the Phenomena of the Human Mind*, 1878, I., 63 note; A. Bain: *The Senses and the Intellect*, 1888, 338.

⁵ T. Hobbes: *Leviathan*, 1881, 6.

⁶ J. Mill: *Op. cit.*, 68; note by J. S. Mill.

⁷ Sir W. Hamilton: *Lectures on Metaphysics*, 1859, II., Lect. XXIII., 259 ff.

⁸ H. Spencer: *The Principles of Psychology*, 1890, I., 288.

⁹ H. Lotze: *Outlines of Psychology*, tr., 1886, 28; cf. also *Medicinische Psychologie*, 1852 477 ff.; *Microcosmus*, I, 203 f.

¹⁰ E. B. Titchener: *A Text-Book of Psychology*, 1910, II., 398 note.

¹¹ "Nur Eindrucke von mittlerer Groesse scheinen sich einigermassen entsprechend reproduzieren zu lassen." *Medicinische Psychologie*, 1852, 479.

¹² "Die vorgestellte Sonne leuchtet nicht und die vorgestellte Glut ihrer Tausende von Waermegraden waermt nicht; das letzte Fuenkchen eines verglimmerenden Streichhoelzchens leistet in beiden Beziehungen weit mehr." H. Ebbinghaus: *Grundzuege der Psychologie*, I., 1905, 549.

Statements as strong as this inevitably raise doubts; it seems at least possible that there should be many to whom the memory of the burning heat of the summer sun would convey more warmth than the dying glimmer of a match. We must not neglect, however, to notice those passages in which Ebbinghaus admits that, in certain cases, images may have a measure of intensity. He tells us that there are circumstances under which the intensity of an image may be increased so as to be comparable to that of the weakest sensation.¹

Turning from the German psychologists, we find that Paulhan speaks of the image as the feeble reproducton of a perception,² and Rabier states plainly that the difference between sensation and image is one of degree and not of nature.³ Sully tells us that "the most obvious point of difference is the greater intensity of the sensational or presentative element in the percept which gives the whole structure its peculiar vividness (or strength)."⁴ Baldwin summarizes the general positions held on the subject as follows: "On the one hand it is maintained that there is a specific difference between presentations and their revived images; a difference of nature. . . . Others hold that between primary and secondary states, there is only a difference of degree."⁵ He proceeds to take his stand with the latter group, holding that presentations and representations have the same antecedents and effects, and "we are aware in consciousness of no peculiar marks of revived states by which to distinguish them from percepts except that they are prevailingly of less intensity."⁶ So far, then, there is agreement that certain images either lack intensity except in the case of hallucinations (which are regarded as abnormal phenomena), or at least have but a small degree of intensity. The difference between images and sensations is regarded as one of intensity or strength, not of nature or quality.

Opposed to the above position are a number of prominent psychologists who regard nature or texture as the more important *differentia* of images, emphasizing the fact that these are incomplete, indistinct, fleeting. They admit that images are ordinarily less intense than sensations, but maintain that this is not universally the case. Images, for these writers, have a marked degree of intensity; indeed, they may be just as intense as sensations. Of those who uphold this view it is sufficient to mention Wundt, Kuelpe, Hoeffding, Taine, James, and Ladd. Wundt insists that while images and sense perceptions usually differ in intensity, it is the difference in elementary composition that is all-important.⁷ Kuelpe goes into the matter at greater length. While admitting that images are usually of lesser intensity, duration, and extension than perceptions, he nevertheless insists that "centrally excited sensations, like peripherally excited, must be accredited with quality, intensity, and a temporal and spatial character."⁸ In temporal and spatial determination and in their results, centrally and peripherally excited sensations, we are told, differ widely. The position of Hoeffding with regard to the subject is clear. He says: "There is, indeed, as a rule a difference in the degree of strength of a memory-image and a percept; but this difference may be very small, and may even quite disappear."⁹ Taine seems to confuse intensity with clearness, but may nevertheless be classed with this group. "We may confidently assert, then," he says, "that the internal event which we call a sensation . . . is reproduced in us without impression from without—in the majority of cases partially, feebly, and vaguely, but in many cases with greater clearness and force."¹⁰

¹ See *op. cit.*, 552.

² F. Paulhan: *L'activité mentale*, 1889, 106.

³ E. Rabier: *Leçons de philosophie*, I., 1896, 157.

⁴ J. Sully: *The Human Mind*, 1892, I., 283; *Outlines of Psychology*, 1891, 157.

⁵ J. M. Baldwin: *Handbook of Psychology, Senses and Intellect*, 1890, 146-7.

⁶ *Op. cit.*, 147.

⁷ W. Wundt: *Outlines of Psychology*, tr. 1901, 282.

⁸ O. Kuelpe: *Outlines of Psychology*, tr. 1901, 182.

⁹ H. Hoeffding: *Outlines of Psychology*, tr. 1891, 130.

¹⁰ H. Taine: *On Intelligence*, tr. 1899, I., 40.

Ladd and James discuss our subject only briefly. The former tells us that the image often lingers persistently in consciousness at a marked degree of intensity, and that images and perceptions differ mainly in their other characteristics.¹ James asserts that "the difference between the two processes feels like one of kind and not like a mere 'more' or 'less' of the same."² Then he adds: "The subjective difference between imagined and felt objects is less absolute than has been claimed."³

As a third group we must mention three writers, Stout, Jodl, and Ziehen, who hold the general view that images and sensations are *toto genere* different and therefore incomparable things. It follows as a corollary that their intensities also do not admit of comparison. Stout believes that images have intensity, but that this differs radically from the intensity that we attribute to sensations.⁴ Sense experience for Stout has an aggressive character which is essential to its nature and not merely due to concomitant motor or organic sensations. The image, we are told, may be just as bright and loud as the sensations, but it lacks the element of aggressiveness which alone could make its brightness or loudness like that of a sense perception. Jodl insists that images may have all the degrees of intensity that are to be found among sensations, but because they are essentially of different composition, are made of different material, we can never compare the intensity of an image with that of a sensation. "Is a *fortissimo* that we image softer than a *fortissimo* that we hear, is sunlight that we perceive brighter than sunlight that we image, is imaged sugar less sweet than tasted sugar? Every attempt to answer this leads to absurdity."⁵ Ziehen's position is more difficult to define. He regards the nature of images as such that they can have no intensity, properly speaking. "It is not a difference in *intensity* between the idea and the sensation, but above all a qualitative difference. The sensual vivacity, characteristic of every sensation, does not belong *at all* to the idea, not even in a diminished intensity."⁶

It is difficult, of course, in any grouping of writers, to avoid a certain amount of misplacement and complication, since point of view and general treatment differ widely. We find, however, as has just been shown, three types of theories regarding the intensity of images. The first takes as its watch-word the phrase, "Images are faint copies of sensation;" it regards images as identical with sensations in quality but as much less intense. The second holds that intensity may differ, but that it frequently does not; the textural difference is all-important. The third regards difference in nature as an insuperable obstacle to any comparison of the intensities of image and sensation. To abstract consideration it would appear that the first class slurs those characteristic differences which make mind the rich and interesting thing it is, and that the third class so overemphasizes these differences as to do away with that degree of unity which mind seems to possess. Whether or not the view of the second class mentioned above is tenable must be decided, however, not by *a priori* speculation but by experimental investigation alone.

(b) *Experimental*

Thus far there has been no experimental work especially directed to the question of the intensity of images. Mention of the subject has been made incidentally, in connection with the discussion of other problems. It will

¹ G. T. Ladd: *Psychology, Descriptive and Explanatory*, 1894, 239, 241.

² W. James: *The Principles of Psychology*, 1905, 70.

³ *Op. cit.*, 72.

⁴ G. F. Stout: *A Manual of Psychology*, 1899, 399.

⁵ "Ist ein *Fortissimo* welches wir vorstellen leiser als ein *Fortissimo* welches wir hoeren, ist Sonnenlicht das wir sehen heller als Sonnenlicht welches wir vorstellen; vorgestellter Zucker minder süß als geschmeckter? Jeder Versuch darauf eine Antwort zu finden, fuehrt ins Absurde." F. Jodl: *Lehrbuch der Psychologie*, 1903, II., 92 f.

⁶ T. Ziehen: *Introduction to Psychological Psychology*, tr. 1893, 152.

be necessary for us to glance only at the results that refer to the memory image.

Experimental investigations of memory are practically confined to the last fifteen years. Prior to this time we have only the introspective record of Galton, who remarks that a remembered object is to him quite comparable to the real object. "I feel as though I was dazzled when recalling the sun to my mental vision."¹ Other instances also are given in which the memory image appeared to be as bright as the actual scene. The investigation of mental imagery by Lay in 1898² rests on the questionnaire method, and its results ought not to be accepted without careful criticism. We find, however, a record of images of considerable intensity, though this fails to measure up to the intensity of corresponding sensation. Meakin³ and Moore⁴ worked respectively on the mutual inhibition and on the control of memory images. The former makes no mention of intensity, and Moore tells us only that an image may be vivid for a period of five minutes. More to our purpose are the experiments of Kuhlmann. In his first series, he investigated the nature of imagery in the recall of a given material; he directed his subjects to recall, after a long interval, and to draw certain meaningless visual forms.⁵ Two types of images were found, the spontaneous and the slowly developing image, yet these in their completed state differed not so much in intensity as in other attributes. Kuhlmann tells us that "less intensity and vividness is among the least of the characteristics in which the memory differed from its perceptive experience."⁶ In another article this author states that, in the case of the spontaneous images, "the words would ring out clear and intense" and "the imagery then approached the perceptive quality characteristic of all vivid recall."⁷ In a further study, dealing with recognition, Kuhlmann says: "We have ceased to be satisfied with the conception of memory as reproduced past experience, of images as faint copies of original perception. We may regard this condition as a good index of the state of our progress."⁸ Gore, in an article entitled "Image or Sensation," entirely rejects intensive difference between images and sensations. "Could you rule out the ideational or perceptual setting, your image would leave off being an image. It would become sensational in quality and value."⁹ H. B. Alexander has also touched upon our problem, but gives us simply a record of personal observation during several years. He finds that "with reference to vividness, three grades of intensities are to be discriminated."¹⁰ His description of the three classes, however, betrays a confusion between intensity and clearness. The first, fleeting images of common thinking, are described as vague, fragile, and ephemeral; the second as small and watery, but growing clearer and assuming color under attention; the images of the third class are life-sized, clear, and bright, with definite background. Memory images, he says, are often more vivid than after-images or than dim perceptions. Slaughter¹¹ and Murray¹² barely mention the question of intensity, both laying emphasis upon the motor and kinæsthetic elements in reproduction.

¹Sir F. Galton: *Inquiries into Human Faculty*, 1883, 89 f.

²W. Lay: *Mental Imagery*, 1898.

³F. Meakin: *Mutual Inhibition of Memory Images*, Harv. Psych. Studies, I, 1903, 244.

⁴C. S. Moore: *Control of the Memory Image*, Harv. Psych. Studies, I, 1903, 282.

⁵F. Kuhlmann: *Analysis of the Memory Consciousness*, Psych. Rev., XIII, 1906, 316.

⁶*Op. cit.*, 342.

⁷F. Kuhlmann: *On the Analysis of Auditory Memory Consciousness*, Amer. Jour. Psych., XX., 1909, 200.

⁸F. Kuhlmann: *Problems in Analysis of Memory Consciousness*, J. of Phil., Psych., and Sci. Meth., IV, 1907, 1.

⁹W. C. Gore: *Image or Sensation?* J. of Ph., Psy., and Sci. Meth., 1, 1904, 437-8.

¹⁰H. B. Alexander: *Some Observations on Visual Imagery*, Psych. Rev., XI, 1904, 320.

¹¹J. W. Slaughter: *A Preliminary Study of the Behavior of Mental Images*, Amer. Jour. Psych., XIII, 1902.

¹²E. Murray: *Peripheral and Central Factors in Memory Images of Visual Forms and Color*, Amer. Jour. Psych., XVII, 1906.

The studies of the memory image made by Bentley¹ and Whipple,² the one dealing with vision and the other with audition, make no reference to our problem, with the exception of Whipple's statement that the image, when held, decreases rapidly in intensity. Kennedy gives a brief summary of the results thus far obtained with regard to judgments of intensity in paired memory images. "In the case of the intensity of sound we find a decrease in the intensity of the memory image; in the case of light, either a decrease or increase of the intensity of the image according to the intensity of the object itself; and in the case of squares and of pressure, a quantitative increase in the image."³ The general light thrown upon our subject by the above investigations, then, is merely an indication that introspection reveals intensity as an attribute of images and that this intensity has various degrees. The need for further and more definite study is apparent.

DIFFICULTIES AND SOURCES OF ERROR

The study of imagery is subject to certain difficulties to which we ought to pay regard at the outset. One of the greatest of these, and one which occurs in the field of sensation as well, though in a less serious form, is what is technically known as the stimulus-error. It is the tendency to evaluate sensations and images in terms of the stimuli which produce them, instead of in terms of the conscious experience itself; the error of allowing a knowledge of the objective order of things to bias introspection. Unless this error is avoided, results become practically worthless. In our experiments, therefore, we have attempted in various ways to eliminate it. The two sounds whose images were to be compared were produced by the same stimulus; there was no difference in pitch or timbre, but only in intensity. Thus, there is no reason why reference to the stimulus should influence judgment. Moreover, the actual stimulus was not seen by the observers; they saw neither the force of the stroke upon the fork nor the distance of the drop of the sound-pendulum. There were cases in which one of our observers, especially, had visual images of this distance; but this fact could not be regarded as evidence of the stimulus-error, since her visual imagery was avowedly based upon actually experienced (that is, heard) intensities, and not upon a memory or perception of the distance of the swinging pendulum. The danger of the stimulus-error was further reduced by the fact that our observers were merely instructed to compare memory images, and it was this comparison, and never that of the sensations, that received emphasis throughout the entire experiment. The judgments of the observers were thus directed a step farther than sensation from the original stimulus. With these various precau-

¹ I. M. Bentley: *The Memory Imagery and its Qualitative Fidelity*, Amer. Jour. Psych., XI, 1899.

² G. M. Whipple: *An Analytic Study of the Memory Image and the Process of Judgment in the Discrimination of Clangs and Tones*, Amer. Jour. Psych., XII, 1900-01.

³ F. Kennedy: *On the Experimental Investigation of Memory*, Psych. Rev., V, 1898, 493.

tions our results would seem to be practically, if not entirely, free from stimulus-error.

More serious even than the danger just mentioned is that of confusing intensity with clearness. With respect to this Wundt cautions us as follows: "We must be especially careful not to confuse the clearness of an idea with its intensity. That is simply dependent upon the sensations which constitute it. The intensity of perceptual ideas is determined by the strength of the sense stimuli, that of memorial ideas by other conditions which have nothing to do with ideational clearness. At the same time, intensity usually promotes clearness and distinctness."¹ By clearness we mean that sharpness or focal distinctness which depends upon, or is identical with, the degree of attention. "As applied to our ideas, then," says Wundt, "clearness and distinctness denote properties which depend directly upon the activity of ideation."² Intensity, in the psychological sense, is the strength or force of a sensation or image in consciousness.³ It is an attribute of a sensation or image, and, if not absolutely, is at least relatively independent of the attitude of the observer, even if upon further investigation it should develop that attention affects intensity.

Before taking up our experiments, therefore, it was necessary by means of preliminary experiments so to familiarize our observers with the introspective difference between clearness and intensity that their reports should be free from any confusion of the two. In these preliminary experiments, two metronomes of different intensities were allowed to beat, and the observers were instructed to perform aloud some task, such as spelling, reciting, or adding, and to attend now to the loud, now to the weak metronome, or else to their task. After 40 seconds they dictated their introspections. The results of these experiments show that the observers were able to hold the weak metronome at a maximal clearness for most of the time, even in the face of the more intense metronome and of the task that was being performed. When the weak metronome was maximally clear, the task and the louder metronome usually alternated in the background. The experiments were then repeated, with the exception that the task was now performed in terms of mental imagery. The observer was told to attend to one of the metronomes while employing visual imagery in counting, spelling, or reciting, having auditory images of the chimes playing a familiar air, or kinæsthetic

¹W. Wundt: *Lectures on Human and Animal Psychology*, tr. 1901, 247.

²*Op. cit.*, 246.

³See W. H. Sheldon: *Definitions of Intensity*, *Jour. of Ph., Psy., and Sci. Meth.*, I., 1904, 233-237.

images of lifting weights. After a brief practice in this, the attention of the observers was directed to the task instead of to the metronome. With little difficulty they were able to get very clear images, while scarcely hearing at all either the loud or the weak metronome. In the next and final group of preliminary experiments only one metronome, loud or weak, was sounded, the observer performing a task aloud and getting an auditory image of the other metronome. At the beginning of each experiment the metronome to be recalled in image was sounded for an instant, and an interval was allowed to intervene before the observers' recall. That all of our observers found it possible, while performing mechanically some task, to keep clear and focal the image of a weak metronome and yet to be conscious that the real metronome beating in the background was louder than the image, goes to show that they had succeeded in sharply distinguishing intensity from clearness. Entirely of their own accord the observers gave many reports of "weak metronome clear, loud metronome vague and dim." These results, added to the fact that all of our observers were practised in introspection, seemed to warrant us in proceeding to our specific study regarding the intensity of images with the assurance that this would not be confused with clearness.

EXPERIMENTAL PROCEDURE

Our experimental investigation of the problem of the intensity of images was confined to the memory image, with the exception of a few experiments involving the image of imagination.¹ The memory image has been variously defined. For us, however, the term designates that experience which does not come to us through external sense perception, yet reproduces this perception to consciousness with its specific temporal reference in such a way as to be clothed with recognition. This, then, is the image whose intensity we endeavored to investigate. The 'mental image' or general, timeless image was left entirely out of account.²

Four observers took part in these experiments: Dr. Helen M. Clarke (C), fellow in psychology; Dr. L. R. Geissler (G), instructor; Mr. W. S. Foster (F), assistant; and Dr. T. Okabe (O), scholar in psychology. All of these observers had had an exceptional amount of training and practice in

¹For a discussion of the differences between images of memory and of imagination, see C. W. Perky: *Amer. Jour. of Psych.*, XXI., 1910, 422-452.

²See Bentley: *The Memory Image and its Qualitative Fidelity*, *Amer. Jour. of Psych.*, XI., 1899, 27 note; also Slaughter: *A Preliminary Study of the Behavior of Mental Images*, *Amer. Jour. of Psych.*, XIII., 1902, 526.

introspection. Throughout the experiments they were kept in ignorance of the purpose of our investigation. They were asked merely to reproduce an experience in memory, and to write their introspections upon the event. Since these introspections were not guided by suggestions of any kind, there were numerous instances in which no mention whatever was made of intensity. The fact that many of the introspections failed to speak of intensity, therefore, does not militate in any way against our conclusions.

Our experiments fall into eight series as follows:

Series 1. The purpose here was to find out in a preliminary way whether or not the observers spoke of intensity and, if so, in what terms. The observer was seated with his back to a table three meters distant. Upon this table stood a tuning-fork on a resonance box. After giving a 'ready' signal, the experimenter struck the fork with a felt hammer and allowed it to sound for one second before damping. After an interval of half a second the fork was struck again, the stroke being either markedly louder or weaker than, or approximately equal to, the first stroke. Again the fork was allowed to sound for one second. The observer was told to wait until all memory after-images had passed, and then to reproduce the whole experience in memory. After every such experience he carefully recorded his introspections.

Series 2. In order to secure a greater uniformity of conditions, it seemed wise to control the length of the interval between the stimulus and the image, and to secure a check upon the mental operations of the observer during this time. After a number of trials with all of our observers, we decided upon 20 seconds as the shortest interval which might safely be assumed to free the observer from the effects of memory after-images. The above experiments were then repeated with the following modifications: After giving the two sounds, a 20 second interval was allowed to pass. These intervals were filled alternately by allowing the observer's attention to follow its own capricious course and by directing it into certain channels through the following means: noise, either voice or metronome; tone, either tone variator or harmonical; or vision, either colors or pictures. In this manner we attempted to avoid the danger of having the image affected by special or persistent characteristics of the experiences that might fill up the interval before recall.

Series 3. Having tried the shortest possible interval after the cessation of memory after-images, we next undertook a brief series of experiments with a decidedly longer interval, in order to see if the length of the interval had any effect upon intensity. The method of Series 2 was repeated in all details,

except that the 20 second interval was lengthened to one minute.

Series 4. Even at this point it was clear that our observers ascribed intensity to the image, and that this imaginal intensity had many different degrees. The question then arose in how far these various degrees were comparable with those of the sensational scale. In attempting the answer, we produced sensations both noticeably and just noticeably different¹ in intensity, determining in each case, of course, just what this difference should be. Some objective scale of stimulus intensities thus became necessary. Instead of the tuning-fork, therefore, we resorted to the sound-pendulum. Series of four just noticeably different strokes—on the pendulum scale, for instance, 20° , 32° , 40° , 55° —were given both ascending and descending; also a series of three strokes noticeably different— 20° , 45° , 75° —in ascending and descending order; and a series of two just noticeably different strokes—for instance, 32° , 40° —ascending and descending. Each of these six stimulus series was repeated four times in all without definite or consecutive order. At the fourth or final trial of each series the stimulus was given while the observer had his memory image, in order thus to get some control or check upon the absolute intensity of the image. It seemed best in these experiments to lengthen the interval between the stimulus and the getting of memory images from 20 to 30 seconds, because of the slightly longer duration of the memory after-images in the case of the four-stroke series. From the three observers who gave their results on auditory imagery we thus obtained seventy-two introspections. G's auditory imagery having shown itself to be very meagre, the above experiments were carried out in his case with weights as stimuli instead of sounds, the procedure in all other respects being analogous to the above.

Series 5. In this series we again used noticeable and just noticeable differences, but changed the preceding conditions in the two following ways: (a) Only pairs of strokes were used; and (b) the interval preceding recall was now of various lengths—not only 30 seconds as before, but also 60 and 120 seconds. As to the direction of the observer's attention during the interval, the procedure of the former series was

¹The 'just noticeable difference' of this paper is not the ordinary differential limen of the psychophysical methods, but a difference such as would be recognized by the observer in at least 90 of 100 consecutive trials. In other words, it is a Fechnerian just noticeable difference, the least difference that an observer can 'carry in his head,' the just noticeable difference of Ebbinghaus' first form of the method of that name. This difference was carefully determined, for each observer, at the beginning of our experiments, and was tested, less accurately, at intervals during their progress.

not changed. It should be mentioned that at various times throughout the course of this series the stimulus was repeated for the purpose of comparison with the observer's image. For observer G the stimuli were again lifted weights.

Series 6. Observations were now made on images of imagination instead of upon memory images. At the beginning of every sitting the experimenter sounded on the pendulum a stroke of moderate intensity, 40° on the scale. After this sound, five minutes were allowed to pass in general conversation, and then the observer was asked to imagine, according to the direction of the experimenter, certain pairs of strokes. These pairs were both ascending and descending in order, now noticeably different, now just noticeably different in the centre of the scale, now just noticeably at the loud end of the scale, and now just noticeably different at the weak end. The observer signalled the appearance of the image, and the experimenter immediately sounded two strokes such as the observer had been told to imagine. The latter thereupon reported how his images compared in intensity with the strokes just sounded. In the cases of a failure of correspondence, the experimenter continued giving other pairs of strokes until the observer said "My images were like those." This usually took only one or two trials—too few for the observer's image to fade in the meantime. The method, though not free from error, was accurate enough to show us the nature of just noticeable differences and noticeable differences in the image, as compared with those on the scale of sensory intensities.

Series 7. A series of experiments was now performed in order to investigate the minimal and maximal limits of imaginal intensity. Both very loud and very weak sounds were given on the gravity phonometer as well as on the sound-pendulum. After a 30 second interval, the observer was asked to reproduce the sound in image.

Series 8. In order to compare our results in the field of auditory imagery with those which might be obtained in other fields, we instituted a series of tests on brightnesses. The brightness-discrimination box was used, an apparatus which enables the observer to see simultaneously two brightnesses side by side. The brightness of both openings, or of either one singly, could be regulated at will and determined by a scale on the box.¹ The observer sat directly in front of the centre of the box in a dark room and after a period of adaptation two brightnesses, either noticeably or just noticeably different, were shown. He then waited until all after-images had dis-

¹For a fuller description of this apparatus, see G. M. Whipple: *Manual of Mental and Physical Tests*, 1910, 163.

appeared, and recalled the experience in a memory image, dictating his introspections to the experimenter.

The results of the above experiments seemed sufficient to warrant conclusions regarding the intensity of auditory memory images, and to tell us something also regarding visual and kinesthetic memory images.

RESULTS

I. Intensity. (a) Ascription of intensity to images

The first part of our problem was to determine whether or not the image possessed intensity. For the answer to this question we may simply turn to the introspections that were given. As has been mentioned above, our observers were not aware of the object or purpose of the experiments,—they did not know that our concern was with intensity at all. Their very frequent references to intensity, as an attribute of images, are therefore the more significant since there was absolutely no ground for supposing that they were reporting anything other than that which introspection actually revealed to them. Only a very few of the statements regarding this subject may be quoted here. They are selected at random and may be regarded as typical.

Series 1. Observer C. "Very clear auditory images, like the sensations in pitch, intensity, and time-interval." "Purely auditory images like the sensations in intensity." "Good images like the heard tones in pitch and intensity." "Intensity of the images a little weaker than that of the sensations."

Series 3. Observer F. "I think the images are very accurate copies of the sensations both as to quality and intensity." "Good images with intensities about like that of sensations." "The images were not quite so clear as in sensation."

Series 6. Observer O. "Images just exactly like sensations in loudness. The sensations and images differ in quality so that unless there were an element common to both I could not compare them without making an arbitrary standard. I do not do this but compare them by their intensities; therefore I know that intensity is the common element, although they differ in every other way, force, liveliness, purity, etc." "Got good images but the first was a little too strong. The second was just like the sensation in intensity."

Series 4. Observer G (lifted weights). "In the image the second was distinctly more intense." "Images like the sensations in intensity."

Series 2. Observer F. "I think the images are very accurate copies of the sensations as to intensity." "Intensities of images about correct." "Loud image good and like the sensation in intensity, weak image not clear this time and a little too loud."

Thus, our observers spontaneously attributed intensity to the image, and this occurred in by far the greater number of our experiments.

The question naturally arises, in this connection, whether the images induced under experimental conditions are the

same as "the normal waking images of every-day life."¹ This query occurred to Slaughter, and he tells us that it is impossible to answer it. It is, however, merely the old question of the value of experimental introspection, recurring in special form, and as such is possible to answer. All of our observers regarded the images evoked in the laboratory as like their ordinary images, with the possible exception of G. This observer had very poor auditory images, and reported them as being so weak as almost to lack intensity altogether, although, as he said, his ordinary, normal auditory images, while they are usually vague, often have a marked degree of intensity. Such statements did not occur in the case of our other observers, nor with G in the field of kinæsthesia. It may be questioned whether the meagre auditory images which G had in the laboratory were really memory images at all, for he tells us that in numerous cases "the images involve no reference to the previous sensations; they occur as independent conscious events."

(b) *Varying degrees of imaginal intensity*

The following results furnish the answer, in part at least, to the second portion of our problem, the question, Can the intensities of images be compared with sensational intensities, and to what extent? The results of the different series, so far as they bear upon the varying degrees of imaginal intensity, may conveniently be thrown into the form of Tables. Con-

TABLE I

Series 1: Tuning-fork struck at two noticeably different intensities; no definite interval before recall.

Obs.	Exp'ts made	Absolute imag. int. mentioned	Imaginal = sensational intensity	Both images weaker	2nd im. weaker or stronger than first
C	33	11	9	2	
F	29	13	12	1	
O	32	18	6		12

cerning the judgments in this series (column 4) and in all following series under the rubric 'both images weaker,' it is important to mention that our observers also reported that the difference in intensity between the images was the same as the intensive difference between the sensations. Thus, C

¹Cf. Slaughter: *A Preliminary Study of the Behavior of Mental Images*, Amer. Jour. Psych., XVIII., 1902, 548.

reports: "The auditory images were like the sounds in relative intensity, but both images were a little less intense than the corresponding sensations;" "both images weaker, but like the sounds in difference of intensity." The following statement of F is to the same effect: "Both images weaker than corresponding sensations . . . intensive difference between images like that between the sensations." O, indeed, gave two judgments reporting the difference in intensity as greater in image than in sensation. "The difference in loudness between the two images seemed greater than in sensation,—it seemed too great;" "the images were pure and simple and the difference in loudness seemed too great."

The experiments with G are not recorded in the Tables because his auditory imagery was, as a rule, too poor to admit of full introspective accounts. We give three of his most definite introspections: "I get no intensive difference between the images, although it was plain enough in the sensations;" "there seemed to be no difference in intensity between the images;" "the intensive difference between the two sensations was marked, but the only difference between the images is a stronger breathing accompanying the image which corresponds to the louder sensations."

TABLE II

Series 2: Fork struck at two n. d. intensities; before recall a 20 sec. interval either left for O to fill or filled by E with noise, tone, or visual stimuli.

Obs.	Exp'ts made	Absolute imag. int. mentioned	Im. = sens. intensity	One im. correct	Both im. weaker	Both different in int
C	24	12	8		4	
F	24	7	3	2		2
O	24	8	2	3	1	2

The reports of columns 5, like the corresponding reports of *Series 1*, include also the judgments that the difference in imaginal intensity was the same as that between the intensities of the sensations. Besides the above, F and O reported four and ten times respectively that the difference in the intensity of the images was the same as the difference between the sensations, without, however, making mention of absolute intensity.

Nearly all of G's images came in kinæsthetic instead of in auditory terms. As regards the direction of the observer's attention during the interval, we find that, in all but two of the cases in which the intensity of the images was like that of

the sensations, this occurred when *O*'s attention was allowed free play. The two exceptions were reported by *O* and occurred when the interval was filled by *E*'s reading nonsense syllables.

TABLE III

Series 3: Series 2 repeated with one minute interval before recall instead of 20 sec. interval.

Obs.	Exp'ts made	Absolute imag. int. mentioned	Imag. = sens. intensity	One im. correct	Both images weaker
C	10	5	4		1
F	10	5	3	2	
O	10	4	1	3	

C here reported two cases in which the difference between the images was like that between the sensations, but she failed to make any statement regarding their absolute intensity. There is no evidence in the results of this series that the filling of the interval affected the images, except *O*'s statement that his images are easier to get after an 'empty' interval, *i. e.*, an interval in which attention was allowed to follow its own course. In two cases *F* mentions that his image seems to be a 'general' image rather than a memory image, and in his final introspection he states it as his belief that the same thing was true in a number of cases. In this series of experiments, *G* was able to report two cases in which imaginal intensity was like that of sensation. The results of this series, it will be noticed, agree in general with those of the above series.

TABLE IV

Series 4: Sound-pendulum; six series consisting of 2 j. n. d. strokes, 4 j. n. d. strokes, and 3 n. d. strokes, in ascending and descending orders. Interval of 30 sec. (filled or empty) before recall.

Obs.	Exp'ts made	Absolute imag. inten. mentioned	Imaginal = sensational intensity	Both images weaker
C	27	6	6	
F	26	8	4	4
O	25	3		3

Besides the seven cases (in column 4) in which both images were weaker but the intensive difference between them was the same as that between the sensations, *C* reported two

cases of correct difference without mentioning absolute intensity. There seems to be no regularity as to which series is most often reproduced correctly in image.

This set of experiments, however, brought out an interesting fact in connection with images of noticeable and just noticeable differences. F reported four cases in which a just noticeable difference was increased in image, and two in which such a difference seemed to grow even smaller; in the case of noticeable differences, three introspections tell us that the difference is lessened in the image. The following record of F's reports makes this point clear:

Stimulus 2 j. n. d. sounds. "I think that the difference between the two intensities in the image was greater than that between the two sensations;" "the difference in the intensity of the two was greater in image than in sensation;" "good auditory image of first sound with intensity like that of sensation, but the second image had greater intensity than the second sensation had." *Stimulus 4 j. n. d. sounds.* "I think the weakest image was too strong and the strongest too weak." *Stimulus 3 n. d. sounds.* "There was less difference of intensity in the series of images than in sensation;" "loudest image not loud enough and weakest too loud;" "loudest image not loud enough and weakest too strong."

Both F and C mentioned cases in which a j. n. d. was eliminated and the two sounded equal in image:

F. *Stimulus 2 j. n. d. sounds.* "Images of equal intensity;" "sounds alike in intensity in image."

C. *Stimulus 2. j. n. d. sounds.* "Auditory images of the same intensity;" "both images were equally intense."

TABLE V

Series 5. Pairs of j. n. d. and n. d. strokes; interval of 30 sec., 60 sec., or 120 sec. (filled or empty) before recall.

Obs.	Exp'ts made	Absolute imag. inten. mentioned	Imaginal — sensational intensity	Both images weaker
C	18	3	1 (n.d. 60 sec.)	2 (120 sec.) 2 (30 sec.)
F	21	6	(j. n. d. 30) (j. n. d. 30) 4 (n. d. 60 sec.) (n. d. 60 sec.)	2 (60 sec.)
O	20	4	(j. n. d. 30) 3 (j. n. d. 60) (j. n. d. 120)	1 (30 sec.)

For the sake of uniformity in the Tables, we have arranged the results of this series also with reference to absolute intensity. The series was undertaken, however, primarily in order to observe the effect of different time-intervals upon the noticeable and just noticeable differences. Our results with regard to this point follow.

Just noticeable difference too great in image: C, 1 case (120 sec.); F, 2 cases (120 sec.); O, 3 cases (30 sec., 60 sec., 120 sec.). Just noticeable difference noticeable in image (images equal); C, 1 case (60 sec.); O, 2 cases (120 sec.). Noticeable difference too small in image: C, 1 case (120 sec.); F, 2 cases (120 sec.); O, 4 cases (30, 60, 120, 120 sec.). Thirty-one tests with lifted weights were made with G, with the following results: 5 mentions made of absolute intensity; 3 cases in which a just noticeable difference was too great in image (60 sec., 120 sec., 120 sec.); 8 cases in which a noticeable difference was too small in image (either 60 sec. or 120 sec.).

The fact that in many cases the just noticeable differences are greater and the noticeable differences less in the image suggests Leuba's hypothesis with regard to the intensity of the single image. "There seems to be a natural tendency in us to shift the sensations held in memory towards the middle of the scale of intensities. It might be conceived to operate somewhat as follows. The image of a recent sensation tends to recall by association the united residual of all past sensations of the same kind."¹ As far as absolute intensity goes,—and it is to this that the quotation refers,—we have found in our experiments no traces of such a tendency. Our results, however, show that at times the relative intensity or difference in intensity between two images does seem to approach a mean, the just noticeable differences increasing, and the noticeable differences decreasing, in imagery. A closer study of our results shows that this change occurred almost entirely after the long intervals. Thus we can regard it as one of the effects of time upon the two images, rather than as a general characteristic of all pairs of images. F reported that after the longer intervals he was conscious of getting not a real memory image but a sort of 'general' image, referring to no sensations in particular. His reports in the case of the one minute interval of Series 3 are in harmony with his observation in this series. From his introspections we gather that this 'general' image, probably the 'mental image' of psychologists, is usually of moderate intensity. We quote one report: "Relaxation at the tone which was of moderate intensity. I reproduced a 'general' experience and not the particular one this time. I feel that this is what I have been doing usually after the long interval."

Leuba's reference to the "residual of all past sensations" indicates that he refers to this timeless mental image and not to the specific memory image. We must not, however, be understood to mean that in every case in which our observers had an increase in image of what was a small intensive difference for sensation, or a decrease in image of the intensive difference of a markedly large sensory step, they did not get memory images at all. In a number of such cases the introspections

¹J. H. Leuba: *A New Instrument for Weber's Law*, Amer. Jour. Psych., V., 1892-93, 382 f.

tell us that the one of the images was like the sensation in intensity, while the other was either too weak or too strong as the case might be; and this change in the one of the images may be accounted for in numerous ways. We regard as 'general' or 'mental' images only those few in which the difference in intensity was changed by the weakening or strengthening of *both* the images, and the images were thus brought to a moderate or medium degree of intensity. For our observers, such images occurred relatively frequently when there was an interval of one minute or more between the giving of the sensation and its recall in image.

Series 6: Moderate stroke given; pause of five minutes; imaginary images called up by *O* according to *E*'s directions, either j. n. d. or n. d., ascending or descending, on loud, weak, or medium part of scale.

A Table here would only complicate matters, since we are dealing with images of imagination, and also since the matter of absolute intensity is a side-issue, the important thing being the noticeable or just noticeable differences. We found, however, that in numerous cases the observer's pair of images corresponded exactly with the first pair of strokes later given by the experimenter. Of these cases *O* reports four; *F*, six; and *C*, six; with *O* the cases occurred with just noticeable differences on the strong end of the scale; with *F* and *C* they occurred with weak just noticeable differences, and a few with noticeable differences. The introspections of our observers corroborate the results which we tabulated in the course of the series. *O* tells us: "It is much harder to get just noticeable differences when both are weak;" "easiest to imagine strong just noticeable differences." *C*: "Weak images are more likely to be like the sounds than the strong ones are;" "very hard and unpleasant to try to get strong images." *F*: "Weak images are easier to get."

Further results regarding noticeable and just noticeable differences are as follows: Out of sixteen tests, *O* reported six cases in which that which he supposed to be a just noticeable difference in his imaginative image proved, on comparison with a just noticeable difference between sensations, to be greater than this. Out of twelve tests, observer *F* reported 1 case in which an imagined just noticeable difference proved to be too great, and three cases where an imagined noticeable difference proved to be too small, when compared with the corresponding differences in sensation. Out of twenty tests, *C* reported four cases of just noticeable difference too great, and two of noticeable difference too small, in the image of imagination. This seems to indicate that, while we can image in imagination both large and small intensive differences, there is a slight tendency for these differences to approach a type or

mean. In so far, then, their behavior resembles that of the 'mental image' mentioned above.

This result leads us to a series of experiments concerned with the limits of the intensive scale in imagery.

Series 7: Loud or weak sounds given on sound-pendulum or gravity phonometer; O told to reproduce in imagery.

The results here merely show that for observers C and O there were apparently no limits to the intensive scale of images. There was no sensation, however weak or strong, of which they were not able to get an adequately intensive memory image. A few of the introspections follow.

O. *Fall of 1 meter on phonometer.* "Intensity in the image, taken by itself, could not be distinguished from that of the stimulus,—the only difference was in thinness."

4° *on sound-pendulum.* "Good image just like sensation in loudness."

C. *One meter on phonometer.* "Image thin with less body than sensation, but intensity just the same."

80° *on sound-pendulum.* "Got good image just like it." Stimulus was repeated and C said, "yes, my image was just as intense as that sound."

4° *on sound-pendulum.* "Image just like that in intensity."

F could get very weak images, but he was unable to call to mind any that were louder than a fall of 75° on the pendulum or 85 cm. on the phonometer.

TABLE VI
Series 8: Pairs of brightnesses; recall in imagery.

Obs.	Exp'ts made	Absolute intensity mentioned	Im. = sens. intensity	Im. of dark stimulus too light	Im. of light stimulus too dark	Both im. too dark
C	23	19	18	1		
F	22	13	6		3	4
O	11	3	2	1		

There were two cases for each observer in which absolute intensity was not mentioned, but the difference in intensity between the images was reported as being like that between the sensations. There were also five cases for O and one for F where a small difference was imaged as greater, and one for O and four for F where a large difference was imaged as smaller than in sensation. C is of a markedly visual type, which fact probably accounts for the large proportion of her accurate images. In the field of vision, then, our results parallel those obtained in audition and kinæsthesia.

2. *Non-intensive Differences between Sensation and Image*

In the comparison of images with sensations, several facts of interest were brought out besides those immediately con-

nected with our main problem. The results above described with reference to intensity show, as we have seen, that it is not here that the distinguishing mark between images and sensations is to be found. For our observers, as we shall see, this difference lay in other characters of the two experiences. They all repeatedly emphasized the incompleteness¹ of the image as compared with sensation and found here the main point of differentiation. Indirectly, then, we find in these statements a verification of our conclusions.

The introspections regarding this point are so numerous that only a few can here be given. They are chosen at random from all the series.

F. "The images seem finer, less bulky and thick than the actual sounds;" "the tones are as intense in image as in sensation but they lack 'volume,' that is, concomitant muscular and organic sensations;" "the images are abbreviated in some way,—they lack a fullness, vividness, aliveness, sharpness, which the sensations have;" "usually concomitants such as sharp clang or aftertone and organic sensations are not reproduced in imagery;" "hard to say how the images differed from the sensations, but I know they lack certain qualities partly of sound and also perhaps of organic and muscular strains and attitudinal setting. 'Deadness' of sound is partly lack of certain qualities, especially certain higher pitches;" "the visual image is thin and threadlike as compared with the sensations. Images are very instable and the main difference between them and sensation is the fact that they are so abbreviated."

O. "Good images of correct intensity. With the sensations there are kinæsthetic accompaniments which are not present in the image;" "the sensation is fuller and has an element of impressiveness that the image lacks"; "the sensations are accompanied by kinæsthetic sensations and also by noises and overtones, but none of these occur in the image, which was purer and more simple than the sensation;" "images differ from sensations not in intensity but in fullness and kinaesthetic sensations accompanying them;" "the only element common to image and sensation is intensity, they differ in every other way."

C. "Image less steady and impressive than sensation;" "got two images exactly like the sensations in intensity, but they were more subjective and did not give the kinæsthetic shock that accompanied the stimuli;" "images thin and abbreviated but like the stimuli in intensity."

G. "The images differed from the sensations mostly in their accompanying kinæsthetic sensations;" "I do not know what the difference between image and sensation is, or how I can tell which is which. The image has a sort of 'Verschwommenheit,' is thin, diffuse, vaporous, and not as clear-cut as the sensation. The difference is less a matter of degree than of quality;" "images are pure and there are no accompanying strains. The concomitants such as pressure and strains are not reproduced in image,—the images are isolated."

These and many other similar introspections made by four observers, differing widely in imaginal type, indicate that the difference between sensation and image is not one of intensity.

¹Cf. Kuhlmann: *On the Analysis of Auditory Memory Consciousness*, Amer. Jour. Psych., XX., 1909, 214; E. Murray: *Peripheral and Central Factors in Memory Images of Visual Form and Color*, Amer. Jour. Psych., XVII., 1906, 231.

Another point upon which our introspections throw some light is the time-relation in imagery. In by far the greater number of introspections we find mention of the fact that the time-relations of images exactly correspond to those of sensations. There were, indeed, a few cases in which each individual image lasted too long and the interval between them was too short, and, conversely, a few in which the time interval was appreciably lengthened. The latter, however, occurred in the cases in which there was a long interval before recall and in which, as is stated above, the images partook of the nature of 'mental' rather than of memory images. On the basis of our results, therefore, we are able to say that memory images tend, in introspection, to reproduce exactly the time-relations of the original sensational experience. The images, moreover, always tended to become less accurate, both as to intensity and as to temporal relation, when there were a number of repetitions of the recall of the original sensation.

Of the many minor facts brought out by our experiments we should, perhaps, mention also the important rôle that kinæsthesi plays in imagery. Others who have investigated auditory imagery have noticed this same fact. Kuhlmann, for example, tells us that in many cases "only one-fourth or one-half of the sound was imaged in auditory terms."¹ While our observers, as already stated, differentiated images from sensations by the lack of certain kinæsthetic elements, nevertheless, in their descriptions of imagery, they reported the presence of other kinæsthetic elements which were lacking in sensation. They found all manner of throat strains and organic attitudes that aided correct reproduction and carried much of the 'meaning' of the image. The shock produced by a stroke was not accompanied in the imagery by those starts and strains characteristic of the sensation, but other bodily tensions and kinæsthetic elements were substituted for them, and gave meaning to the memory image. While pitch was usually recalled in auditory terms, it was sometimes carried in the memory images by throat settings of which there was no trace in the sensation. Thus, corroborating the results of Kuhlmann, our observers also reproduced the sound only partially in auditory terms. In this fact there is a further distinguishing mark of images, but a mark which, again, is not intensive.

CONCLUSION

The results of our experiments, then, as above described, warrant the drawing of certain conclusions concerning the

¹Kuhlmann: *On the Analysis of Auditory Memory Consciousness*, Amer. Jour. Psych., XX., 1909, 214.

intensity of images. Our problem included the questions whether intensity is an attribute of images and, if so, whether there is a scale of imaginal intensity, and what the nature of such a possible scale may be. The answer to the first of these questions we have found in the introspections of our observers. It appears beyond doubt that, at least under ordinary laboratory conditions, images possess the attribute of intensity. It is undoubtedly true that, oftentimes, the image is to some extent weaker than the original sensation, but it is far from true that this is always or necessarily the case. The true memory image frequently reproduces very exactly the sensational intensity, be it weak or strong; the intensity of the image of imagination, though likely to be of a moderate degree, may at times be very strong as well as very weak. The 'general' or 'mental' image, being a type-image referring not to any particular sensation but to a number of past sensations of varying degrees, is almost always of medium intensity.

The question regarding the nature of imaginal intensity cannot be so briefly or so definitely answered. We have every reason to believe that there are not two different kinds of intensities, but that the intensive attribute is one and the same whether it be that of sensation or of the image. In no case did any of our observers note any difference in the nature of imaginal and sensational intensities. But we need not rely on negative evidence alone, for, as above quoted, there were introspections which stated that intensity is the one element common to image and sensation. The question that has at times been raised, "Could imaginal intensity, as such, replace sensational intensity?" we are, therefore, inclined to answer in the affirmative. One observer explicitly stated that imaginal intensity, taken by itself, is the same thing as sensational intensity. Certain it is that none of our observers found any difficulty in comparing the intensities of images and sensations.

As regards, more specifically, the scale of intensities, we find that with the possible exception of the very loud end of the scale the degrees of imaginal intensity correspond with those of sensational intensity. About very weak sounds there is no doubt, and it is certain, too, that some intense sounds can be exactly reproduced in memory imagery. Whether, however, we can thus image the loudest possible sound, or the brightest possible light, or the heaviest possible weight, we have not ascertained. With this exception all manner of intensive differences, even those just noticeable in sensation, we have found to be accurately reproduced in memory; indeed, with the exception of very loud sounds in the case of observer F, no degree of intensity was given to our observers that was not correctly reproduced by them. From this it would

appear that the intensive scales of sensations and images are identical.

Incidental to the negative conclusion that the difference between images and sensations is not one of intensity, we have gained positive introspections as to the nature of this difference. Our observers regarded the incompleteness, thinness, abbreviatedness of the image as its main point of differentiation from sensation. Compared with the latter, the image lacks a certain 'aliveness' or kinæsthetic complex, and it is this which makes of it a very different thing. In so far, our results confirm the views entertained by the second group of writers to whom we referred in our historical note,—those, namely, who regard imaginal as quite comparable with sensational intensity, but maintain that the image differs from sensation in texture or nature.

We ought perhaps also to refer briefly to two other points: the influence of individual differences in imaginal type, and the question of physiological substrate. As regards the former we may remark that observer C, markedly visual in type, had more good and accurately intensive visual images than any of the other observers; G had almost no images except those of kinæsthesia, and we find him hesitant about intensity until he is tested with kinæsthetic images; the other two observers, individuals of mixed type, seemed to get all images with equal ease. As regards physiological substrate, the question arises what relation we must assume to exist between the cortical centres of sensation and memory, such that a correspondence in intensity may be rendered intelligible.

While our experiments point to certain positive conclusions regarding imaginal intensity, we are well aware that, owing to their limited scope and to the small number of our observers, we are not justified in assuming a dogmatic attitude. The intensity of images still remains a promising field for experimental investigation, an especially interesting problem being that of the upper limit of the intensive scale. In such an investigation the stimulus-error would assume large proportions; yet it might be overcome, we believe, by suitable apparatus and by a method similar to that which we have employed.

THE COLOR SENSATIONS OF THE PARTIALLY COLOR-BLIND, A CRITICISM OF CURRENT TEACHING

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A. INTRODUCTION

Although the existence of color-blindness has been known since 1777 (32, *cf.* 76), and although large numbers of cases have been studied and described (19) during the century and a quarter which has elapsed since that date, the general topic of color-blindness is still in a state which many psychologists consider to be most disgraceful to their science. One reason for this backward condition is undoubtedly to be found in the extreme complexity of the subject, and the enormous variation from case to case; but an even greater obstacle to the progress of knowledge has been the almost universal practice of studying and classifying cases under the domination of some pre-conceived color-theory (36). Perhaps the most notable example of this practice is to be found in the description of the sensations of the red-blind, which Helmholtz gives in the first edition of his *Optik*.² But even to-day, after fifty additional years of extended observation and experimentation upon color-defectives, many psychologists seem disposed to discuss the topic in such a loose and superficial fashion as will make it accord with the color theory which they have espoused, rather than to work out a full and clear

¹The writer wishes to express his gratitude to Professors E. B. Titchener and J. W. Baird for much helpful criticism and suggestion.

²Helmholtz (18, p. 298) states that red, if seen at all, is seen as a weak green; yellow, as a stronger, saturated green; green, as a whitish green; blue and violet, as blue; and white as greenish blue. Holmgren (quoted 33, p. 29) describes the sensations of the green-blind and of the violet blind, in the same fashion; his procedure is essentially a logical process, and his description is an *inference* as to how the defective retina *must* see colors when its green-sensing or its violet-sensing fibres are lacking.

statement of the facts thus far known, regardless of their theoretical implications.

Ever since the work of Seebeck (69), in the early part of the nineteenth century, it has been customary to divide the partially color-blinds into two or more classes,—the writers of the Helmholtzian school tending to distinguish *three* groups, while those who advocate a four color-element theory tend to distinguish *two*. Seebeck regarded the shortening of the spectrum, which he found in certain cases, as a fundamental basis of differentiation; and, in present-day usage, the two best established groups of partially color-blinds are those which are distinguished by the different lengths of their spectrums: *Deuteranopes*, whose color-system is reduced to blue, yellow and grey, but who see color throughout the whole length of the spectrum; and *Protanopes*, who likewise see only blue, yellow and grey, but whose spectrum is shortened at the red-end, and who show the Purkinje phenomenon in ordinary light, *i e.*, whose region of maximum brightness is displaced from yellow toward blue (40). Authorities disagree upon the question of the existence of a third group of partially color-blinds—*Tritanopes*, whose color system is reduced to red and green (40, 71, 74). All three groups are commonly referred to as *dichromates*, because their color system is assumed to be reduced to two colors.

Unfortunately this simple classification does not provide for all the cases of color deficiency which have been discovered. Seebeck (69, pp. 216 ff.) found mild cases of color deficiency which he was loath to include in either class; and ever since his time, no wide survey of cases has failed to reveal a considerable number of marginal forms which were neither normal nor limited to a two-color spectrum. These cases were long spoken of as "color-weak" or "incompletely color-blind," until Rayleigh's work (62) led to the conclusion that many of them were unequally sensitive to red and to green. The equation by which Rayleigh made this discovery,—the mixture of red and green to match yellow,—is commonly spoken of as the "Rayleigh equation." Ever since König's work (39) upon defectives of this type, they have been known by the name which he applied to them,—"*anomalous trichromates*," *i. e.*, persons whose color-system includes all three of the fundamental colors of Helmholtz (red, green and violet), but whose sensitiveness to red or green is abnormal. It is now customary (15, 37, 40, 42, 43, 52) to distinguish two groups of anomalous trichromates, upon the analogy of the two groups of dichromates,—the red-anomalous or protanomalous trichromates, whose sensitiveness to red is below normal, and the green-anomalous or deuteranomalous trichromates, whose

sensitiveness to green is below normal. Recent writers report other peculiarities of this group, the most marked of which is a heightened sensitivity to contrast. Guttman¹ identifies color-weakness with anomalous trichromacy, claiming that the defect is a complex state involving seven inter-related symptoms. Nagel insists that color-weakness is the wider term,—that color-weakness may occur without the other symptoms of anomalous trichromacy being present.

It has been the purpose of the writer to examine the evidence upon which is based the assumption that the partially color-blinds are dichromates,—see only blue and yellow,—and to present a body of new experimental evidence upon the question of the color sensations of color-defectives.

A survey of the literature of color-blindness indicates that we are indebted to Herschel (21) for the first suggestion of the idea that the color-system of the color-blind is reduced to blue and yellow; and that the general acceptance of this idea is based upon the following lines of evidence (61):—

1. Testimony of the color-blinds themselves, and inference from color-confusions, from the naming of spectral colors and the colors of objects.
2. Color equations in which the various colors have been matched by mixtures of blue, yellow, black and white.
3. The study of acquired and temporary color defects.
4. The analogy of peripheral color-blindness.
5. The study of monocular cases of color-blindness. Of these five sorts of evidence, the last is by far the most important, since its evidence is direct.

But it seems very clear to the writer that *theory* has, in many cases, prejudiced the interpretation of the facts obtained from all five sources, and that we have no right to conclude from the evidence at hand that all typical cases of partial color-blindness are dichromates. Almost every writer who has had any wide experience with color defectives has seen mild cases of color-blindness, "incomplete" color-blindness, etc., in which the subjects give evidence of seeing some kinds of red or green; and the conviction seems to be growing common that dichromates are the extreme and not the typical forms of partial color-blindness,—that there are protanopes who see some greens, deuteranopes who see some reds, etc.

In view of the great importance attaching to monocular cases, it has seemed best to review all such cases available. After this the experiments performed by the writer upon a monocular protanope will be described; and the evidence derived from this study will be compared with the results of

¹For the discussion of this question by Guttman and Nagel, see the series of articles in the *Zsch. f. Sinnesphysiol.* 41-43.

similar experiments upon a number of color-blind subjects whose defect extends to both eyes.

B. HISTORICAL CASES OF MONOCULAR (RED-GREEN) COLOR-BLINDNESS¹

1. *Woinow's case of green-blindness in one eye (1871)*

The earliest case of monocular partial color-blindness known to the writer is that reported by Dr. N. Woinow (78) of Moscow in 1871. The patient, a woman 34 years old, was tested with rotating discs and her case was diagnosed as green-blindness. The following equations are reported:—

Left (normal) eye:

$$225 \text{ black} + 135 \text{ white} = 135 \text{ red} + 125 \text{ green} + 100 \text{ violet}$$

Right (color-blind) eye:

$$220 \text{ black} + 140 \text{ white} = 105 \text{ violet} + 255 \text{ red}$$

$$310 \text{ black} + 50 \text{ white} = 30 \text{ violet} + 330 \text{ green}$$

Dr. Woinow evidently studied this case with the Helmholtz three-color theory in mind; but his equations seem to indicate that his patient was very deficient both in red and in green vision. The case was not so simple as this, however. The patient seems to have had an hysterical fear of reds, and both eyes appeared to be somehow sensitive to this color. She said she could not bear to look at red or orange, but that, if she had to do so, "she felt better" with her right (color-blind) eye closed. Moreover, she reported that when she looked with her right eye alone, "everything was tinged with red." Woinow concluded that she was color-blind to green only. But in view of the complications involved, and the small number of tests made, this case seems to have practically no value as evidence of the actual color sensations of the color-blind.

2. *von Hippel's monocular partially color-blind subject (1880)*

In the literature of the last thirty years, constant reference is made to the case of monocular color-blindness which was studied and described by von Hippel (28) in 1880. The subject, a young man, came to von Hippel for spectacles to correct double vision, and in von Hippel's exploration of the subject's right visual field with a Förster perimeter, constant confusions of red and green with yellow were noticed. Before this the subject had known nothing of his color defect. Von Hippel then made a long and careful series of experiments, using a Hoffmann spectroscope, Radde's international color

¹ The monocular cases described by Niemetscheck (59), Holmgren (31), Kirschmann (35) and Piper (60) are intentionally omitted because this paper deals only with protanopia and deuteranopia.

charts, Holmgren's worsteds, Stilling's *pseudo-isochromatische Tafeln*, contrast shadows and tissue contrast, color equations with rotating discs, Dor's charts for the recognition of colors at a distance of five meters, and von Hippel's photometer with colored glasses. In all these experiments, the subject's left eye seemed perfectly normal, while with his right eye he made constant confusions of red and green with yellow, although occasionally using the words "red" and "green" correctly. In the experiments with the spectroscope, von Hippel reported that when the whole spectrum was shown at once the subject claimed to see red, yellow or greenish, and blue; but, when only a narrow band was shown, the whole warm-end of the spectrum was called yellow.

Von Hippel diagnosed his case as one of red-green blindness, and there seems little ground for questioning his decision. As he found no shortening of the spectrum, the subject was probably a deuteranope.

Holmgren (30) studied the same case, and diagnosed it as "a typical case of red-blindness," with shortened spectrum, and the two fundamental colors which such a case *should* have, according to the form of the Helmholtz theory presented in the first edition of the *Optik* (18),—a greenish yellow, and a blue tinged with violet.

Von Hippel (29) then made further experiments with the spectroscope, and substantiated his claim that the subject's spectrum was not shortened. He also carefully compared the subject's judgments of color made with his normal and his color-blind eye, and showed that he used "blue" and "yellow" for the same kinds of sensations in the two eyes. He added a series of experiments with negative after-images, in which the subject reported normal after-images for the left eye, but for the right gave blue as the color of all after-images from red, orange, yellow and green; yellow as the color of after-images from blue and violet.

On the whole, there seems good evidence that this subject saw only two colors (probably yellow and blue); and, as this was the first monocular case pointing clearly towards dichromacy, one can easily understand its importance.

Holmgren claims to have seen another case of monocular color-blindness in 1879, which, however, "unhappily became useless through an accident" (31).

3. *Steffan's case of monocular color-blindness* (15, 70, 73) (1881)

We get no clear light on our problem from this case. The patient was a man sixty-two years old, who showed defective color-vision in one eye after an attack of apoplexy. It is interesting to note that although, the patient showed lowered

sensitivity for all colors, the only color he completely lost was green; but considering our present uncertainty as to the relation of atypical acquired color-blindness to typical congenital color-blindness, we have no right to reason from the one kind to the other (38, 56, 71).

4. *Kolbe's case of "monocular red-green weakness" (1882)*

Kolbe (37b) used many of the same tests as von Hippel; and while this case does not show the grave color deficiency of von Hippel's case, repeated evidences of sub-normal color vision were found. No neutral band in the spectrum was established. But at 518 $\mu\mu$ the subject said at one time that the color was weaker than in the neighboring region; and at another time he reported that from 508 to 520 was an uncertain color. In the use of the Holmgren wools, the Stilling cards of 1879 and Dor's charts, the subject showed himself below normal, but considerably more color-capable than von Hippel's subject. In a series of tests of color sensitivity, Kolbe's subject showed decidedly high thresholds for both red and green. In the experiments with contrast shadows and negative after-images, this subject gave normal results for blue and yellow; but his reactions with red and green were practically those of a color-blind person.

From Kolbe's report, it would seem that this case might well be diagnosed as a mild case of color-blindness, although one hesitates to form such a conclusion without the use of color-equations, and a repetition of the spectral experiments.

Kolbe refers to a monocular case of "red-green" blindness described by Hermann (26a) in a pamphlet not accessible to the present writer. This subject's spectrum was shortened at the violet end,—the brightest region from 588 $\mu\mu$ to 583 $\mu\mu$, which appeared as a dull band, separating red, on the left, from green, on the right. These details seem to point rather to *violet-blindness*.

5. *Schufelt's case of monocular color-blindness (67) (1883)*

Following are the observations made upon "a healthy young man twenty-one years old" when tested with the Holmgren wools:—

"With both eyes open, he succeeded, without trouble or hesitation, in picking out a series of purples and greens to match the test shade; but he exhibited a good deal of uncertainty when called upon to do the same for the reds, the test color being a bright red-lead shade. The worsteds being again mixed up, he successfully chose the purple and green shades with either eye, one or the other being closed, and the

redswith the right eye, the left one being closed. The worsteds were mixed once more, and he was asked to close his right eye, and to pick out the red shades. This he essayed to do by first selecting a pale shade of brown, placing it on one side, and with considerable hesitation of manner, he proceeded in the same way until he had laid aside a full series of brown shades from dark to light ochre. It was amusing to see his confusion when I suddenly released his right eye, as the lids were kept together with my finger, and quickly closing his left, allowed him to see what he had done."

This case adds very little to our knowledge. One would hesitate to base any conclusions upon a preliminary test with the Holmgren worsteds. The case is included in this paper simply for completeness in reviewing the evidence.

6. *A case of color-blindness limited to the nasal half of the left retina, described by Hess in 1890 (27)*

A young man, thirty-one years old, found he had difficulty in distinguishing colors and thought his difficulty a matter of recent origin. Upon examination, it was found that on the nasal half of his left retina he was quite insensitive to red, and had a decidedly lowered sensitivity for the other colors, while on the other half of this retina, and on the whole of the other retina his color-vision was normal. Colors were presented simultaneously to the right and to the left of a given fixation-point; and he was asked to tell what colors he saw. His replies were as follows:

Normal (temporal) half of left eye	Color blind (nasal) half of left eye
Red appeared red	"dirty dark yellow"
Orange " orange	"dirty sulphur color"
Yellow " yellow	"yellow"
Yellow-green, normal	"weak yellowish grey"
Urgrün appeared "	"greenish grey"
Blue " "	"blue with violet tone"
Violet " "	"less saturated violet than that on temporal side"
Purple " "	"greyish violet"

Experiments with spectral lights gave similar results. It seems plain that this subject thought he saw *green* with his affected tract. In one place he especially said that "green looked neither yellow nor blue;" and since his other eye seems to have been completely normal, there is no apparent reason why he should use the wrong name for what he called "green."

In spite of the fact that the patient thought he saw green, Hess diagnosed the case as red-green blindness, explaining its

presence by the assumption that the red-green substance, posited by the Hering theory, was quite out of function. But one feels loath to accept this conclusion. The fact that the defect was limited to one half of one eye, and that the patient thought his difficulty a recent thing, would suggest that the case was possibly of central origin and acquired. Hence, whatever the results obtained by Hess, we ought not too readily to accept them as representative of typical partial color-blindness. And Guttman (15, p. 280) has recently suggested that, under similar tests, a red-anomalous trichromate would have responded in much the same way, for, when small areas outside the fovea are simultaneously stimulated, the anomalous trichromate responds in much the same way as a patient who is typically color-blind.

7. *Hering's case of monocular partial color-blindness* (25)
(1890)

In the same volume with the case just described, we find an account of a series of experiments by Hering upon a patient with partial color-blindness in one eye. The method of experimenting was practically the same: with a simple stereoscopic device he presented patches of color to the two eyes simultaneously, and asked the subject to compare them, and report what colors he saw with each eye. By means of mirrors, Hering was able to change the brightness and saturation of either color presented. Occasionally he tried, by increasing or decreasing the illumination, to present to the normal eye a color or a grey that should match the sensation experienced by the affected eye; but he gives no numerical values, and hence it is impossible to know exactly what his results mean, or to compare them with the results obtained from other subjects. To the affected eye, bluish red was reported to look "grey with a reddish shimmer;" spectral red, "dark yellowish grey;" orange, yellow and yellowish green, "whitish yellow;" *Urgrün*, "light grey;" ultramarine blue, "whitish blue;" and violet, "dark blue."

Upon the basis of these results, Hering diagnosed his case as one of red-green blindness, with weakened sensitivity for blue and yellow.

In a spectrum of moderate brightness, this patient reported three colors; yellow, green and blue. When the brightness was increased, only a "greenish shimmer" was mentioned, though the normal eye saw a beautiful saturated green. The spectrum was shortened at the red end,—the spectrum beginning at wave-length $630 \mu\mu$ for the affected eye, while the normal eye saw color at $670 \mu\mu$. From $630 \mu\mu$ inward, the

patient saw only yellow with the affected eye, where red-orange and orange were visible to the normal eye.

Looking through a telescope at the spectrum, the patient described light of $630 \mu\mu$ as yellow-red, but still more yellowish than it appeared to the normal eye; light of $600 \mu\mu$ she described as orange; light of $570 \mu\mu$ as cream colored. The lights from $500-420 \mu\mu$ she described, sometimes as grey, at other times as greenish grey.

On the whole, Hering seems scarcely justified in calling this a case of red-green blindness, for in one test or another the subject correctly named both red and green. Guttman (15, p. 279) suggests that this case also closely resembles red-anomalous trichromacy, and there is enough similarity to prevent our complete acceptance of the case as evidence for the claim that dichromates see only blue and yellow. Wundt (79, p. 229, note) says of these cases reported by Hess and Hering, "In these two cases we find complete red-blindness, while the sensitivity for green as well as for the other colors is merely lowered."

Of all the above cases, that of von Hippel alone furnishes evidence for the claim that partially color-blinds are dichromates. In Woinow's case, the results are complicated by the patient's emotional reaction to red, which she was supposed not to see; in Steffan's case there was probably some disturbance in the cortex, and in Hess's case there would seem to have been an acquired disorder of some kind; Hering's subject seemed to recognize all the colors when they were intense enough, and should perhaps be classed as a red-anomalous trichromate; the preliminary test performed upon Schufelt's case is suggestive but not conclusive. Von Hippel's subject was probably a dichromate, and von Hippel's work furnishes sufficient evidence that there can be clear-cut blue-yellow vision. But when one considers the great variation among color blind subjects which has constantly been noted by experimenters, one scarcely feels justified in jumping to the conclusion that *all* partially color-blind subjects see only blue and yellow. On the whole there seems good ground for the following confession of von Kries (40, p. 166):—"In general, one may well admit that the factual basis for the oft-made assertion that dichromates are blind to red and green but see yellow and blue, is very insufficient. In reality this claim is the result of theorizing, and its value is to be estimated according to its harmony with theory."

C. A NEW CASE OF MONOCULAR PROTANOPIA

During some work with colors in the year 1907-08, Miss G. S., a Senior in Mt. Holyoke College, made some remarks which

indicated that her color vision was not normal. Preliminary tests showed that she was quite unable to recognize reds with her right eye, while no lack of ability with this or any other color was shown when the left eye was tested.

She had studied psychology for two semesters before the following experiments were begun,—one semester of introductory text-book work and one semester of elementary laboratory work. She seemed to be an intelligent, careful observer of the "objective type." The experiments described below were performed in June, 1908, October, 1908, and November, 1909. The subject was in good health at each of these periods. In November, 1909, she was examined by a professional oculist, who reported that the ophthalmoscope showed nothing abnormal in either eye, but that she was slightly myopic in her left eye, and had some weakness of vision in the right eye which no lens seemed to correct. The subject reports that her maternal grandfather was color-blind, but she knows nothing about the details of his defect, and has never heard of any other case of defective color vision in her family.

1. Color confusions

a. Test with the Nagel cards (fifth edition)

With the left eye, the subject made no mistakes. With the right eye, she could see no red on any cards, and selected as grey A 9 (correct), and numbers A 3, 7 and 15 which have upon them red or red and grey dots. As green she selected the one green card A 5. In series B she thought the reds and browns were black and grey, but correctly named the green in B 3 and the yellow-green in B 1.

In the Nagel test, then, the subject showed herself blind to red, but made no mistakes in green. This is the more remarkable, because many persons who, upon further examination, show only slight defects in color discrimination make numerous confusions between green and grey.

b. Test with Bradley papers

Fifty pieces of Bradley paper, 3 cm. square, including all the standard colors and many tints and shades, with similar squares of the ten Bradley blacks, whites and greys, were spread upon a table in a good light. The subject stood before the table with her left eye covered. The test was conducted in the same manner as the Holmgren worsted test, the subject being given a sample and requested to select ten or a dozen pieces of paper of the same color. She selected green and yellow pieces to match the green sample (green yellow shade 2); four reddish pieces, one light orange, and six

greys to match the rose sample (red tint 2); and nine light and dark reds, four orange pieces and seven greys to match the red sample (red tint 1).

c. Tests with Holmgren worsteds¹

Green A, presented to the right eye, was matched by worsteds 2, 4, 6, 8 and 12, all of which are green or yellowish green. 14, 16 and 18 looked like the sample to her, but darker. 10 and 20 seemed about the same in brightness as the sample, but bluer.

Rose B, presented to the right eye, was matched by 28, 32, 34, 36, 38 and 40, all of which have red in them, by the confusion colors 13, 15, 19, 33, 35, 37 and 39, all of which are browns and far removed from the rose sample, and by the confusion colors 1 and 3 which are faint greys with very little color of any kind in them. The grey 5, the light blue 21, and the bluish reds 22, 24, 26 and 30 were selected as like one sample, but "tending more or less toward blue." In these tests we have a strong indication of red blindness.

Red C, presented to the right eye, was matched by 32, 34, 36 and 38, all of which are reds. 40 looked like the sample but darker. In this test she made no color confusions.

In all of these tests, the subject refrained from holding the sample close to each bunch as she examined it, but, glancing at the sample and then at the other, decided by memory. In most cases she decided quickly and easily.

d. Tests with dots of Hering papers on grey cards²

In view of the fact that color-blindness has been reported (51, 52, 57, 68) to be sometimes more extreme at the fovea than elsewhere on the retina, a special series of experiments was performed to decide the point in this case. Thirty-two grey cards 8 cm. square were secured, and at the centre of each was pasted a round dot of Hering paper 4 mm. in diameter,—small enough so that when viewed directly at a distance of half a meter, only the fovea would be stimulated. The cards were spread out upon a table in a good light, and the subject, with one eye closed, was asked to pick out all the cards having a dot of the same color as one given as a sample. Hering papers 1-12 were each represented by two cards (except red no. 2 with which four cards were used), and upon 4 other cards dots of Hering grey no. 8 were pasted.

¹Forty skeins with metal tag attached. Supplied by Chicago Laboratory Supply & Scales Co.

²Rothe papers made under Hering's direction.

With the right (color-blind) eye, Miss G. S. selected red and grey to match the red sample; purple, violet and blue to match the blue sample; green and yellow-green to match the green sample; and orange and yellow to match the yellow sample. There was no evidence that she was more color-blind at the fovea. With the left eye all the colors were correctly and exactly chosen and named.

e. Additional confusion tests

The subject stood before a window and looked skyward through colored films and glasses. The right eye was tested first. Blue, yellow and green were easily recognized; red looked dark grey, and all mixed colors which contained red lost their red element; blue-green looked greyish. With the left eye all the colors were correctly named. When a film or glass was moved over from the left to the right eye, the subject said it always looked darker.

A rough test was made to determine whether the subject used color associations in recognizing greens, etc. Seven black and white reprints of famous pictures were colored contrary to nature with crayons and water colors,—a face was painted a strong green, a cow purple, a tree red, four kittens were colored red, yellow, green and grey, a sky green, etc. With her right (color-blind) eye, the subject detected the trick in most cases, naming all the strong greens, yellows and blues correctly. None of the reds appeared to her to have color; and in those places where the green was weakened by the black of the print underneath the thin paint she failed to detect the green.

From these confusion tests one must conclude that this protanope is unable, under ordinary conditions, to see red as a color, but that under the same conditions she is repeatedly able to recognize and correctly name various kinds of green,—even such greens as those upon the Nagel cards. Now since this subject seems to have perfectly normal color sensitiveness with her left eye, we must assume that she knows what the sensation of green is like, and when she correctly insists that a certain color seen with her color-blind eye is green, we have very strong evidence for the conviction that green (as a specific color quality different from yellow and grey) is included in the color system of her protanopic (right) eye. At the same time, her occasional difficulty with greens gave evidence of a lowered sensitiveness for that color, and seems entirely consistent with the later discovery that there is a certain region in the neighborhood of the blue-greens which this subject confused with a light red.

2. *Experiments in color discrimination*

a. *Determination of the color threshold with rotating discs¹*

Upon white discs with a radius of 95 mm. were pasted circular rings of the four standard Hering colors 5 mm. in width, at a distance of 60 mm. from the centre of the discs, one color being pasted upon each disc. Upon a fifth disc a strip of Bradley's neutral grey no. 2 was pasted, and this disc and a pure white one were interwoven with the discs bearing colors, and all mounted on the color-wheel together.

The subject was seated about one meter from the color-wheel, with her back to the source of light, and her left eye covered. The experimenter stood in front of the color-wheel when it was not in motion, to conceal it from the subject, in order that she might not know in advance what color was to be given. The experimenter would then draw out one of the colors or the grey, so that a small number of degrees were exposed, set the mixer rotating and ask the subject to name the colored ring. By varying the colors and the amount given, and by occasionally introducing grey to make sure that the subject was not merely guessing at the colors, a minimum amount was at length determined upon as the least amount of each color which the subject could correctly name. Frequent rests were given. After completing the series with the right eye, the experiments were repeated with the other eye.

TABLE I

Showing the Color Thresholds, as determined by means of rings of Hering paper upon white discs. (The determinations are expressed in degrees.)

			RED	GREEN	YELLOW	BLUE
Miss G. S.	Protanope	Right Eye	x	105	65	50
Miss M. S.	Deuteranope	Right Eye	110	x	40	25
		Left Eye	140	x	35	22
Miss H. E.	Deuteranope	Right Eye	270	320	200	210
		Left Eye	285	320	250	210
Miss G. B.	Deuteranope	Right Eye	230	225	250	205
		Left Eye	210	210	285	180
Miss E. C.	Deuteranope	Right Eye	315	350	270	180
		Left Eye	325	330	260	180
Miss I. B.	Deuteranope	Left Eye	148	180	55	90
Average of 40 eyes,—20 women who made no mistakes with the Nagel test			21	22	25	21

¹It is a matter of considerable regret that it was impossible to use spectral lights for many of the experiments now to be reported upon. The recent work of Nagel, von Kries and their pupils shows the great advantage of such lights. But unfortunately the great cost of the apparatus necessary renders it unattainable in a small College laboratory. Rivers found the Lovibond Tintometer (44) very useful for quantitative determinations of the color sensitiveness of the natives of Torres Straits; but this apparatus, with a sufficiently large assortment of colored glasses, proved too expensive for us.

In the foregoing table the results obtained with Miss G. S.'s right eye are compared with the results of similar tests upon five deuteranopes, the last two of whom are mild cases; and with the results of tests upon normal eyes. An "x" in the table indicates that no color was recognized even when the whole ring (360°) was exposed to view.

Slight differences in the color sensitivity of the two eyes have been noted by many observers. Hence the eyes of all the subjects mentioned in this paper were tested separately, with the exception of the test with the Hegg sheet and the experiments in contrast. When there was not time to test both eyes, only the right was experimented upon, except in the case of Miss I. B. whose left eye was found to be weaker in color sensitivity than the right, and therefore more nearly comparable with the other subjects.

The foregoing experiment is subject to criticism upon the ground that the colors used were not the best ones for testing the color sensitivity of the color-blind. In making an equation of red and green with color-blind observers, it is often necessary to add blue to the red, or to the green, or to both these colors, in order to make them both appear grey. A second series of experiments was therefore performed using Hegg's pigments¹ instead of the Hering papers. Four discs 10 cm. in diameter were cut from Hering's grey paper number 14, and upon these discs rings 5 mm. in width were painted with Hegg's pigments, the rings being 60 mm. from the centre of the discs, as in the earlier tests. No. 14 grey was selected because that appeared to the writer and to two other normal observers to be the nearest in brightness to the grey upon the sheet of colors which is provided with the Hegg set.² Before the experiments upon the color threshold were begun, this sheet of colors was presented to Miss G. S., with the request that she name any colors she saw. Using her right eye, she at once named the blue and the green, but she recognized neither the red nor the yellow. The yellow she called grey, and said it was lighter than the grey band in the middle of the sheet; the red also seemed grey to her, but of the same brightness as the central grey. With her left eye she named the four "invariable" colors correctly, although she was at first a little uncertain about the yellow, and said it was a very poor yellow at best.

Table II presents the results of the tests with Hegg's pigments. The results with Miss G. S. are compared with those of

¹Baird (4, p. 29) gives an account of the way in which these colors were decided upon.

²Unfortunately no apparatus for the exact evaluation of brightness was available in the laboratory.

five deuteranopes. Miss G. B. was not tested with the Hegg pigments, so the results of the Hegg test with another deuteranope, Mr. A. H. P., are substituted. The superiority of the Hegg red and green for a threshold test with color-blind subjects is clearly demonstrated. Evidently these colors approximate the neutral bands of the partially color-blind. The high thresholds for blue and yellow may possibly indicate a slightly decreased sensitivity for these colors; but in view of the difficulty which normal observers have in distinguishing them, when mixed with a considerable amount of bluish grey, it is perhaps unwise to come to such a conclusion as yet.

TABLE II

Showing the Color Thresholds, as determined by means of the Hegg pigments upon a grey background. (Results are expressed in degrees.)

			RED	GREEN	YELLOW	BLUE
Miss G. S.	Protanope	Right Eye	x	75	100	70
Miss M. S.	Deuteranope	Right Eye	240	x	x	110
		Left Eye	250	x	270	50
Miss H. E.	Deuteranope	Right Eye	x	x	x	240
		Left Eye	x	x	x	240
Mr. A. H. P.	Deuteranope	Right Eye	200	x	210	90
Miss E. C.	Deuteranope	Right Eye	x	270	x	315
		Left Eye	x	340	x	x
Miss I. B.	Deuteranope	Right Eye	x	55	x	125
		Left Eye	x	x	x	70
Average of 40 eyes,—24 women who made no mistakes with the Nagel test			57.1	74.6	98.8	71.6

For the general thesis of this paper, the most important point in this table is this, that almost all of these color-blind subjects recognized either red or green repeatedly, when a considerable amount was given, and Miss G. S. was sure of green at 75°,—about the average for normal observers.

The value of the Hegg pigments as confusion colors was further tested by showing the sheet of colors painted with the Hegg pigments to a considerable number of color-blind subjects with the request that they name the colors. This sheet measures 16 x 10 cm. Across the middle there is a band of neutral grey 2.5 x 10 cm; on each side of this band are two patches of color 5 x about 6.75 cm. in area, the red and the green being on one side, the blue and the yellow on the other. The following table, number III, shows how these colors were named. Of course, this test is of secondary importance, since a shrewd subject might readily assume that the four fundamental colors were displayed and then guess correctly which was red and which was green. In general, however, the subjects did not seem to think of this; and as the table shows, the grey band was several times reported to be colored.

TABLE III

Showing the names that were employed in describing the Hegg pigments

		RED	GREEN	GREY	YELLOW	BLUE
Miss G. S.	Protanope	Grey	Green	Grey	Grey	Blue
Miss M. S.	Deuteranope	Pink	Grey	Grey	Brown	Blue
Miss E. C.	Deuteranope	Pink	Grey	Grey	Grey	Blue
Miss I. B.	Deuteranope	Red	Green	Grey	Yellow	Blue
Mr. A. H. P.	Deuteranope	Grey	Grey	Grey	Grayish-Yellow	Blue
Miss H. B.	Deuteranope	Grey	Grey	Grey	Brown	Blue
Mr. D. B. Y.	Deuteranope	Grey	Reddish	Grey	Grey	Blue
Mr. M. H. H.	Deuteranope	Grey	Green	Green	Brown	Blue
Mr. J. F. McD.	Deuteranope	Red(?)	Green	Pink	Yellow	Blue
Mr. A. B. C.	Deuteranope	Pink	Green	Green	?	Blue
Mr. C. R. B.	Deuteranope	Red	Brown	Grey	Red or Brown	Blue

b. Determination of the distance threshold for colors

Upon a sheet of Hering paper (no. 14 grey), four rows of squares were painted with the Hegg pigments, — three horizontal rows on the right half of the sheet, and one row at the middle of the left half. The sizes and colors of these patches were as follows:

	15 mm. blue, green, red, yellow.
2.5 mm. green, red, yellow, blue.	5 mm. red, yellow, blue, green.
	10 mm. green, blue, yellow, red.

The subject was stationed 14 meters from the card, with her left eye covered. She was asked to tell whether she saw any patches of color upon the grey sheet. As she could see none at that distance, she was asked to advance slowly until she could see some colored patch. At 3.5 meters she correctly named the largest blue square; at 2, the largest green square; and at 1.5, the largest yellow square. The smallest squares were recognized at 0.1 m. She wholly failed to recognize the red patches as colored.

TABLE IV.

Showing the distances at which small squares of colored paper were correctly identified (monocular vision)

DISTANCES (expressed in meters)	RIGHT EYE	LEFT EYE
14		15 mm. Red
13		
12		
11		15 mm. Green called Green or Blue
10		
9		
8.5		
8		15 mm. Blue; 15 mm. Yellow called White

7		
6.5		
6		15 mm. Yellow; 10 mm. Red
5.5		
5		
4.5		15 mm. Green; 10 mm. Blue; 10 mm. Yellow called White 10 mm. Green
4		
3.5	15 mm. Blue	
3		10 mm. Yellow
2.5		
2	15 mm. Green; 10 mm. Blue	5 mm. Blue; 5 mm. Red
1.5	15 mm. Yellow; 5 mm. Blue;	5 mm. Green
1	10 mm. Yellow; 10 mm. Green;	5 mm. Yellow; 2.5 mm. Red
.5	5 mm. Yellow; 5 mm. Green	2.5 mm. Blue; 2.5 mm. Green 2.5 mm. Yellow
.1	2.5 mm Blue; 2.5 Green; 2.5 mm. Yellow	

A comparison of these results with the results of similar experiments upon twenty normal women and the six deuteranopes mentioned in Tables I and II shows that Miss G. S. occupies a middle position between the two groups in her recognition of green. Normal women recognize the largest green squares at 9 meters, and the smallest at 1.5 m; Miss G. S. recognized the largest green squares at 2 m. and the smallest at 0.1 m. Three of the deuteranopes did not recognize the green squares of any size at any distance; and the three who did recognize them succeeded at about the same distance as Miss G. S. When one remembers that this subject's visual acuity, in her color-blind eye, is somewhat below normal, it is quite surprising that she recognizes green so well. Possibly her inability to see yellow or blue at a distance is to be explained in the same way, although the deuteranopes also have considerable difficulty with these colors, especially in their recognition of the small squares. Miss G. S. did not appear to detect the red patches at all. In the tests with her left eye, Miss G. S. compares favorably with the normal women.

The experiments in color confusion showed very plainly that with her right eye Miss G. S. is color-blind to all kinds of reds tried, but that she fails to recognize green only when it is weak or mixed with blue. The experiments upon the color threshold and the distance threshold gave similar results. To the Hering and the Hegg reds the subject is quite blind; to both the Hering and the Hegg green, however, she seems to be sensitive, failing to recognize them only when they are quite reduced in saturation, or at a considerable distance from her eye. Her color threshold for green is conspicuously lower than that of the other color-blind subjects. It would seem then that if the subject is blind to any kind of green, presented in saturated form, it must be of a somewhat different tone

from the Hering, Hegg, Holmgren, Nagel and Bradley greens. In the color equations to be described later, the particular green to which this subject is insensitive was determined. But it is already pretty obvious that this subject's color sensations are not limited to blue and yellow. If her two eyes were defective, one might perhaps explain her recognition of green from an employment of secondary criteria of some kind, such as we have to assume (10, 71) in subjects who repeatedly recognize greens and reds in experiments with colored papers, but are able to see only yellow and blue in the spectrum. But since Miss G. S. sees the colors normally with her left eye, she has a clear consciousness of green as a quality distinct from yellow or grey; and when she uses the word "green" to describe the sensations aroused by stimulation of her right (color-blind) eye, we must assume that she sees green as green.

c. Campimetry experiments

Preliminary experiments were performed with Hering papers. The standard red, yellow, green, and blue were placed upon a color mixer one at a time, and rotated behind a grey screen, through which a hole 15 mm. in diameter had been cut. Upon the screen were pasted strips of millimeter paper, leading away from the hole in four directions. The subject fixed her gaze upon a pencil point which was moved outward or inward. The results showed coincidence of the green with the yellow and blue zones, and relatively constricted color areas in the right eye.

Further tests were made with the Hegg pigments by means of a small perimeter. In these tests the stimuli were always introduced first at the extreme periphery, and every precaution was taken to prevent the subject from anticipating which color was to be presented. The left eye showed the normal color zones, and is quite comparable with the eyes of five normal women tested in the same way. The right eye again showed coincidence of the green with the yellow and blue zones, and constricted color areas. It is, of course, possible that in the outer color zone this subject confused green with yellow, though one would rather expect that this green would there appear grey, as it does to normal eyes. The matter is further complicated by the fact that the subject repeatedly recognized it as green. Blue also was twice called "green." The red disc was called "white" and seen far out beyond the color zones. Yellow and green were called "white" on the extreme periphery, but blue was twice seen first as "black."

d. Contrast and Negative After-Images

Ever since Stilling's suggestion, in the seventies, that contrast shadows might be successfully used for the diagnosis of color-blindness, many experimenters have tested the ability of the color-blind to obtain contrast colors (6, 15, 75) and colored after-images.¹ The writer has used various methods with Miss G. S. and the other subjects;—contrast rings upon rotating discs, tissue contrast (grey strip on a color, all covered over with tissue paper), the Hering contrast box after Ragona Scina's method, and negative after-images from colored patches of Hering paper 9 cm. square, upon grey cards 40 x 50 cm., the after-images being projected upon grey cards with a black fixation dot in the centre. The following Table shows the results, *i. e.*, the colors induced in the various experiments.

TABLE V

Showing the color-names which were employed in describing the induction effects in the contrast and after-image experiments

		RINGS	TISSUE	HERING BOX	NEGATIVE AFTER-IMAGES
Miss G. S.	Prot.	Blue Yellow	Blue Yellow		Blue Yellow
Mr. J. W. P.	Prot.			Blue and Yellow	
Miss E. C.	Deut.	None	None	Blue and Yellow	
Miss I. B.	Deut.	Blue Yellow			
Miss H. B.	Deut.	Blue Yellow		Blue and Yellow	
Mr. D. B. Y.	Deut.			Blue and Yellow	Blue Yellow

In general, these subjects seemed to be less sensitive to contrast than normal observers. Occasionally a subject would report a slight tinge of pink or green as an induced color, but from the other experiments upon the same subjects it seemed more likely that the words were used by chance, as color names are so often used by such persons. Generally blue was reported as the contrast color both for red and for green, though often only a brightness contrast was noted.

e. Rapidity of color discrimination

Miss G. S., the monocular protanope, and four of the women deuteranopes were tested for rapidity in sorting into 6 piles, 60 pieces of Milton Bradley paper 30 mm. square, mounted on pieces of white cardboard 88 mm. square.² All five subjects sorted tints and shades of blue and yellow more rapidly than

¹Von Hippel (29) and Guttman (16) report blue and yellow after-images from all colors.

²This test is modeled after that described by Henmon (20).

tints and shades of red, orange-red and green. Miss G. S. made the typical confusions with reds, putting them all in one pile which she called "black;" but she correctly sorted and named the greens, confusing only the darker shade of green with the standard green. The other subjects, also, showed considerable ability in recognizing and correctly sorting reds and greens. Of course, it is possible that in all such cases the color-blind is assisted by secondary criteria, and to make this test of real value colors should be used which are nearer to the reds and greens which the color-blind calls grey, such as the Hegg pigments. But even then one should have a slightly different set for protanopes and deuteranopes in order that the reds and greens may appear to be greys of equal brightness.

f. Experiments with Spectral Lights

(1). *With the Schmidt and Haensch direct-vision spectroscope*

The subject was first asked to look through the spectroscope toward a cloudy sky, and read off in wave-lengths the limits of all the colors she could see with her right eye. But she was unable to do this, because the lenses provided with the spectroscope were not suited to correct her visual defect.¹

A collection of pieces of Hering paper 4 cm. square (including 2 pieces of each of the 12 colored papers, and grays 1, 5, 10, 15, 20, 25, 30, 35, 40, 45, and 50) was then spread out upon a table; and, using only the right eye, the subject was asked to select papers like those seen in the spectroscope and arrange them in the same order.² After looking into the spectroscope several times, the following papers were chosen and arranged in the order given:—50 grey, 20 grey, 5 grey, yellow 5, green-yellow 6, green-blue 8, blue 10 and violet 11. From these results it seems highly probable that the red-end of the spectrum is colorless and shortened, and that she may have seen some green. Of course, there is the alternative that she *remembered* that green comes between yellow and blue in the spectrum; but then we must admit that she sees green in the colored papers.

When the papers were mixed up again, and the experiment was repeated with the left eye only, she put two squares of red 2, and one of yellow 5, in place of the greys at the red-end of the series.

The Schmidt and Haensch spectroscope was found to be a very convenient instrument for roughly determining whether or not a subject's spectrum was shortened at the red-end.

¹ See test by optician mentioned above p. 378.

² In 1878 Magnus (45) recommended the matching of spectral colors with Holmgren wools to avoid the use of color names.

TABLE VI
 Showing the length of the Spectrum, and the Colors which it was reported to contain. The numerical data are expressed in $\mu\mu$. All determinations were made with the right eye

	LIMIT	RED	ORANGE	YELLOW	GREEN	BLUE	VIOLET
Miss G. S.	?	Grey	Yellow	Yellow-Green	Yellow-Green	Green-blue-	Violet
Mr. J. W. P.	620		"	Yellow	Yellow	Blue	Blue
Miss M. S.	650	Red	"	Yellow	"	Blue	"
Mr. C. R. B.	650	Red	"	Green or Yellow	?	Blue or Purple	Blue or Purple
Mr. G. W. B.	650	Yellow	"	Yellow	Yellow	Blue	Blue
Miss E. C.	650	Red	"	"	"	Blue	"
Mr. A. B. C.	640	Orange or Yellow	"	"	"	Violet	Violet
Mr. M. H. H.	650	Red	"	Red	Red	Blue	Blue
Mr. J. F. McD.	650	Green	"	Green	Green	Blue or Violet	Blue or Violet
Mr. C. D. R.	650	Red	"	Yellow	Yellow	Blue	Blue
Mr. A. H. P.	650	Yellow	"	"	"	Blue	"
Mr. F. B. S.	650	Red	"	?	Green	Purple	Purple
Mr. D. B. Y.	650	Red	"	Yellow	Yellow	Purple	"

And while the subject was using the instrument, the experimenter asked what colors were visible. None of the subjects claimed to see more than three colors, although several of them recognized the instrument and knew what colors they ought to see. No great importance is attached to the use of color names in the following table, although several of the subjects who claimed to see colors other than blue and yellow showed a similar ability to distinguish these colors in other experiments. Other experimenters (28) have noted the tendency of color-blind subjects to see three colors when the whole spectrum is shown at once; and it was a matter for regret that our instrument had not an attachment for exposing one color at a time.¹ Table VI gives the results of these experiments.

(2). *Viewing a petroleum flame through a spectroscope*

It was attempted to determine the length of Miss G. S.'s spectrum by having the subject set the instrument in such a position that all color appeared at the right of the vertical line across the centre of the field of the spectroscope. The setting of the instrument was read off in degrees. The instrument was then adjusted until all color was at the left of the vertical line. The following table shows the results obtained.

TABLE VII

Showing the Limits of Visibility in the Spectrum. (Results expressed in terms of our scale readings.)

			RED END	VIOLET END
Miss G. S.	Right Eye,	Protanopic	285°	282° 9'
	Left Eye,	Normal	285° 40'	281° 28'
Miss M. S.	Right Eye,	Deuteranopic	285° 33'	281° 21'
Miss L. D.	Right Eye	Normal	285° 40'	281° 40'
Mr. S. P. H.	Right Eye	Normal	285° 26'	281° 25'

The typical shortening at the red end is plainly seen in the case of Miss G. S.'s protanopic right eye; there would seem to be a shortening at the violet end also.

(3). *Experiment with bisulphide of carbon prisms*

A wide beam of light was sent from an electric lantern through two slits and then through two bisulphide of carbon prisms² placed side by side in a darkened room. The two spectra produced, were interrupted at two meters by a screen in which two slits had been cut, so that on the next screen beyond, two patches of colored light could be seen, each 6 cm. high and 1 cm. wide. The prisms rested upon discs of card-

¹Cohn (7, pp. 84 f.) describes such an instrument.

²Made by Wm. Gaertner & Co., Chicago, Ill., Cat. No. L, 4025.

board, which were held in place by thumb tacks pushed through the centre of each. These discs could be rotated easily to right or left; and by this means the colors seen on the farthest screen could be changed from red to violet. The movements necessary to cause these changes in color were so slight that the vividness of the colors was not materially affected; and in the case of red, green, blue and violet, the colored patch was approximately monochromatic. Yellow always had a fringe of orange on one side, or of green on the other.

The subject was seated in front of the last screen, about one meter from it, where she could not see any colors except those shown in the two patches.

As a preliminary experiment, the subject was asked to close her left eye, and name the colors shown her. Only one prism was used in this experiment. This prism was turned so that the extreme red appeared on the farther screen. The subject reported no color, so the prism was very slowly rotated until the subject said she saw "yellow." The colors then in the patch were yellow and orange. The prism was then moved slowly again; and as soon as the yellow began to turn greenish, the subject said she saw "green." She continued to report green until that color was no longer visible to the experimenter, and then she at once said she saw "blue," which she continued to report well out into the violet. The subject was then asked to rest her eyes, and the prism was turned back until the patch was a strong pure red. The subject was now asked to tell what she saw, and she replied that she saw a patch of "grey."

α. Color comparison with two prisms

The subject turned away from the screen, and both prisms were illuminated. The left prism was set so that it gave a pure green patch, the right prism at the extreme red. The subject was then asked to look at the screen with her right eye only, and report what she saw. She replied that the left patch was green and the right grey. She was then instructed to watch the patches and tell when they looked just alike. The right prism was then slowly turned, and not until both patches were pure green did the subject judge them alike. Evidently it was impossible to match spectral green with spectral red or yellow of any kind.

The subject turned away from the screen again; the left prism was now set at red, and the right at the extreme violet. She reported that she saw grey at the left and faint blue at the right. The right prism was slowly turned, with frequent stops, but she was not satisfied with the match until this prism also gave a pure red patch. In this test the attempt to match

red with blue-green, green or yellow was unsuccessful. No attempt was made to equate brightnesses; the subject insisted throughout that there was a qualitative difference.

These experiments with spectral lights emphasize the same fact that was indicated in the experiments with colored papers and the Hegg pigments,—that the subject is blind to red, but not blind to green.

β. Determination of the Color Threshold with Spectral Lights

Only one prism was illuminated, and an episkotister was set up just beyond the screen with the slits in it. The prism was turned to give pure green on the patch, the episkotister set at 5° and rotated by means of an electric motor. The subject was asked to look with her right (protanopic) eye at the screen where the left colored patch had appeared in the earlier experiments. She reported that she could see nothing. She closed her eye and the episkotister was set at 10° . The subject now reported that she saw green, although there had been no intimation on the part of the experimenter that green would be the first color shown. The prism was moved back and forth, but she could recognize no other color. The episkotister was set at 8, 6 and 4 degrees in successive tests; but below 10° the subject saw no color. At 6° she claimed she saw a faint line of light when the prism was turned to green.

The episkotister was again set at 10° , and the prism moved back and forth; green alone was recognized. At 15° no other color was seen. The episkotister was opened 5° at a time, in successive tests, and the prism turned through the series of colors, with each new opening. At 30° , blue was recognized and at 55° , yellow.

No definite attempt was made in these experiments to attain dark adaptation in the right eye. There was considerable light in that part of the room where the lantern was stationed; and in most of the experiments the subject gazed about freely during the intervals between tests. In this particular series of tests, however, the subject was asked to close her eye while the episkotister was being adjusted, so that she might not see what changes were being made. The conditions, then, were favorable for dark adaptation, and we cannot say with certainty whether the extreme brightness of the green was merely a manifestation of the Purkinje phenomenon in dim light, or the displacement of the maximal brightness which we would expect of a protanope even without dark adaptation.

The experiment was repeated with the left eye only, and all four colors, red, yellow, green and blue, were repeatedly recognized and correctly named at 5° opening.

Of course these experiments with a home-made spectral apparatus are open to the grave criticism that no provision was made for determining objectively just what lights were given to the subject,—instead of a statement of the exact wave length of the patches of color exposed, the reader has only the writer's assurance that red, green, etc., were given. But the apparatus necessary to conduct this experiment objectively was too expensive to be bought from the funds of a small laboratory; and since the results of these rough tests are quite in harmony with those obtained by the use of colored papers, they certainly add considerable weight to the total mass of evidence in favor of the view that this protanope is not blind to green. It should also be said that repeated tests have shown the experimenter's color sense to be perfectly normal, so that there is no possibility that wrong colors were given by mistake.

From the experiments thus far described, certain conclusions regarding the color sensations of Miss G. S. seem amply justified. There can be no doubt that her right eye is totally insensitive to the quality *red*. She never used the word *red* to describe any sensations obtained through her right eye, in any of the experiments. It seems equally clear that her right eye is not insensitive to the quality *green*. With scarcely a single error, she repeatedly recognized green in the Nagel cards, the Holmgren worsteds, the Milton Bradley and Hering papers, in gelatine films, in prints painted with water colors, in spectral lights, and in the Hegg pigments. And since she is familiar with the quality *green*, through the use of her normal left eye, we must grant that the sensation which she correctly names "green", when her right (protanopic) eye is stimulated, is probably the same sensation-quality which normal persons describe by the use of the term "green." In other words, her spectrum is not reduced to blue and yellow, although in general she shows the ordinary characteristics of protanopia. When the greens used are reduced in saturation or in brightness, however, or when only a very small patch of color is presented, Miss G. S. shows herself somewhat less sensitive to green than normal persons. She shows a slightly sub-normal sensitivity for blue and yellow also; but these colors are less affected than green.

It is considerably more difficult to decide whether the other subjects see either red or green as normal persons do. There is, of course, no doubt that color-blind subjects can distinguish many reds and greens from each other and from yellows and blues. Some of the subjects mentioned in this paper showed a good deal of facility in distinguishing colors, so that from day to day the writer hesitated whether or not to class

them as color-blind. This decision was, however, somewhat simplified by a change in mental state on the part of several of the doubtful subjects. Misses H. E. and E. C., for instance, long maintained that it was ridiculous to class them as color-blinds, insisting that they could distinguish the reds and greens one encounters in the daily use of colored objects, even though they could not always correctly name the weak colors with which they were tested in the laboratory; but after a considerable number of equations had been made, and the results exhibited to them,¹ these subjects took a more calm and objective attitude in the matter, frankly giving themselves up to the task of ascertaining just how much their color sense was affected, and even recounting instances in which their friends had detected their errors in the naming of colors. In order to eliminate the "secondary criteria" by which such subjects are supposed to distinguish colors,² as many as possible of these subjects also were tested with spectral lights, using an arrangement of apparatus similar to that used with Miss G. S. But a lime light was substituted for electricity, the experiment was performed in a dark room, and the carbon bisulphide prisms were placed one above the other with a movable slit in front of each to give the different colors. Three of the subjects,³ the protanope and two deuteranopes, reported only blue and yellow; red, yellow and green all looked alike to them, but no equation between red and blue-green could be made, because the blue-green was reported to be whitish and lacking in the yellowish tinge which they saw in the red. These three subjects, however, are extreme cases of color-blindness. Misses E. C. and I. B. insisted that they saw red as a color distinct from yellow, and repeatedly recognized it. Miss I. B., whose color defect is the least marked of all the cases reported in this paper, recognized green also with quite as much certainty as the extreme cases did blue and yellow. And since these two subjects are the ones who have showed the greatest keenness in distinguishing reds and greens in the other tests, one must interpret this as another indication that only extreme cases of partial color-blindness are limited to blue and yellow.

3. Color Equations

a. Equations with the Hering color sense apparatus⁴

This apparatus is so constructed that the subject, on looking

¹Following Maxwell's suggestion (46, p. 287) the subjects were occasionally requested to look at their equations through a colored glass. The inequality in the mixtures thus demonstrated helped greatly in inducing the objective attitude.

²Differences in brightness, saturation, color associations, etc. 10 p. 210.

³Mr. J. W. P. (protanope), Mr. D. B. Y. (deuteranope), and Miss H. B. (deuteranope).

⁴This instrument is described and figured by Hering (24).

down a dark tube may see a disc one half of which is colored by light filtered through one colored glass, the other half by a mixture of lights transmitted through two glasses. The intensity of the color presented is varied by the amount of light reflected through the colored glasses from movable reflectors, whose position is indicated upon a dial. It is thus fairly easy to form color equations, such as red = green + blue, etc.; and to read off from the dial the amount of light passing through each glass. When fully open the reflectors register 120 units. The following table shows the result of the experiments with this apparatus upon Miss G. S. and various other subjects.

TABLE VIII

Equations with the Hering Color Sense Apparatus. (Results are expressed in degrees, 120 being the maximum reading possible)

		GREEN = RED + BLUE			RED = GREEN + BLUE		
Miss G. S.	Prot. R.	40	120	120	120	60	120
Mr. J. C. H.	Deut. R.	30	70	90			
	Deut. L.	30	62	90			
Miss M. S.	Deut. R.	40	120	75	60	120	50
	Deut. L.	30	120	95	35	120	90
Miss H. E.	Deut. R.				80	60	120
	Deut. L.				60	50	120
Miss G. B.	Deut. R.	50	45	120	120	120	120
	Deut. L.	80	85	120	120	120	30
Miss H. B.	Deut. R.	120	120	0			
Mr. D. B. Y.	Deut. R.	120	120	0			
Miss E. C.	Deut. R.	120	55	120	120	120	120
	Deut. L.	120	120	120	120	120	120
Miss I. B.	Deut. R.				33	25	35
	Deut. L.	20	55	20	120	50	80
Miss L. W.	Deut. R.	120	25	50			
	Deut. L.	120	40	60			
Mr. J. W. P.	Prot. R.	108	120	30			

The wide variation in the results for different subjects can probably be partly accounted for by differences in the amount of sunlight on different days, or at different hours of the day.¹ But the constant insistence of many subjects, besides the monocular protanope, that they saw red or green when more than a certain amount of either color was used, gives added evidence that many color-blind are not dichromates. Miss G. S. was very sure she saw green when 50 was given alone, or 70 mixed with blue.

The instrument would be greatly improved if four glasses could be used at the same time, so that blue could be mixed both with red and with green simultaneously. The subjects

¹A series of experiments upon Miss H. E. (Deut.) extending over 5 days in the spring of 1909 showed a variation of from 20 red on a bright day, to 120 red on a dark day.

were told that a colorless equation was to be made; and the small amounts of red or green allowed when given alone may be attributed to the natural confusion of these colors with yellow, and the insistence that the single glass should appear colorless. A slight admixture of blue would obviate this difficulty. But the small amount of red or green allowed by some subjects, when mixed with a large amount of blue, remains as evidence for the main thesis of this paper,—that red or green sensations may be possible to the partially color-blind.

It seems quite likely that the neutral grey bands of different deuteranopes may occupy slightly different regions of the spectrum (40, p. 158). If this is the case, those whose bands are most nearly represented by the particular red and green glasses of the Hering apparatus would accept a much larger amount of red or green when given alone in this instrument.

b. Equations obtained with rotating discs

Ever since Maxwell's work (46, 47) with rotating discs in the fifties, it has been customary to make color equations which shall indicate the extent of color confusion to which color-blind subjects are liable, and to determine whether all colors can be matched by the mixture of blue and yellow, black and white.

(1). *The Rayleigh equation*

It is commonly asserted (41, 64) that all equations which hold for normal observers will be found to hold also for the partially color-blind. From a theoretical point of view, one would expect that deuteranopes, at least, who are blind to both red and green, would accept all normal equations; and the writer found that all the deuteranopes tested with such equations did accept them. But in protanopia, the shift of maximum brightness might be expected to vitiate the equation somewhat. It was mainly to test the validity of this assumption that Miss G. S. was tested with the equation red + green = yellow; but the unexpected result of the experiment led to the testing of a number of the other subjects in the same way.

The normal Rayleigh equation,—the equation accepted on a bright day by a number of normal, trained observers, from which 30 untrained observers varied by only about 10 degrees,—was presented to the subject. Miss G. S. found the mixture much too green, and was not satisfied until the green was reduced to 63°. All the other subjects accepted not only the normal equation, but also wide variations from it. The following table gives the normal equation with the

Hering papers, the equation formed for Miss G. S. and the extreme amounts of red and green accepted by the other observers. The amounts of yellow, black and white are omitted, as not pertinent to the question.

TABLE IX
Rayleigh equation of color-blind subjects

		RED + GREEN = YELLOW + WHITE + BLACK				
Normal Equation		175	185	33	49	278
Miss G. S.	Prot. R.	297	63	70		290
		Extremes allowed by other color-blind subjects				
		RED + GREEN		RED + GREEN		
Mr. J. W. P.	Prot. R.	300	60	0		360
Miss M. S.	Deut. R.	315	45	90		270
Miss H. E.	Deut. R.	225	135	110		250
	Deut. L.	225	135	75		285
Miss E. C.	Deut. R.	182	178	65		295
	Deut. L.	190	170	95		265
Miss I. B.	Deut. R.	192	168	160		200
	Deut. L.	190	170	125		235
Miss L. W.	Deut. R.	190	170	160		200
	Deut. L.	200	160	160		200
Miss H. B.	Deut. R.	190	170	130		230
Mr. D. B. Y.	Deut. R.	300	60	0		360
Mr. A. H. P.	Deut. R.	178	182	50		310

The wide variation in the amounts of red and green accepted in the Rayleigh equation by these subjects is in striking contrast with the results obtained with Miss G. S. who would not allow a variation of more than 5° in the green. In this she resembles the anomalous trichromates discovered by the use of this equation, as she does also in her sensitiveness to green in the other experiments. But in view of the fact that, unlike them, she is completely lacking in sensitivity to red, has a neutral band in the blue-green (*cf.* the experiments with the Hering color-sense apparatus p. 395), and appears to get no contrast colors nor after-images from red or green, she would seem to be more properly classed with the protanopes than with the anomalous trichromates. Nagel (52) reports a similar experience with the Rayleigh equation, in comparing his own vision with that of two normal observers. Their equation, made up of about 180° each of red and of green, seemed very red to him, and had to be changed to between 140 and 150 red on a dark day, and to between 89 and 95 red on a bright day. His variation was then hardly more than that of Miss G. S.

It will also be noticed that some subjects show a much wider variation than others. The subjects who vary between the widest limits in this equation are the subjects who made the worst confusions of colors in the other tests, and have therefore been classed by the writer as extreme cases of color-blindness.

The most striking point in this table is the very small amount of green allowed by Miss G. S. When more was introduced, she at once objected to it, insisting that the mixture then had a greenish tinge. As the subjects were kept in complete ignorance in regard to the changes made in these equations, it is difficult to understand how she could repeatedly make correct judgments, if she did not see green somewhat as normal persons do.

The relatively small amounts of red and green allowed by the last three subjects is also striking. When more red or green was added, they were quite as sure of the change as was Miss G. S. in the case of green; and no increase in the amount of blue served to cancel the additional red or green, as one would expect if red and green appear yellowish to these subjects. To be sure, other tests have shown that these three subjects are mild cases of color-blindness; but such cases are quite valuable for the main contention of this paper, that people properly classed as color-blinds have some sensations of red and green.

(3). *Monocular comparison of colors*

In order to test still further the claim that all colors perceived by the color-blind can be matched by mixtures of blue and yellow, and at the same time to obtain a full statement of the appearance of the different colors, each of the Hering papers was rotated before Miss G. S.'s protanopic right eye, and matched by a mixture of colors rotated before her left eye. Two color-mixers were set up and operated behind a screen, and the subject looked through two blackened cardboard tubes 6 cm. in diameter and 55 cm. long. In this way she saw only the standard papers with her right eye and only the mixtures with her left. The results appear in the appended table.

TABLE XI

Showing how the Hering papers appear to a protanopic eye

	WHITE	BLACK	YELLOW	YELLOW- GREEN	GREEN	BLUE- GREEN	BLUE
Red No. 1	35	325					
Red No. 2	10	340	10				
Orange 3	75	170	115				
Y-Orange 4	105	62	193				
Yellow 5	50	175	135				
Y-Green 6	12	22	55		271		
Green 7	13	32		150		165	
B-Green 8	42	68			130		120
Blue 10	68	78					214
Violet 11	20	265					75
Purple 12	70	220					70

In this experiment, the colors were presented to the right eye in random order; and in each equation, the attempt was made to match the color presented to the right eye by a mixture of black and white with yellow or blue. Green was introduced only after every effort to make an equation without it had failed, and green had been insisted upon by name. As the table shows, there was no need of red to match red or orange. This experiment then gives additional evidence in support of our contention that this protanope perceives green but not red.

A long series of color equations was formed for two deuteranopes, Misses M. S. and H. E., in which the attempt was made to match mixtures of red and yellow, red and blue, green and yellow, and green and blue, by mixtures of blue and yellow with black and white. In these tests, the equations were formed by the use of large and small discs on a single color-mixer, and the mixtures to be matched were given in irregular order, a few at a time, through a period of three weeks. In the case of Miss M. S. green was not distinguished at all; but red was regularly distinguished from yellow and grey mixtures whenever a large amount of red was used, even though the red was mixed with considerable yellow or blue. Miss H. E. recognized both red and green when presented in large quantities.

In order to gather upon a single page a large part of the evidence for the contention that partially color-blinds are not dichromates, excepting in extreme forms of the defect, the following table has been constructed. In the vertical column are given the tests which seem of most importance for this question, and which were used with a considerable number of subjects. Under the initials of each subject, R. or G. is used to indicate that the subject showed considerable ability in recognizing red or green under the conditions of the various experiments mentioned; "con." indicates that the subject made the typical color-blind confusions, though occasionally distinguishing red or green correctly. The proportion of confusions to recognitions gives a fair idea of the degree of the color-blindness in each of the subjects. Miss G. S., Prot., distinguished green in practically every test, and the other women all recognized red or green (or both) repeatedly; the three men appeared to use the color names capriciously, although occasionally applying them correctly. They are probably as insensitive to both red and green as Miss G. S. is to red and Miss M. S. is to green.

The Stilling plates were not obtained until after the first four women mentioned in this Table had left college. Thirteen color-blind subjects have been tested with these plates, one

TABLE XII

Summarized Statement showing the Comparative Data obtained from the various observers. In this Table, Con. expresses the fact that the observer generally confused red and green color-stimuli in the test or tests indicated, while the initials R., G., express the fact that the observer recognized red or green fairly well under the conditions of the experiment.

	PROTANOPES		DEUTERANOPES								
	G. S.	J. W. P.	D. B. Y.	M. S.	H. E.	G. B.	H. B.	A. H. P.	E. C.	I. B.	L. W.
Nagel Cards	G.	Con.	Con.	Con.	R.	R.	Con.	Con.	R.	Con.	R.
Holmgren Worsteds	G.	Con.	Con.	Con.	Con.	R. G.	R.	Con.	Con.	Con.	
Stilling's Plates	Con.	Con.	Con.	Con.	Con.	Con.	Con.	Con.	R. G.	Con.	R.
Hering Color-Sense Apparatus											
Color Threshold (Hering Papers)	G.			R.	R. G.	R. G.		Con.	Con.	R. G.	
Color Threshold (Hegg Pigments)	G.			Con.	Con.			Con.	Con.	Con.	R.
Hegg Sheet	G.		Con.	R.			Con.	Con.	R.	R. G.	
Spectroscope (Schmidt & Haensch)	G.	Con.	Con.	R.				Con.	R.	R. G.	
Spectrum (Bisulphide of Carbon Prisms)	G.	Con.	Con.	Con.			Con.	Con.	R.	R. G.	
Rayleigh Equations	G.	Con.	Con.	Con.	R. G.		R.	Con.	Con.	R. G.	R. G.
Dichromate Equations	G.	Con.	Con.	R.	R. G.	R. G.	R.	Con.	R. G.	R. G.	R. G.

protanope, Mr. J. W. P., nine men and three women deuteranopes. All except two of these subjects read Plates 4 and 12 correctly, though according to Stilling (72, p. 11) deuteranopes should read only 4 and 13-15, protanopes only 1, 12, and 13-15. Five of these deuteranopes read some figures in Plate 1; and three read parts of 5, 6, 7, 8 and 9. Mr. J. W. P. (Prot.), read 1, 12, 13-15; Miss E. C. was the only subject who had real difficulty with 12. She read parts of Plates 1, 2, 3, 4, 5, 6, and 8, and the whole of Plates 13-15.

D. CONCLUSIONS

What, then, must be our conclusion regarding the color-system of the partially color-blind? How many qualitatively different sensations of color does he possess? From a study of the literature, no less than from an experimental investigation of numerous cases of defective color-vision, the writer has been convinced that partial color-blindness is not identical with dichromatism. The statement that sensations of blue and of yellow alone are possible to the partially color-blind cannot be reconciled with our findings. A brief review of the evidence which has been brought forward in the literature, in support of the contention that dichromatism is identical with partial color-blindness, will make our position clearer.

1. Certain color-blinds, who have made a careful study of their color-systems, have declared that they are limited to sensations of blue and of yellow (Dalton, Pole). Opposed to this is the testimony of five of our observers (Misses M. S., H. E., G. B., E. C., and I. B.) that red and green are specifically different color qualities from yellow and grey.¹ There are doubtless cases of defective color vision where only sensations of blue and of yellow are possible. Of our observers, Messrs. J. W. P., D. B. Y., and A. H. P., probably belong to this extreme type of defective, which is represented by Dalton and Pole. But this conjecture does not justify the inference that all partial color-blinds belong to the extreme type. The evidence which has been presented in this paper supports the contrary view,—that many intermediate or transitional stages and degrees of abnormality may be found to exist between dichromatism and normal color-vision,—and that, moreover, these intermediate forms need not be identical with anomalous trichromatism.

¹Nagel (52, p. 32) came to the conclusion that he, a deuteranope, saw red as a specific sensation-quality when an extra-foveal region was adequately stimulated; and Schumann (68) reports that he also, a deuteranope (?), can see red as red, and can distinguish it from yellow. But Guttman (15) classes Schumann as a green-anomalous trichromate.

This view is further supported by the inferences which may be drawn from our findings regarding color confusions and color naming. The repeated recognition of greens and of reds throughout, and even under relatively unfavorable conditions, furnishes a body of indirect evidence which cannot be ruled out of court by assuming the participation of secondary criteria.

2. When reds and greens were presented under favorable conditions of stimulation, many of our observers have wholly failed to match them with mixtures of yellow, black and white. When the conditions of stimulation are unfavorable, reds and greens may be matched with such mixtures, or with each other, if blue be added to one or to both. But when a considerable amount of red or of green was employed, the red or the green was seen in the mixture by all of our less pronounced cases of color-blinds, and by certain of the extreme cases, even when every precaution was taken to eliminate the influence of 'chance' and of 'guessing.' The evidence which is furnished by our color equations, then, points to the existence of specific sensations of red and of green in the color-systems of the less pronounced cases of color-blindness.

3. This paper has included no data derived from an examination of acquired and temporary defects of color vision. In the opinion of the writer, it is premature and unsafe to seek for analogies between these atypical cases and cases of congenital color-blindness, although Stilling (71) seems to advocate such a procedure. The findings of other investigators, however, (38) raise a significant question, whose ultimate solution promises to support the thesis of the present paper. If sensitivity to green may lapse before sensitivity to red is lost, and if transitional forms between trichromatism and dichromatism occur in acquired color-blindness, what theoretical warrant can there be for refusing to believe that an analogous series of transitional forms occurs in congenital color-blindness? The less pronounced cases which we have described would fit into such a series.

4. Light is thrown upon the general problem of abnormal color-systems by a consideration of the phenomena of indirect vision. Recent explorations of the peripheral retina (4, pp. 53 f.) have yielded results which support the thesis of the present paper,—that retinal function lapses, when it does lapse, in a gradual and not in an abrupt fashion. "The whole retinal surface, with the exception of the macula and the blind spot, is endowed with a similar function, to the extent, at least, that no region possesses a capacity which is wholly lacking in any other region. The color sensitivity of the periphery is unquestionably less acute than that of more central areas; and in consequence of this diminished sensitivity

a constant stimulus may arouse different sensations at different regions. It cannot, however, be said that any part of the normal retina, save the macula and the blind-spot, is wholly or even partially color-blind. For the whole manifold of sensation qualities which any region is capable of furnishing may, under appropriate conditions of stimulation, be furnished by every other region." (4, p. 65.)

5. Cases of monocular color-blindness constitute the crux of the whole question. Our review of the monocular cases which have been reported in the literature showed that, with the exception of von Hippel's, they reveal nothing but meagre experimentation, glaring contradiction, and theoretical bias. A survey of Table XII (p. 401 of this paper) shows, in striking form, the trend of the evidence which has been obtained from a study of Miss G. S. Green was recognized, and its specific quality was insisted upon, in almost every experiment with spectral colors, as well as with pigments,—colored papers, glasses, gelatines, etc. There can be no justification for the statement that she sees only blue and yellow. Yet she is clearly a protanope, and not an anomalous trichromate. If we grant that von Hippel's patient saw only blue and yellow, must we not also grant that Miss G. S. sees green, blue and yellow? This assumption is supported by abundant indications that many others of our color-blinds possess a similar sensitivity to red or to green.¹

There seems, then, to be a large mass of evidence, direct and indirect, which attests the presence of sensations of red and green in the color systems of the partially color-blind. The reader who still insists that partial color-blindness is identical with dichromatism must find some means of explaining away this mass of evidence. It seems much more reasonable to admit that a strict classification of color defectives is necessarily artificial; to assume the existence of slight degrees of variation from normality, and numerous transitional forms between normality and total color-blindness; and to regard dichromacy as an extreme variation, and not as a typical condition of the partially color-blind.

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¹ Nagel (57) reported that among thirty dichromates, both protanopes and deuteranopes, who were recently examined by him, none failed to recognize various shades of red when a sufficiently large area of the retina was stimulated.

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RECENT FREUDIAN LITERATURE

By RUDOLPH ACHER.

1. FREUD, S. *Eine Kindheitserinnerung des Leonardo da Vinci*. Wien, 1910. 71 pp.

The one fundamental assumption of all practical experts in psychoanalysis is that all psychic phenomena including dreams, gestures, automatisms and reveries are governed by law and order, and have a causal sequence which it is possible to discover if the proper data are at hand. Psychoanalysts not only make this assumption theoretically but they proceed to lay bare these laws and principles of psychic life in concrete cases by an examination and interpretation of the psychic manifestations. The materials for psychoanalytical investigation are the facts of the life-history of the individual, including, on the one hand, the environmental influences and, on the other hand, the reactions of the individual to this environment. Supported by his knowledge of the psychic mechanism, the psychoanalyst attempts to grasp the dynamic factors of the individual's make-up, and to discover the sources of the mental motive power, as well as its later transformation and development. If the psychoanalysis is successful, the inner characteristics of the individual personality resulting from inner energy and outer influences are explained. While they deal largely with a class of persons whose mental condition is pathological and who might be considered more or less abnormal, they strongly insist that the distinction between normal and abnormal cannot be sharply drawn, and that the laws and principles which they discover in the psychic life of their patients hold for normal life also. Thus they proceed to apply the laws of psychic life, which they have discovered, to an interpretation of the life and character of historic personages, as well as to characters in drama and fiction. Their aim is to lay bare the very inmost workings of the human soul by an interpretation of its manifestations. This does not seem so hazardous in cases where there are sufficient data at hand concerning the lives of the persons under consideration. But they do not stop here. In cases where many of the facts of the lives of noted characters are wanting and the data meagre they attempt to fill out the gaps by making the known facts tell the full story which they implicitly contain. A certain apparently insignificant experience or impression of the person under consideration is made to bristle with meaning and significance under the magic touch of the psychoanalyst.

Perhaps the boldest of these attempts to give a new and fuller interpretation to a great historic character is that of Freud in his treatment of the childhood memories of Leonardo da Vinci. Although Freud does not claim absolute authenticity for his findings, he declares that they have a reasonable degree of plausibility, and that they seem to him more satisfactory than other attempts to account for this remarkable character. An effort will be made in what follows to give a summary of the main points in Freud's analysis of this man.

The significant facts of the life of da Vinci, so far as they are known, are given by Freud and are somewhat as follows:

Born in 1452, da Vinci was one of the foremost and most versatile characters of the Italian Renaissance. He was not only a paramount painter; he was also a noted and original scientist. In fact these two capacities were never quite separated from each other, the spirit of investigation always manifesting itself along with the artistic genius; and, in the end, the former almost wholly overshadowed the latter. He was not only great but well balanced, being possessed of a keen intellect, a strong body, an admirable address and a happy and lovable disposition. His scientific interests made him a worthy forerunner of Bacon. He prosecuted all kinds of researches; he dissected the bodies of horses and of men, built flying machines, studied the nourishment of plants and their reactions to poisons. It is said that at thirty he made this famous representation to the Duke of Milan: that he understood instruments of war and implements of peace; that he could construct bridges both light and strong; that he could cut off the water from the trenches of a besieged fortress, make pontoons and scaling ladders, and construct cannon which would be light and easy to transport, but which would throw small stones like hail; that in times of peace he could construct buildings both public and private, conduct water from place to place, execute sculpture in marble, bronze or clay; and that in painting he surpassed all of his contemporaries.

As has already been indicated, his scientific interests caused him to devote less time to painting, often led him to abandon unfinished works, and made him careless concerning the condition of his products. In these respects he was peculiar. The slowness with which he worked was proverbial. He worked three years on the "Lord's Supper" in the cloister at Santa Mariadelle Grazie at Milan, after the most painstaking preparatory study. He would, at times, work from daylight until dark without taking the brush from his hands. Then days would pass when nothing was attempted other than an examination of the work and an inward testing of it. He worked four years on the portrait of Monna Lisa, the wife of Francesco del Giocondo, without finishing it.

The extraordinary number of preparatory sketches in his note book, and the great number of notes which he made as to motive in his paintings show that carelessness or unsteadiness had nothing to do with his attitude toward his work. On the contrary, the very extent of his preparation and the great amount of preliminary study made the wealth of possibilities so great that definite decision was often difficult. This led to a sort of inhibition in the execution of his work. The slowness with which he worked was a symptom of this inhibition and prophetic of his later turning away from painting entirely. He was never aggressive, and always avoided opposition and quarrels. He refused to eat meat because he thought it not right to take the lives of animals. It gave him great pleasure to buy birds in the market place and give them their freedom. He severely arraigned war and bloodshed; and he called man not king of the animal world so much as the worst of wild beasts. But this feminine tenderness of feeling did not prevent him from leading condemned criminals to their execution in order that he might study their horrified facial expressions and draw them in his note-book; neither did it prevent his drawing the most horrible weapons, nor his entering the service of Cesare Borgia as chief military engineer.

What is known about his sex life is limited, but significant. In a time which saw boundless sensuality struggling with gloomy asceticism, da Vinci was the embodiment of sexual indifference,—a thing which one would scarcely expect to find in an artist who sets forth in his work the beauty of woman. His writings, which deal not only with the most profound scientific problems but also contain much varied and indifferent matter, are, as a rule, free from erotic reference to a degree scarcely found in the literature of our own day. In this respect he was in marked contrast with other

great artists, who took peculiar pleasure in setting forth their fancy in erotic and even obscene expressions.

There is no evidence that he ever loved a woman or had any spiritual intimacy with one, such as Michel Angelo had with Vittoria Colonna. There is good evidence that he had homosexual tendencies which were however either sublimated or, in the main, successfully repressed.

This peculiarity of emotional and sexual life, in connection with his double nature as artist and scientist, is, in the opinion of Freud, to be understood in only one way. He subordinated all feeling and emotion to intellectual pursuits. This idea is expressed by da Vinci himself, in his "Tractate on Painting," in which he defends himself against accusations of being irreligious. He says these accusers may well hold their peace, for to know and love the creator we must understand his works. Great love springs from great knowledge of the loved one. This, as Freud points out, is not true; for love is an emotion, and thought about an object tends to deaden the emotion aroused by it. Da Vinci's idea was to withhold emotion and make it subordinate to thought; and this he succeeded in doing. He neither loved nor hated; was indifferent to good and bad; was always calm and unperturbed, because he subordinated all else to the interests of thought. He was, however, not apathetic. He did not dispense with the divine spark that is either directly or indirectly the dynamic force of all human activity; but he transformed it into the impulse to know. He devoted himself to an investigation of natural phenomena, with a persistence and steadiness that can come only from the enforcement of transformed feeling. Only after the conquest of knowledge did he allow the inhibited feeling to break forth as the stream that has driven the wheel, and is then allowed to go on its way. He has been called the Italian Faust because of his insatiable desire for scientific knowledge; but he was more akin to Spinoza in his development. The significant thing about this programme was that in trying to know before loving he made the latter impossible. In his efforts to do so, emotion was largely swallowed up in the interests of intellectual activity. This is the key that unlocks the mystery of da Vinci's hunger for knowledge, and his apparent indifference to the emotional phases of life.

It is possible that he began his investigations in behalf of his own art for the purpose of mastering the laws of light, color, shade and perspective, in order to be true to nature. Then he was forced by the interests of the painter to investigate the objects to be painted: the animals, the plants, the proportions of the human body, the inner structure of all these, and the functions which manifest themselves without and need to be considered by the artist. But his scientific interests, thus begun in behalf of the art of painting, led him far away from the demands of his art, and finally impelled him to abandon it almost entirely in behalf of pure science.

Freud grants that any marked capacity in a character such as the scientific spirit in da Vinci rests on special native endowment. But he holds that such a strong bent of mind has very probably strengthened itself in early childhood through some external influence and that it originally attracted, to its use, energy from the sexual sphere. In this way, it derives its strength partly from the sex field and acts as a substitute for it in later life. Such a person would later take great delight in scientific investigations at the expense of emotional life. This whole theory is based upon the assumption that the energy that is usually spent in the sphere of sex can be sublimated into non-sexual ends. That such contributions are made from the sex realm to other special spheres of activity is shown by daily observation. This process is unquestioned, Freud thinks, if during childhood this overpowering spirit of curiosity served sexual interests and if later, during mature life, there is a strong development of this same spirit of curiosity directed toward scientific ends accompanied by sexual indifference. Objection would generally be made to such a theory on the ground that young children have neither the spirit of investigation nor sexual

interests. But Freud holds that the curiosity of little children is abundantly shown by their endless questioning. He thinks these questions are circumlocutions, and that they have no end since the child wishes to ask one question which it does not state. Psychoanalysis shows that all children, but more especially the bright ones, pass through a period, beginning with about the third year, which might be called the infantile sexual investigation period. This curiosity is usually awaked by some sexual impression, such as the birth of a sister, or brother, by which the egoistic interests are threatened. This curiosity has, for its object, the solution of the problem as to where babies come from, just as if the child would wish to prevent their coming. It has been found that the child does not believe the stork story, and that from this date there arises a mental independence because the child feels himself opposed by adults, and never again pardons them for deceiving him about this matter. The child begins to investigate in its own way, usually guesses the source to be the mother's body; and from its own sexuality it gets impressions that help it work out theories, about the source of babies as a result of eating, of being born through the alimentary tract, and may touch the rôle of the father which appears to it as something malignant and forced. But owing to their own very immaturity, they never reach a satisfactory solution, and finally leave it in this unsolved condition. The failure of the first attempt at independent investigation makes a profound impression, and affects the attitude of mind in all later investigations in a somewhat similar way.

When this period of infantile sexual investigation ceases through sexual repression, there may result any one of three possible conditions which affect the future impulse to investigate, as a consequence of its early connection with sexual interests. In the first type, the spirit of investigation shares the fate of sexuality and is repressed; the desire for knowledge is inhibited, and the free exercise of the intellect remains limited, especially if later, during puberty, the powerful religious thought-inhibition is brought into play. This is the type known as neurotic inhibition.

In the second type, the intellectual development is strong enough to withstand the sexual repression, and is not repressed with it as in the first type. Some time after the disappearance of the infantile sexual investigation and when the intelligence is strengthened, it bids for its return to help evade sex repression. This repressed sexual investigation energy returns from the unconscious as imperative brooding (*Grübelzwang*) somewhat distorted and hampered but sufficiently powerful to sexualize the thinking, and to color the intellectual operation with pleasure and anxiety of the individual's own sexual processes. The investigation is turned towards sexual activity, often almost exclusively; the feeling of success in thought takes the place of actual sexual satisfaction; but the indefinite character of the infantile investigation shows itself in the fact that the brooding never finds its end, and the sought-for intellectual feeling accompanying its solution moves further into the distance.

The third type is the rarest and most completely developed. The sexual repression is present here, too, but it does not succeed in repressing a part of the sex impulses into the unconscious. The Libido withdraws itself from the conditions of repression by being sublimated at once into a thirst for knowledge which strengthens the already strong impulse to investigation. Here intellectual interests are powerfully reinforced by the sublimated energy from the sex realm.

Now, when we consider the fact that in da Vinci the overpowering spirit of investigation appeared in connection with a greatly diminished sex instinct which was limited to ideal homosexuality, it seems very probable that he was an excellent example of this third type. If we knew the details of his early life, it would be possible to decide this point with certainty. But very little is known about this stage of his life. It is known that he was an illegitimate child, that his mother, Catarina by name, was a peasant

girl and that his father, Sir Piero da Vinci, belonged to a considerably higher stratum of society than his mother. When Leonardo was five years of age he went to live with his father who had married another woman. Here he remained until he went away to school.

Though the account of Leonardo's childhood is scant, there is one record of his childhood memories that he himself made, which Freud thinks is very significant to the psychoanalytical expert. It throws a flood of light upon those childhood experiences which were important in determining his later life. It is from this record of his memories of childhood that Freud extracts, by means of the psychoanalytical method, the significant facts in Leonardo's early life.

The reference to this memory of his childhood seems to have been entirely incidental to the theme under discussion. In an article which deals with the flight of the vulture, he turns from the main thought to what appears to be the reason for his great interest in this bird. He says that there comes to his mind what appears to be a very early memory. While he was yet in the cradle a vulture flew down to him, opened his mouth with its tail and pushed its tail against his lips many times. This is a strange and improbable story and could not possibly have been a memory of a real experience. It was a fancy of later life, projected into the time of his infancy. This is frequently the case with so-called memories of childhood. They are not recalled until childhood is past, and then they are modified and falsified in the interests of later tendencies so that there is not much to distinguish them from pure fancies. The individual is, in this respect, much like the race. While the race is young and struggling for existence with all manner of enemies there is no effort to record its history. But later, when it grows powerful, and there is more time for meditation, the desire for a past history arises. Then one employs all available material such as tradition, sayings and proverbs, and weaves them into a story of one's past in which one's present wishes and desires tend to fill in gaps, distorting much of the evidence, and misinterpreting the rest. The result is not history; neither is it pure fancy. It is a combination of the two. To treat it as pure fancy would be to throw away valuable historical material. It is the business of the historian to separate fact from fancy and interpret both. Just so is it with the vulture story. What a person thinks he remembers of his childhood experiences is very significant; as a rule, the most telling evidences of his mental development are hidden in these supposed early memories.

It is just here that the technical skill of the psychoanalyst is needed to follow out and interpret these alleged memories, and discover their hidden meaning. It is in this way that an attempt is made to fill out the gaps in Leonardo's early life through the analysis of the vulture story.

Similar material is found in dreams; and it needs to be treated similarly. The story is symbolic. It is erotic in its meaning and symbolizes *fellatio*. Tail (*coda*) is a symbol of the male sex organ no less in Italian than in other countries. The passive part of the subject in the story is significant. It is similar to the dreams and fancies of homosexuals. The experience which furnishes the material for this is nothing less than the infantile means of securing its food from the mother's breast. This is why the story is projected back into Leonardo's infancy. Back of this fancy is hidden the faint remembrance of his infantile food-getting. In his later life this beautiful scene was often painted in the form of the infant Jesus and his mother. This reminiscence was converted into a passive homosexual fancy. But in the place of his mother was substituted a vulture. How came this to pass?

In the holy picture-writing of the Egyptians, the mother is always represented by the picture of a vulture. The Egyptians also worshipped a goddess called Mut, who was represented by a vulture-headed statue. Perhaps the vulture was a symbol of motherhood because it was supposed

that there were only female birds of this species. The wind performed the male function as they flew through the air. There is good evidence that Leonardo was familiar with the fact that the Egyptians used the vulture as a means to represent motherhood. He was a voluminous and omnivorous reader. Milan was the chief centre of books and libraries. The church fathers often used this story to support the story of the conception of the holy virgin.

Thus the origin of the vulture fancy of Leonardo might be conceived somewhat as follows: As he may at some time have heard the story from a church father, or read from a book of natural science that there were only female vultures and that they reproduced themselves without the assistance of males, there appeared faint echoes of a memory which took the form of the story as he later recorded it, in which he unconsciously identified himself with the vulture's offspring; for he too had a mother but no father, and with it there was associated in a manner as only such old impressions can express themselves an echo of the pleasure which he enjoyed at his mother's breast. Perhaps the idea of the virgin with her son led him to value the fancy more than usual. He, in a measure, identified himself with the Saviour. The substitution of the vulture for the mother indicates that the child, being illegitimate, missed the father. It is known that he spent the few first few years alone with his mother. In this sense he was a vulture child. This is the key to the memory of later years.

In the first few years of a child's life, experiences are so indelibly impressed that they never again lose their meaning and effect in later life. If it is true that the unintelligible childhood memories, and the later fancies built upon these, portray the most important features in the mental unfolding of an individual, then the fact that Leonardo spent the first few years of his life with his mother alone must have had the most profound influence in moulding his inner life. Under the influence of this constellation it could not but happen that the child, who, in his first years, found a problem in addition to that of other children, began to ponder with special interest upon the riddle of the origin of children, and whether or not the father had anything to do with it. He thus early became an investigator. Leonardo himself seems later to have perceived some faint echo of the connection between his childhood struggles and his later investigations, for he is led to remark that it seemed to have been destined for him to investigate the problem of the flight of the vulture since he was visited by one while he was yet in his cradle.

As has been stated before, the vulture story symbolizes Leonardo's homosexual tendencies. But since the vulture was considered the symbol of the female why was she also given male attributes? It is well known that many Egyptian goddesses, as well as those of Greek creation, were composites of both male and female organs; and it is very probable that Leonardo derived this knowledge from books. But how are we to account for the fact that he accepted this notion and incorporated it in his so-called memory of childhood?

To understand this we must consider the infantile sex theories which young children create. There is a time in the child's early life when first he begins to have his curiosity aroused concerning sex matters, when he believes that everybody has organs similar to his own. The male child thinks his own sex organs so interesting and important that he cannot think of any one without one. He has great curiosity to see others. Later, when he discovers that his conclusion was wrong and that the female is different in structure, this curiosity gives way to disgust which at the time of puberty may lead to psychic impotence and permanent homosexuality.

But the intensity with which the child works out this early sex theory leaves permanent traces upon his mind. Certain foot fetishes seem to be the outgrowth of a substitution of the foot, for this much-valued organ. Freud points out that his notion of the child's interest in sex matters will

not receive much credence from those who hold to the modern attitude of minimizing the elements of sex in life, and regarding it with shame and disgust. He thinks that the great sexual interest which children manifest has its analogue in primitive races. He holds that most primitive people followed some form of phallic worship; and that many gods arose from this primitive worship through sublimation to higher non-sexual divinities. The childish assumption that the mother has a male organ similar to his own originated in the same way that the androgynous conception of the goddesses of old originated. The old hermaphroditic goddesses were in reality feminine figures with male sex organs attached just as the child conceives the case to be. In this respect, the child recapitulates the race. Thus the alleged memory of da Vinci concerning the vulture's tail had its origin in his early life when he attributed to his mother a male organ similar to his own. If this interpretation is correct, it furnishes further evidence that his infantile curiosity was unusually active. Freud thinks there was a causal relation between da Vinci's early sex theory and his later homosexual tendency. This causal relation has been discovered many times in the psychoanalysis of homosexual patients. In all these cases, there occurred in early childhood a very intensive erotic attraction towards the mother, due to over-tenderness of the mother, and perhaps strengthened by the absence of the father. Later this attitude toward the mother is repressed. The child identifies himself with the mother, takes his own person as ideal, and chooses an object of love similar to his ideal,—and thus becomes homosexual.

In reality he has gone back to auto-erotism, since the boys to whom he is attracted are only memories of his own childish person which he loves as his mother loved him in his infancy. He finds his object of love as did the Greek Narcissus. Freud thinks that such homosexuals retain in the unconscious a memory of their mother. Through the repression of the love of mother, he conserves it in his unconscious, and remains true to her. If he appears to seek boys upon whom to bestow his love, he is, in reality, running away from those women who might make him untrue. Observation has also shown that those homosexuals who are apparently stimulated only by males are, in reality, moved by the attraction which comes from a woman. But they hasten to transfer the stimulus received from a woman to a man; and therefore rehearse the same psychic mechanism that made them homosexual in the first place. This may only produce one type of homosexuals. Da Vinci's was of this type. Although he succeeded in sublimating most of his sexual tendencies, yet it cannot be assumed that he succeeded absolutely as this can never be attained. Other than mere hints of erotic love must not then be expected in him. These were homosexual. It is well known that he selected only beautiful boys for his school and not promising ones. Other evidences are found in a record of some purchases which he made for some of his students. He kept a note book in which he used signs which only he understood. These records are few, and apparently of little significance; but they mean much to the psychoanalyst.

The record shows a very exact account of a small outlay of money, as if he were keeping the strictest account of household expenses. However the expenditure of larger sums is not recorded, and there is no evidence that he was a strict economist. One of these records is the purchase of a new coat for one of his students; other similar records are found of the purchase of wearing apparel for other students. Most biographers simply regard this event as evidence of his foresight and goodness toward his students. But this does not satisfactorily account for these records; we must look for some affective motive that led him to make them. The cue to this is found in another record, the motive of which is more certain and evident. This other record consists of a statement of the funeral expenses of his mother who died while visiting him at Milan. Da Vinci had succeeded

in forcing his feelings under the yoke of investigation and thus inhibited their free expression. But there were times when his repressed feeling manifested itself by attaching itself to some apparently insignificant object; and the death of his once intensely loved mother was one of these times. In this record we have the distorted expression of grief for the mother. This is not a normal expression of feeling, but in so-called imperative neurosis, this is a common phenomenon. In these cases we see intense feeling, which has become unconscious through repression, attaching itself to some trifling matter. The repressive factors have succeeded in diminishing the expression of this feeling to such a degree that its intensity would never be guessed were it not for certain evidences of an inner demand that the apparently insignificant feeling be expressed. The recording of the funeral expenses of his mother is just such a case of the disguised expression of a strong though unconscious feeling towards his mother. The strong repressive factors of his later life which repressed the infantile feeling would not allow a more worthy memorial to be made; so there was a compromise in the form of the record of funeral expenses. The same affective motive was at the basis of the record of expenses for his students.

The vulture story has still other significance. The expression that the tail pressed against his lips many times suggests the intensely erotic relation between mother and child. It is not far-fetched to assume that the mother planted numerous kisses upon his mouth. Thus the vulture fancy is a synthesis from the memory of nursing at his mother's breast, and being fondled and kissed over-much by her. The artist succeeded in unconsciously expressing in his artistic work the elements that were perhaps his strongest mental stimuli during early childhood. These elements are contained in the remarkable, fascinating and enigmatical smile which he has placed upon the face of his feminine characters in his paintings. It is strictly characteristic of his work; and has been called *Leonardesque*. In the strangely beautiful countenance of *Monna Lisa* it has affected visitors most strikingly. Much has been written in explanation of it, and the most varied interpretations given. He worked upon this portrait four years, and left it unfinished. After this painting all his later feminine characters wore this smile. The smile fascinated him no less than it has those who contemplated it during the past four hundred years. Since he first gave expression to this smile while painting a portrait, many critics have assumed that this model must have possessed the smile. Freud believes this is not the true explanation, and that she only awakened a memory that had long slumbered in the unconscious. Its arousal so fascinated him that he never again could free himself from its influence. It was nothing less than the smile of his own mother, which he had forgotten, but which was revived by the model. Chronologically, the next painting was the holy *Anna with Mary and the boy Jesus*. Both these feminine characters have the *Leonardesque* smile. This painting is a synthesis of his childhood experiences. When he was five years of age he went to live with his father; here he not only found another mother, but also a grandmother who was very attentive to him. This suggested to him the mother and the grandmother idea. But, in the picture, the grandmother is yet young and with unfaded beauty. In reality, the boy has been given two mothers, one reaching for him and the other in the back-ground. This exactly embodies Leonardo's own childhood, for he had two mothers,—his real mother a little older than his stepmother and a little farther away from him, just as the grandmother is represented in the painting.

Another entry is found in the notebook concerning the death of his father, which only the psychoanalyst can interpret. The error is a repetition of the time of day when his father died. Ordinarily this might be considered as a matter of inattention; but such is not the case. Such a repetition is called *perseveration*. It is a key which shows an affective coloring as a result of the momentary suspension of inhibition in which

the strong, suppressed feeling attaches itself to an unimportant matter. His father was a man of great strength, and became an important factor in the psychosexual development of Leonardo, not only in a negative way through his early absence, but, positively, in his later childhood.

Whoever as a child was attracted to his mother cannot but want to place himself in his father's stead, and identify himself with him in his fancy, and so lays the foundation for a later attempt to accomplish the conquests which his father made. The father was a gentleman; and Leonardo tried to be like him in this respect, although his means would not always allow him to do so. Since an artist has the attitude of a father towards his productions, da Vinci identified himself with his father here also, because he was indifferent to the children of his brush just as his father had been indifferent toward him.

But if his imitation of his father injured his artistic success, his father's neglect made him a great scientist. The keenness and independence of his later scientific investigations were due, in a measure, to his early sexual investigation caused by his father's absence. It has been found by psychoanalysts that the idea of and the belief in God is closely related to the father complex; and that the personal God is psychologically nothing else than an enlarged idea of father. The idea of nature is the embodiment of the mother complex. Thus God and nature are grand sublimations of the father and mother complexes. Da Vinci illustrates this tendency very well. He was entirely free from religious dogma; and he worshipped nature. His close study of natural phenomena enabled him to guess some of the most fundamental of later scientific discoveries. His was not a personal religion, but a natural one. External authority in matters of religion had no significance for him.

One great ambition of da Vinci's whole life was to build and to operate a flying machine. He seems to have desired to do in this world what most people of his time hoped to accomplish in the next. Why this interest in flying? Psychoanalysts have found that this wish is only a thinly veiled means of expressing another wish. The stork story, the winged phallus of old suggest the meaning of the wish. The most frequently used expression for the sexual act is called *vögeln*. By Italians, the male organ is termed *vogel*. (*uccello*). All of this suggests that the desire to fly does not mean anything other than the desire to be able to carry on sexual activities. This is an early infantile desire. It has been thought that children are satisfied by the incidents of each moment. But the desire to be large like adults ever haunts them, and determines most of their plays. If they learn that mature people can do something in the sexual sphere, which they must wait to do, they are consumed by the desire to do likewise, and dream of it in the form of flying. Thus modern aviation has its infantile erotic roots. In da Vinci's case both the suppressed wish and the symbolical embodiment were doomed to failure.

This symbolism is scarcely intelligible unless one has followed the extensive literature on this subject, which has recently received so much attention from the Freudians.

2. PFISTER, OSKAR. *Die Frömmigkeit des Grafen Ludwig von Zinzendorf. Ein psychoanalytischer Beitrag zur Kenntnis der religiösen Sublimierungsprozesse und zur Erklärung des Pietismus*. Zurich, 1910, 118 pp.

This psychoanalysis of the life of Count Zinzendorf shows, in yet another instance, how closely sex and religion are united, and how inextricably they become intertwined in the same individual. His whole religious life and piety were dominated by his erotic life. His feeling towards Jesus was plainly of a homosexual nature. God and the Holy Ghost, the other two elements in the Trinity, were almost crowded off the stage,—so completely did Jesus receive the religious devotion of this man.

Two factors were, in a measure, responsible for this strange co-mingling of sex and religion in the life of Zinzendorf. The first of these was the spirit of the times, which looked upon all pleasures as the work of the devil. All Christians were called upon to wage warfare against this foe of man and God. The "lusts of the flesh" were preeminently the most difficult to conquer, and therefore received most attention. This severe repression often led to surrogates where they were least expected. In the case of Zinzendorf Jesus became this surrogate unconsciously.

The second factor was the early impressions received by Zinzendorf from his mother and teachers. He was born in a rigidly pietistic family. Spener, the father of Pietism, laid hands on him at four years of age for the kingdom of Christ. His father, being tubercular, found compensation, in the world of belief, for his earthly suffering; but he died a few months after the count's birth. The latter was profoundly impressed by the story of his father's love of Jesus as told him, over and over again, by his mother. Her fondest hope was to have her son become a devoted follower of the martyred Jesus; and to this end she detailed with the greatest minuteness his suffering and crucifixion. She dominated her son's early life to such an extent that he always considered himself subject to his mother. This attitude of the son towards the mother was erotic at first, but later Jesus became the substitute. From early infancy he was refused worldly pleasures. He could not and dared not be a child. Association with other children was forbidden. Prayer to Jesus was the only form of pastime which he was allowed to indulge in without restraint. Jesus thus became a substitute for friends, companions, brothers, mother, and father. At four years of age, he already learned that Jesus was our brother, that he had died for us. He was deeply affected by this. The songs of Jesus' martyrdom pleased him early in life. Before six years of age he decided to live for Jesus who had died for him; and at seven he had his first feeling of how the wounds of Jesus felt, and he shed tears over it. He wrote a letter to Jesus which he threw out of the window. The seeds of his later sadistic and masochistic tendencies were here sown. He later preached what he felt in these early years. Even his closest friends thought he went too far in his love for Jesus. Their criticism, however, made him feel happy because he was suffering for Jesus' sake. From the time he was eight years old he never allowed himself to forget for a moment the wounds of Jesus. He early developed a condition of anxiety which was plainly a result of sex repression.

At ten years of age he went to live with Francke, the great teacher at Halle. When his mother delivered him to his teacher, she reminded him that her son had shown unmistakable signs of pride, which must be crushed. This was successfully accomplished. In all this difficulty Jesus was the youth's only friend and guide; he acquired a real hunger for suffering. At thirteen years of age, he wrote to Jesus thus: "Receive us into thy wounded sides; from there we will fight the evil and conquer." At sixteen he wrote that the devil could not harm him while he rested body and soul in the wounds of Jesus. As a boy at school, he founded organizations and prayer meetings. In all of these the suffering, wounds, and death of Jesus were the only themes. The Lord's Supper with its revival of the memory of Jesus' suffering would almost put him into an ecstasy.

At the University of Wittenberg, his pietistic tendencies were strengthened rather than dampened. Believing that his nature was essentially bad, he became an ascetic; he prayed whole nights, and read the Bible. When 19 years old he said that if he could die he would look upon it as a wedding joy. He wanted to come nearer to Jesus.

He felt it his duty to marry but could not persuade himself to love a woman for fear of doing an injustice to Jesus. Four times he was about to marry, but each time decided that his fiancée suited some one else better, and surrendered her. Finally, however, he was persuaded by friends that

a marriage would not interfere with his duty to Jesus. He married and had twelve children, four of whom lived. He never cultivated domestic happiness, and never manifested more than respect for his wife. He entered the service of the state with tears because he felt this would do Jesus little honor.

His homosexual attitude towards Jesus was manifested throughout his life by the terms which he employed. He referred to Jesus as the bridegroom of his soul; he prayed that strange love might be extinguished from his soul and that he might be allowed to win his Saviour's love. He preferred to consider his soul as the bride of Jesus, and used the most extravagant terms in his praise of the bridegroom. He said it was Jesus' own business if he kissed us after he had forgiven our sins. He also talked of the embrace of Jesus. The manner by which Elisha called back to life the woman's son who died had great fascination for him. He declared that Jesus forgave sins in this identical way, and that the thrill which was felt throughout the body and soul when this took place could only be compared to the feeling of a wife when loved by her husband.

At the age of forty there seems to have been a new outbreak of his repressed homosexual, sadistic and masochistic tendencies. The author thinks this was due to the fact that the repression of his sex impulses was more severely complete than before and therefore needed other means of expression. This led to a polymorphous perverse expression. At this time he also came to rely more fully upon himself; and his authority in matters of religious experience became greater. He trusted his own fancies and sub-conscious manifestations more and more. As a result his natural inclinations were given full sway and his unconscious impulses had full expression.

Aside from the unmistakable homosexual manifestations, which now became more outspoken, other perverse expressions were the following: the tendency to necrophilism was clearly shown by his emotional excitement in contemplating the dead body of Jesus, in partaking of the Lord's Supper, and in advising the wife to place her arms about her dead husband's neck for a stated period. The tendency to sadism is shown by his outspoken pleasure in contemplating the wounds of Jesus. He prepared a wound litany which the author pronounces as monstrous. Among other things which are addressed in this litany are the scratches made by the crown of thorns; the mouth with saliva dripping from it; the cheeks which were spat upon; the exhausted eyes; the bloody foam; the sweat-covered hair, and finally the wounds. The wounds became the only and highest good towards which his whole life turned. The blood of Jesus, too, became a fetish with the most extravagant sentiment woven about it. Its appeal to his sense of taste, smell and sight is always evident. The sweat of Jesus, too, made a peculiar appeal which could only feed his sadistic tendency. The wounds in Jesus' sides were of greatest interest. They were called feminine genitals, organs of birth, and sources of greatest pleasure. The author quotes endless passages to show that the count's thought and feelings were strongly colored by his erotic life, when contemplating the details of the crucifixion.

Zinzendorf introduced, as church ceremonies, footwashing, the brotherly kiss, the night-watch, and the love-feast,—all of which the author thinks sprang from his erotic needs. The same was true of his celebration of other religious ceremonies, such as baptism, the Lord's Supper, confirmation, funeral ceremonies, ascetic practices, mission-work, training of children, and his founding of the United Brethren organization.

In all this manifestation of his erotic life there was left no room for the ethical teachings of Jesus. They made no appeal to him because Jesus, as the object of his erotic life, excluded all else.

Although there may have been a natural predisposition to begin with, it is nevertheless true that in much of Count Zinzendorf's later life we see

the direct influence of his early impressions and teachings. They laid well the foundation for just such a career as he led. The count perhaps never suspected the nature of his piety, but recent studies in sexual abnormalities show very clearly that his attitude towards Jesus could be almost exactly duplicated in the attitude of perverts towards the object of their sexual desire.

The effort of his elders to have him concentrate his early attention upon the martyred Jesus paved the way for his later libidinous attitude. The fact that he did not know his father in childhood, except as his mother informed him of his love for Jesus, accounts for the fact that he had no place for God in his later religion. The great emphasis which his mother, early in his life, placed upon Jesus, to the exclusion of relatives, friends and companions, accounts in a large measure for the count's later lack of interest in the social message of Jesus. The tears of joy of the seven-year-old over the bloody Jesus on the cross may well have laid the foundation for his adult sadistic tendency. His desire, at the beginning of puberty, to be taken up into the wounds of Jesus later found satisfaction in his cult involving the wounded sides of Jesus.

The author gives a wealth of quotations from Zinzendorf's works to substantiate every point he makes; but only the merest outline could here be given. This psychoanalysis adds another valuable chapter to the now rapidly growing literature which shows how large a part sex plays in human life, and often in ways not usually suspected, so plastic is the sex instinct.

3. GRAF, MAX. *Richard Wagner im "Fliegenden Holländer."* Ein Beitrag zur Psychologie des künstlerischen Schaffens. Leipzig, 1911, 46 pp.

An effort is here made to explain the life and works of Richard Wagner by showing how his early experiences influenced his later conduct, but more especially his musical and dramatic productions. These early experiences are conceived as resulting in dynamic factors which gave color and direction to his whole later life.

The Flying Dutchman more than any of his other productions gives this key to Wagner's personality and psychic peculiarities. It is the one production in which the author reflects his life history, his inner conflicts, his unconscious longings and wishes. When, as a young man, he first heard the story or legend as told by the author, Heine, it fascinated him because it contained something akin to his own inner struggles. Wagner's sea-voyage to London and Paris, and his high hopes and crushing defeats at the latter city, all tended to deepen his feeling of identity with the legendary Flying Dutchman, and prepared him for the production of the opera by that name. After he had written out a rough sketch of this opera, he versified it in ten days and set it to music in seven weeks,—so completely had both the music and verse taken form before he began to write it out.

He found it necessary to modify completely the *motif* of the romantic operas of his time, in order to express himself. Before his modifications were introduced, the three main characters in the opera were as follows:

A virtuous young woman and a worthy young man love each other devotedly. A demon with supernatural powers appears, and carries away the maiden into captivity. But the power of the demon is only temporary, because virtue must triumph in the end; and so the maiden is subsequently recovered by her lover.

Wagner retained the three characters but their relations are entirely changed. Two persons are in love, as in the previous operas, and an unlucky, demon-like man approaches as before; but now the maiden suddenly experiences a complete change of heart, and receives him with open arms because she dreamed of his coming and his presence wakes her slumbering love for him. By her devotion to the new-comer, she saves him from some

terrible doom. She willingly offers her life as a sacrifice to show her fidelity; and by this act the spell, which held the unfortunate man captive, is broken and his struggles are over.

This is the central *motif* of practically all of Wagner's works. This one theme seems to have occupied him throughout his life. He could never get away from it. He found it necessary to change somewhat the story of the Flying Dutchman in order to express his own life by introducing the character Eric, as the lover of his heroine, before the main character appeared. But into the character of the Flying Dutchman himself, he seems to have projected a complete embodiment of his own sufferings; and in Senta he found his ideal of woman as she appeared in his dreams and visions, when his innermost feelings determined their character. In fact, the women of his artistic creation are all of one type. All have longings for something more or less fantastic, something not to be found in their immediate environment.

Thus in all of Wagner's productions, but more especially in the one here under consideration, there seem to be evidences of the fact that his soul experienced something which pressed for expression, either in imagination or in life; a phantom, an idea, a dream fancy, an intensive wish; longed for fulfillment if only in fancy. Odysseus and Columbus both made a strong appeal to him; but neither quite embodied his case and it was only when he heard the story of the Flying Dutchman that he saw the reflection of his own life. The fact that the hero could be saved only by the sacrifice of a true woman seems to have been the feature of the story which touched Wagner. At the age of twenty-three he was rather hastily married, but after seven months his wife left him; and although there was later a reconciliation it never completely effaced the disharmony. They were separated by a fancy or dream of his, in which he embodied the form of an ideal, self-sacrificing, insightful woman, who would be faithful unto death, and whose love would soften his troubles.

Why this conflict in Richard Wagner's life and in his artistic productions? The answer to this question is found in his early childhood experiences. When he was six months old his father died. After six months of widowhood, his mother married the painter and actor, Ludwig Geyer, who had befriended the family after the death of her first husband. Geyer seems to have made a profound impression upon young Wagner; for the latter as a boy was fond of the belief that he might have been the son of Geyer. This belief is not uncommon among boys. The wish is father to the thought that they might be the son of some other more famous man, king or even God, than their legal father. Goethe and Beethoven entertained this idea in their youth. The same notion is also found in myth and poetry. It gives the son a chance to choose his father. As a boy, Wagner seems to have toyed with this idea that Geyer was his father. But the very satisfaction which he derived from this possibility so impressed the youth that he came to act as if the assumption were true. Even in his later life, this attitude toward Geyer was not abandoned. He always liked to have Geyer's portrait with him; he adorned his step-father's grave; and referred to him often in letters, but never to his real father. He even dressed himself as Geyer did, and wore the latter's cap and gown. What is the motive of this fancy of Wagner's youth, and that of other boys? The Oedipus complex is at the basis of this fancy. The child's attitude towards the mother is more or less erotic, and is due to over-fondness of the mother. This stimulates a rivalry with the father, whom the son would like to equal, but to whom he is subordinate. Thus the father is the rival and yet the ideal of the son. Although the fancy of having a different father involves the infidelity of the mother, it is by this means that the rival father is set aside. At the same time, the ideal qualities of the father as conceived by the son are given to the God, king, or prince, whom his fancy chooses as his father. The conflict between the love of the father

and the rivalry with him is settled by setting aside the rival father, and at the same time giving his qualities to a new father.

This attitude of the son towards the mother may easily give rise to fancies in which the infidelity of the mother plays a large rôle. The ambition of the little Œdipus to equal his father extends to the latter's relation with his mother. In normal children, and under normal conditions, this attitude towards the mother is repressed, and dies out entirely. But if there is a natural precocity, or if the mother is unduly tender with the child, and caresses him too much, the erotic impulses may become so strong that the fancies of rivalry and infidelity become so established that they can never be wholly overcome. Repression and suppression may remove them from consciousness, but they enter the unconscious realm as active forces, and constantly exert a determining influence on conduct.

This is just what seems to have been the case with Richard Wagner. His father having died when he was six months of age, his mother lavished her affection upon the little son to drown her grief; and from this he never recovered. As a man, he was always hungering for love and honor. No sacrifice of friends was too great, and no honor and praise from women admirers was ever sufficient to satisfy him. His memory of his childhood was unusually keen; and his childhood characteristics were remarkably well retained. His hate and his love, his suffering and his ecstasy were all very childlike.

Thus the *motif* of the Flying Dutchman, and of some of the other of Wagner's greatest works, springs from his childhood experiences and fancies. The overfondness of the mother made such an impression upon the child's mind that he was never afterwards entirely free from her personality and characteristics. His heroines all embody the idealized qualities which he conceived his mother to have possessed. When the Flying Dutchman first saw Senta he said: "How strangely this maiden standing before me seems to arise from long past memories!" This shows how closely his mother's memory entered into his heroines. His fondness for the fancy that Geyer might be his father sprang from the childish wish that his mother had been unfaithful to his father for his sake. He identified himself with Geyer, and remembered the latter because he attributed to him what was in his own early childish fancy. This attitude of rivalry left such a strong impression upon his psychic life that his whole career was colored by it. It is just this relation of rivalry that is manifested by nearly all of his heroes. They come, as a third party, into the relations of two lovers, and successfully compete for the maiden's love as did the Flying Dutchman. In the original story, this rivalry was absent; but Wagner created the character Eric as Senta's lover, prior to the arrival of the Flying Dutchman, in order to embody the demands of his life and to fulfill his own wishes. He looked upon his artistic productions as a means of realizing his unfulfilled wishes, in actual life. He said that if life itself were lived in its completeness there were no need of art.

Although Wagner himself never guessed the source of these unfulfilled wishes, psychoanalysis seems to make it clear that they sprang from his childhood impressions; and that they were revived in his later life and fulfilled in the heroes of his creation. They embodied what had always been to him a fancy and a dream; and the Flying Dutchman was *par excellence* the expression of this dream.

An effort has been made to give a somewhat condensed *résumé* of the main facts and principles involved in each of foregoing articles. These attempts at an intensive study of individual characters and personalities are so thoroughly of the nature of pioneer work, and go so far beyond the accepted standards of orthodox psychology in many of their explorations, that it is difficult to attempt an evaluation. Perhaps it is not yet time

to become too critical. It is in their effort to get at the dynamic factors of psychic life that these studies are of immense importance. Whether one accepts all their conclusions or not, they are still highly suggestive, if not illuminating; and it does not seem too much to say that they are prophetic of what the psychology of the future will largely concern itself with. The emphasis placed on the evolutionary and genetic aspects of psychic functioning cannot but meet the approval of all thorough-going evolutionists. The effort to trace out the modification and sublimation of the basic racial impulses into ever higher and more complex forms of which all civilization is the expression is a stupendous programme,—but a most fruitful one. One cannot but feel that these men are grappling with the vital factors of mental life; and that, although their methods and conclusions at times seem somewhat crude, they are opening rich mines of information concerning psychic life, which are destined to make much of the present introspective, laboratory psychology look pale and frothy if it is not undermined entirely. The evidence which shows that consciousness is not to be trusted in attempting to explain its own motives has already grown overwhelming. The tremendous part played by unconscious complexes, in the mental life of every normal person, cannot longer be doubted. In the light of these facts it seems a little belated to continue the discussion as to whether or not there is imageless thought.

It seems important, therefore, that this school of psychologists should be given a full and free hearing. Nothing so tests one's open-mindedness, and one's desire for more light, than such departures from the accepted standards as the above studies are. The summary dismissal of the contributions of the Freudian School as unscientific and without a basis of fact, as some have a tendency to do, is simply a reflection of their own inability to weigh principles in an unbiased fashion. Would it not be better to adopt the same attitude towards these new contributions to psychology that some denominations do towards repentant sinners; and put them on probation for, say, a period of from five to ten years?

Coming now to closer quarters with these studies, an effort will be made to point out a few of the common principles running through all of them.

Great emphasis is placed on sex in each of the studies, which is in harmony with one of the fundamental principles of the Freudian school. This, in itself, is sufficient, in the eyes of many, to condemn it; although it is safe to assume that not very many of those who criticise have clearly in mind Freud's use of the term. He uses it in a very much larger sense than current usage would warrant. He assumes that all evolutionary variations and sublimations of the primitive impulse to procreate, in the lower forms of life, can rightfully be designated sexual. In this he is justified by Darwin's use of the term, as well as by all modern scientific students of the subject. He accepts the dictum of the poet that hunger and love rule the world; and he uses the terms sex and love synonymously.

The contention that the so-called partial sexual impulses manifest themselves early in the infant's life, and virtually determine its later career, is difficult to believe. While the above studies consider only the facts as they actually occurred, and do not speculate as to what might have happened if conditions had been different, yet it does not seem to do violence to the spirit of these studies to say that, on their contention of the great influence of the first years of infant life on the later career, Leonardo da Vinci's life would have been completely changed if he had enjoyed the presence of his father during the first four years of his life; and that Wagner received his lasting impressions by the time he was a year old.

In the case of Zinzendorf, the evidence of the effect of the early impressions upon his later sex life is more convincing. The repression of practically every form of play activity, and every other means of outlet for energy, and the increasing emphasis upon the crucifixion of Jesus, with its tendency to arouse strong emotions, were well fitted to give direction to his entire emo-

tional life. The religious care with which his mother fostered every incipient emotional attitude towards Jesus, even in his earliest years, could not have been better planned to bring about his adult attitude.

The testimony which the Freudians marshal in support of the contention that infancy and childhood give evidence of sex manifestations is being extended, from time to time, and is growing convincing. It is safe to say that several chapters are yet to be written on the early sex life of children, of which Freud has at least written the headings. That over-much fondling, petting, and kissing tend to sensitize an infant to this form of treatment, and make it sexually precocious, is now generally granted by all close students of this phase of infant life. That this should leave permanent traces in the unconscious is easily possible. The following up of early impressions, and the tracing of their influence upon later life is a challenging problem, but one not yet fully developed. Freud has here given direction to a line of investigation that is sure to yield an abundant harvest.

So little is known about Leonardo da Vinci's early life that it seems a pity that some other more familiar character was not chosen in preference to him; and yet when one reads this classic analysis by that prince of psychoanalysts, one almost feels that, if more facts had been at hand, his almost magic subtlety of analysis would have had less opportunity to reveal its power and penetration. It is to be hoped that Freud will, in the near future, psychoanalyze Goethe, Napoleon, Alexander the Great, or some other great man, concerning whose life more facts are at hand.

One great contribution of the Freudian investigations to the knowledge of sex is the demonstration that the energy expended in the satisfaction of the sex impulse may be sublimated to higher mental activities. This principle is illustrated most fully by the life of Leonardo da Vinci. In the case of Wagner, it was not nearly so complete; while Count von Zinzendorf offers a perhaps unique illustration of the pathological possibilities in this respect. Instead of sublimating his sex impulse, he directed it towards the physical Jesus almost *in toto*. It was simply the substitution of one sex object for another, and the transfer of physical satisfaction to a satisfaction due to the active use of the imagination.

This sublimation of sex energy into higher mental powers and capacities is assumed by the Freudians to have been the very means of establishing civilization. It was the long-circuiting of the sex impulse that produced art, religion, poetry and scientific achievements. It is when sublimation does not take place, and there is a successful effort to suppress the normal physical expression of the sex impulse, that pathological mental symptoms may begin to manifest themselves.

This whole problem of the sublimation of energy usually expended in the sexual sphere to higher ends is of immense practical, as well as of moral and hygienic, importance. The scientific study of this phase of life cannot be too strongly commended. What the possibilities and limits of sublimation are, is, of course, not yet clear; but here again the Freudians have begun a line of investigation that promises to give a scientific basis for dealing with this most perplexing and far-reaching of human problems. It might be said, in passing, that it is this inclusive conception of sexuality that must be adopted, if one is to follow the sublimation theory as worked out by the Freudians.

The large place given to the unconscious, in these studies, seems to the writer to be wholly justified, even if one is unable to accept all the complicated mechanism and symbolism attributed to it. The Leonardesque smile is most effectively accounted for from this point of view. The same might be said about the peculiar characteristic of Wagner's musical and dramatic productions, with their triangular complications, of which the Flying Dutchman is perhaps the best type. In the case of Zinzendorf the evidence of the effect of the early impressions upon his later life, through

the mechanism of the unconscious, is so unmistakable that few are likely to question it.

Another prominent characteristic of the Freudian theory of the unconscious is that there is a positive tendency to suppress those elements of childhood experience which do not conform to the moral standards of adults, and that this suppression forces these memories into the unconscious, where they have a positive influence in directing conduct. This is so thoroughly established by the clinical experience of the psychoanalysts that it is beyond the realm of controversy. And yet this point is frequently objected to on the ground that the objecter does not find it so in his own case. This kind of argument is about as effective as was that which attempted to refute Berkeley's idealism by striking the earth with a cane. If a thing is suppressed and forgotten the person who forgot it is certainly not in a position to argue whether or not it has been forgotten. That the child, in each of the above cases, should have suppressed his erotic attitude towards his mother cannot be doubted in the light of the mass of clinical evidence adduced by the psychoanalysts. Here, in the unconscious, it still exerted a great influence upon the adult mind in all of these characters.

4. FREUD, S. *Die zukünftigen Chancen der psychoanalytischen Therapie*. Zentralblatt für Psychoanalyse, 1910. I. Jahrgang, Heft ½. pp. 1-9.

Freud wisely admits that psychoanalytical therapy has not yet completely won its battle, much as it has already accomplished in the treatment of nervous diseases. He believes, however, that this method of treatment has a bright future and he enumerates the sources from which it will derive more strength as time goes on.

The first source from which strength will come will be a better understanding of the mechanism of the unconscious. This is necessary to a correct diagnosis. Advances may be expected along the lines of a proper interpretation of the symbolism of dreams, and of the unconscious. The symbolism of dreams is a rich field, and needs yet to be fully developed and explained.

Another source of strength will be a more thorough mastery of the technique of psychoanalysis. Two problems are involved here: the lessening of the labors of the physician, and the discovery of a direct avenue to the unconscious. Considerable change in the technique has developed since the beginning of this method of treatment. Attention was at first directed to an explanation of the symptoms; then to the discovery of the complexes; and now attention is given to the forces of opposition. In order to be successful in the technique, the physician must have examined his own psychic life sufficiently to recognize symptoms in the patient.

An inevitable increase in authority and in prestige will constitute a third source of strength. Heretofore, authority with its powerful ally, suggestion, has been against the psychoanalyst. The very truths which psychoanalysis discovers tend to be used as weapons against it. But truth must ultimately prevail and Freud has faith that it will be so in the case of psychoanalysis.

A fourth source of strength will come when the knowledge of the nature of these psychoneuroses becomes generally known. These psychoneuroses are due to the disguised compensatory satisfaction of an impulse, whose existence is denied by the patient himself. Its very success depends upon this distorted and unrecognized process. When the symptoms of these neuroses become generally known, and the patient knows that his ailment is generally understood, he will try to conceal this symptom and this concealment will effect a cure. At one time, peasant maidens were frequently afflicted with the delusion of being the holy virgin, for it received some credence among the people. But now when such cases occur, people feel that the girl is in need of medical treatment, and consequently such delusions are rare. Just so will it be with the psychoneuroses.

Freud gives a word of warning against the invariable employment of therapeutic and hygienic measures in all cases of psychoneuroses. He thinks the psychoneurosis may at times be the mildest and best outlet of an impulse that would lead to something worse if this means of expression were cut off.

5. FREUD, S. *Ueber 'wilde' Psychoanalyse*. Zentralblatt für Psychoanalyse. 1910. I Jahrgang, Heft 3. pp. 91-95.

This article is a protest against the use of psychoanalysis as a therapeutic measure by those who show, by their application of it, that they have mastered neither its scientific principles nor its technical details. The paper was inspired by the complaint of a patient, who stated that she had been given advice by a young physician which it was impossible to follow; and that her feeling of anxiety became more intense after consulting this physician. He had told her that her condition was due to unsatisfied sexual needs, and that she should return to her divorced husband or secure a lover.

Freud laments the fact that any one should do such violence to the principles of psychoanalysis, when a study of the literature of the subject would prevent any such unpardonable misapplication of its principles and technique.

The first blunder which this physician made was to narrow the term sexual life to the merely somatic phases of the term, whereas psychoanalysis uses the term in a very much more inclusive way. This is justified from the genetic point of view. All of the tender emotions are considered to be a part of the sexual life which had their source in the primitive sexual impulse, even though they inhibit the original sexual end or transform it to non-sexual ends. Psycho-sexuality is preferred by psychoanalysts, because it gives proper emphasis to the psychic factors. It is almost synonymous with the term love. The author points out that there are cases which show every indication of a lack of mental satisfaction, with all its consequences, accompanying no lack of sexual indulgence in the somatic sense of the term. These unsatisfied sexual strivings, which often create a sort of substitute satisfaction that shows itself in nervous symptoms, are helped very little by sexual indulgence. Freud emphasizes the fact that those who limit the term sexuality to the merely somatic factors have no right to apply the principles of psychoanalysis as therapeutic measures. It deals with the etiological significance of sex, and must include all factors however remote they may have come to be, through individual and racial sublimation.

A second misconception of the above mentioned young physician was the contention that want of sexual satisfaction is the cause of nervous disorders. It is not the lack of satisfaction, but the conflict between the libidinous impulse and the effort to suppress it that causes the trouble. Another error is to assume that all symptoms that indicate anxiety are due to anxiety neuroses, and can be cured by somatic therapeutics. It is necessary to know the symptoms which indicate anxiety neurosis, so as to distinguish this form of nervous disorder from other pathological conditions with anxiety as a symptom. No adequate therapeutic measures can be applied without a clear grasp of this distinction, because their etiology is different in each case, and the treatment must likewise be different.

The assumption that mere lack of knowledge of the cause of the symptoms does the injury, and that this information given to the patient can effect a cure is as foolish, says Freud, as to assume that the menu card can satisfy the appetite. It is not the ignorance, but the opposition which causes this ignorance by suppressing and repressing the knowledge of the facts, that produces the psychic disorder. It is the problem of therapeutics to conquer this opposition, and to bring to the surface the facts in the case.

Mere telling would not suffice. The physician must prepare the patient for the information, and must, at the same time, secure the patient's confidence so that when the true state of affairs begins to dawn upon the patient's consciousness, he will believe it and trust the physician. This takes infinite tact and patience; and it is difficult to acquire the requisite technique. To avoid responsibility for the universal application of psychoanalysis, the leaders in this field have effected an international organization whose membership is limited to practitioners who are competent to apply psychoanalytical principles. In this way, it is hoped that the friends of psychoanalysis will be protected from the blunders of those who would apply it without a mastery of its fundamental principles.

6. FREUD, S. *Die psychogene Sehstörung in psychoanalytischer Auffassung*. Ärztliche Fortbildung. Jahrgang 1910, Nr. 9. pp. 1-7.

Freud is not satisfied with the explanation of the psychogenic visual disturbances which is offered by the French school, of which Janet is the chief exponent; and he offers a theory of his own, which he believes comes much nearer to the facts. All psychopathologists have come to recognize the unconscious as an ever present phenomenon in cases of hysteria. For example, in the hysterically blind certain visual stimuli will awaken strong emotions, even though the patient declares he sees nothing. These people are blind only for consciousness. For the unconscious they can see. It is such phenomena as these that force us to recognize a distinction between the conscious and the unconscious.

Why this conscious blindness and the unconscious ability to see? The French school answers with the statement that there is a tendency to dissociation. Perhaps the idea of being blind acts as an auto-suggestion; and the actual state of blindness follows. In this way, many unconscious processes become separated from conscious processes. In all of this, there is an innate, dispositional inability to synthesize experiences, due perhaps to native weakness.

Freud holds that this is only substituting one riddle for another. He points out that it is difficult to harmonize the following phases of Janet's theory; the rise of an idea that acts as an auto-suggestion; his discrimination between conscious and unconscious mental processes; and the assumption that there is a mental tendency to dissociation. All of these are used by the French school in their effort to explain these cases.

Psychoanalysis offers a more satisfactory explanation. It accepts the ideas of the unconscious and of dissociation; but it considers them in a different relation. It considers the psychic life as made up of dynamic factors which enforce or inhibit one another. If a group of ideas is in the unconscious, it does not assume a constitutional inability to synthesize the various psychic elements as the basis of this dissociation. It considers this group of ideas in the unconscious as having come in conflict with another group of ideas, and as having been repressed by them into the unconscious. It assumes that such repressions play an extraordinarily important rôle in our mental life, and that disturbances may often arise as a result of an unsuccessful effort to repress ideas. This gives rise to the symptoms of hysteria.

When in psychogenic disturbances of sight, certain ideas connected with sight are shut out from consciousness, psychoanalysis assumes that these ideas came into opposition with other stronger ideas which forced them into the unconscious by an act of repression. This latter group of ideas may be termed the self group. Why this conflict between groups of ideas? Here we must consider the significance of impulses for the rise and decline of ideas. Every impulse tends to arouse and appropriate to its use all those ideas which serve its ends. These impulses do not always have the same ends; and conflict of interests is common. The conflict of ideas, therefore, rests upon a conflict of impulses. There

is an undoubted conflict between those impulses which have sexual pleasures for their object and those others which tend to the preservation of the individual. These latter might be called the self-preservation impulses; and they might correspond to the group of ideas which were mentioned above, and which are known as the self group. Freud accepts the words of the poet that hunger and love rule the world; and holds that all organic impulses which manifest themselves in the psychic life of the individual could be classified under the terms hunger and love.

The sexual impulse has been followed from its first manifestations in childhood to its mature development; and it has been found to be made up of a number of partial impulses, which arise from the stimulation of various parts of the body. It has also been found that these isolated impulses must undergo a complex development before they can be brought to serve their final purpose of procreation. The application of psychology to the study of cultural development shows that culture arises by means of the sublimation, inhibition and repression of these isolated or partial impulses. All disorders, known as neuroses, are traceable to the miscarriage of these attempted transformations of the partial sexual impulses. The impulse to self-preservation feels that it is threatened by the demands of the sexual impulse, and protects itself through repressions, which do not always have the desired result. These repressed impulses may establish a substitute as a means of satisfaction; and they will thus have an injurious effect upon the mental integrity of the individual. In this way the symptoms known as neuroses are built up.

From this point of view the neuroses are brought into vital relation with the whole psychic life. Returning now to the special problem under consideration, it must be granted that all organs and systems of the body may serve both the sexual impulse, and the impulse to self-preservation. Sexual pleasure is not limited to the genitals. The mouth serves for kissing, as well as for eating. The eyes not only observe what is necessary for the preservation of life, but also those features of an object that make it an object of love. It is not easy to serve two masters. The more such an organ with a double function serves the one impulse, the less it tends to serve the other. This principle must lead to pathological consequences, when the two fundamental impulses work at cross purposes, and the self-preservation impulse represses any partial impulse that might serve the sexual end. The application of this to visual disturbances can easily be made. The partial sexual impulse connected with the eye might be called sexual curiosity. If this impulse, on account of its undue service in the interests of sexual pleasure, draws to itself the opposition of the self-impulse, so that the ideas in which it expresses itself are repressed, and do not come to consciousness, there is sure to be a disturbance in the relations of vision to consciousness. The self has lost its domination over the eye, which now gives itself over entirely to the service of the repressed sexual impulse. It gives the impression of having gone too far in the repression of the partial sexual impulse, in that the self now refuses to see at all, since the sexual interests pressed forward so vigorously in sight. As a sort of retaliation the repressed impulse claims the exclusive use of the eye; and this is the price consciousness has to pay for the repression.

A similar case is that of the hand which becomes hysterically paralyzed after it has attempted to carry out some sexual aggression, but is inhibited from accomplishing its purpose, just as if it remained stubbornly by its impulse to carry out the repressed innervation. In the beautiful legend of Lady Godiva, all the townspeople hid themselves behind closed shutters in order to lighten the task of this lady who was required to ride through the streets naked in daylight. Any one who looked at the naked beauty was punished by losing his eye-sight. This legend is one of many in which the key to interpretation is found in neuroticism.

Freud says the criticism that these pathological processes are explained by purely psychological theory is unjust, since the emphasis in all of these cases is placed upon the pathogenic rôle of sexuality, which is certainly not exclusively psychic. Psychoanalysis never forgets that the psychic factors rest on the organic, although its work only leads to the latter, and it does not attempt an organic explanation. It is also ready to postulate that not all functional disturbances of vision are of psychic origin. When an organ which serves both kinds of impulses increases its erotic rôle, it is very probable that this does not happen without a change of irritability and innervation, which manifest themselves as disturbances of the function of the organ in its service of the self. It is not improbable that there may be toxic changes at the basis of a change of the organ's service from that of self to that of sexual ends. The term neurotic disturbances covers disorders of functional or physiological as well as of toxic origin.

7. FREUD, S. *Ueber den Gegensinn der Urworte*. Jahrbuch für psychoanalytische und psychopathologische Forschungen. Band II. 1910. pp. 179-184.

In this article Freud points out a striking parallel between certain dream phenomena and certain ancient linguistic usages. He maintains that, in dreams, the negative does not occur. Opposites are brought into unity, or are presented as one, with peculiar predilection. Since in dreams all desirable things are attained, because of the law of wish fulfilment, there can be no opposite or opposing factor.

The dream interpreters of old seem to have recognized the fact that in a dream a thing can represent its opposite.

Freud says he reached an understanding of this peculiar dream phenomenon of avoiding the negative and of presenting opposites with the same word, on reading Abel's pamphlet. This author points out the great age of the Egyptian language, and then shows that in this language there are many words which possess two meanings, the one of which is the direct opposite of the other. It will thus be seen that this familiar characteristic of dreams is identical with that of the oldest of ancient languages.

The explanation which Abel offers for this characteristic of ancient languages is as follows: Our notions of things are a product of a process of comparison. If it were always light, we could have no conception of darkness. All things are thus relative to one other. Thus every conception is, in a sense, a twin of its opposite; and, originally, the one could not be thought of without the other. Thus, one word always brought to mind both ideas; and the two ideas were expressed by the one word. It was by a gradual process that each idea came to have a term of its own, and could be thought of without its opposite. In writing, the ancient Egyptians always used a determining picture before the word to designate the meaning intended. Two words were subsequently evolved which sprang from the same root with its double meaning. According to this writer, the same characteristic is common to the Semitic and to various European languages. In Latin, *altus*—means high, and deep; *sacer*,—holy, and damned. Some phonetic modification may be made as *clamare*, to cry out—*clam*, quiet; *siccus*, dry—*succus*, soft. In German *Boden* means the uppermost as well as the lowest in the house, even to-day. From *bös* (*schlecht*) sprang *bass* which means good; in old Saxon *bat*, which means good, as against the English, bad. In English, "to lock," as against the German *Lücke*, a hole, illustrates the same phenomenon.

Another peculiarity of the Egyptian language is that the letters of a word may be reversed and still represent the same thing. If *bad* were Egyptian it might also be written *dab*. This also holds true of other languages. It is also a thing which children take a peculiar pleasure in doing. In dreams, the material is often reversed to serve a definite end. Here,

however, it is not letters that are reversed, but ideas and images. Freud thinks that this similarity between dreams and ancient languages justifies the inference that dreams are regressive and archaic in character; and that, to understand dreams, we must know more of the evolution of language and speech.

8. FREUD, S. *Beiträge zur Psychologie des Liebeslebens*. Jahrbuch für psychoanalytische und psychopathologische Forschungen. Band II. 1910. pp. 389-397.

Freud believes that poets have been entrusted too exclusively to tell us about the psychology of love, and its various manifestations. Their aims have never been to be true to the fact, for they always make full use of poetic license in dealing with this theme.

Psychoanalysis gives special opportunity to obtain glimpses into the love-life of patients, which one may also notice in daily life after one's attention has been called to it. Certain types are discovered on the basis of object choice. The type here discussed is characterized by several conditions which call forth the feeling.

The first condition is that of including an injured third party. Such a man never loves a woman who is free or has no lover. Sometimes the woman may even be ridiculed until she enters into the above-mentioned relation, when she at once becomes the object of the most intense love.

The second condition of love is that the woman be not virtuous, or, at least, not above suspicion. This characteristic may vary from the flirt to the genuinely polygamous coquette.

As the first condition gives opportunity for satisfying the malignant feeling or impulse towards the man whose loved one is won away from him, the second condition gives opportunity for the exercise of the feeling of jealousy, which seems to be a necessary accompaniment of this type of love. It is only then that the woman attains to full worth in the eyes of the lover. Strangely enough, jealousy is never directed against the rightful possessor of the loved one, but against the new-comer, with whom the woman might be brought into question. It is only during this triangular relation that the love continues. This is an abnormal condition, because, in normal love, the moral integrity of the woman is a necessary pre-condition. A peculiar trait of this type of lover is that he wants to save his object of love from a career of vice. But a successful accomplishment of this purpose does not intensify the love relationship; in fact, failure to save her increases his love.

A psychoanalysis of these characters reveals the fact that there is one determining cause for these various conditions of masculine love. It springs from the infantile tenderness towards the mother, which has become fixed. In normal love, there remain few traces of this early attitude towards the mother. Occasionally they manifest themselves in cases in which young men fall in love with older women. In the type here under discussion, the object of love is a mother surrogate, because the mother's influence cannot be cast off. This accounts for the fact that the woman who attracts attention must be attached to a third party. The child soon learns that the mother is united to the father; and the latter becomes the injured third party. The intensity and fidelity of the devotion of this type of lover is also an echo of the undivided love of mother. The frequent change of the object of love also suggests that the surrogate does not fully satisfy the unconscious demands of the individual.

How does this love for an unfaithful coquette spring from the mother-constellation of the child, when the very term mother is the direct opposite of prostitute in our adult conscious minds? The unconscious often considers as one what consciousness separates into opposites. Here, again, we must go back for an explanation to the time when the child obtains

his first knowledge of the sexual relations of adults. This information often comes in ways that destroy the child's faith in adults. He may even deny that this relation between adults of the opposite sex applies to his own parents. At about this time, he also learns that some women become prostitutes; and that their conduct destroys people's respect for them. When he learns that his mother is not different from other parents in sexual matters, he cynically says that, after all, there is not such a great difference between his mother and the prostitute, because both are guilty of the same thing. This information awakes his memories of his infantile impressions and wishes, which again become active. But the father stands between him and his desire. The Œdipus complex becomes active. He therefore lives, in fancy, his wish fulfilment. The two motives of desire and revenge are favorable to the fancy that the mother is untrue. The lover with whom she is untrue is usually the idealized, mature self.

It is thus easy to see that this family romance leaves traces in the unconscious; and that this is why it is necessary for the woman to be a coquette or a prostitute in order to arouse the passion of love in the adult. The pubertal fancies persist in the unconscious, and demand satisfaction in the reality of later life.

The desire to save the woman who is loved springs from the parent-complex. When the son learns that he owes his life to his parents, he is seized with a desire to repay them in some equally worthy way. His attitude toward the father becomes more haughty and he fancies that he saves him from some great danger. Toward his mother, his attitude is more tender and worthy, and the notion of saving his mother is transformed, in the unconscious, to a desire to present her with a child,—naturally a child like himself. The mother has given the child his life, and he can only give her another life, that of a child which resembles himself. In this sense he identifies himself with his father, and wishes to become his own father. Thus, the notion of saving the woman he loves really means to bring a child to birth; and the symbol must be interpreted just as in dreams. The idea of danger is associated with the birth of the child. Freud thinks the experience of being born is a sort of type of all later danger and anxiety, since it left an affective impression which developed into anxiety.

9. SADGER, J. *Aus dem Liebesleben Nicolaus Lenaus. Schriften zur angewandten Seelenkunde*, 1909. Sechstes Heft. pp. 1-98.

This is a psychoanalytical study of the love between the poet Lenau and Sophie, the wife of his friend Max Löwenthal. The writer points out that in any such triangular relation, the situation is wholly in the hands of the woman. Sophie Löwenthal was an intelligent woman, who married at the urgent entreaties of her parents, and not because she loved her husband. She felt he was not her equal. She had three children; and, at twenty-six years of age, refused to have further sexual relations with her husband. She was sexually anaesthetic, excepting that she loved to be caressed and kissed. This she received from Lenau freely and almost daily during their period of love. Her husband was assured that Lenau would not go too far because of her peculiar condition. This is a typical symptom of hysteria, in the case of women who have borne children. Sophie had other symptoms of hysteria also.

The influence of her father dominated her entire life. He had the patriarchal attitude and Sophie had more than a child's love for him. He called the children together two or three times a week to tell them of nature and of history, and to these recitals Sophie listened intently. From this relation with her father, she acquired the longing to associate with famous men.

It is well known that the first love of children is always the parent of the opposite sex; and that later so-called first loves are simply the renewal of

this earlier love in disguise. This was strikingly true in the case of Sophie. The two men whom she really loved resembled her father in many ways.

The love of the child for its parent should not be confused with the conscious sexual love of later years. The tender love of Sophie for her father is a universal phenomenon, and contained nothing but the purest sentiment. He kissed her, embraced her, took her on his lap, carried her upon his arm, and the like, as any father would do. However, it is nevertheless true that this early experience sinks deeply into the child's very soul and often determines its later love-choice. It is held by many writers that, in cases where two persons fall in love at first sight, they resemble each other. This is explained by the fact that the man selects a woman that resembles his first love, *i. e.*, his mother, whom he naturally resembles. The same is true with the woman. Thus each resembles a parent of the other which insures their resemblance to each other. The innocent love between the child and parent, therefore, teaches the child to love, in later life, and determines the choice, all unconsciously.

This explains Sophie's attitude toward Lenau. He was a noted man like her father, as she thought. She granted him everything which her father granted her, in her childhood and refused other concessions because she repressed these in her attitude toward her father. Sexually anæsthetic women are often made so by the fact that they repress the incestuous feeling toward the parent of the opposite sex at puberty, and continue in this attitude toward the men of their choice throughout life.

Her piety also sprang from her love for her father, which shows that religion and love have the same foundation. God becomes the embodiment of fatherly virtues. At fifteen, she placed all suffering upon the Lord who cares for all as her father cared for his children. Her hope for another life was due to her unsatisfied longings in this life. This played an important rôle in her love for Kochil, her first lover, whom she surrendered at the request of her father. The only other man she ever loved was Lenau; and toward him she manifested the same attitude that she did towards her father.

In Lenau's early life the *Œdipus* complex was unusually well developed; and he never succeeded in getting away from his mother's influence. She was intensely emotional and violently passionate. She threatened to take her life, when her dear ones died, just as Lenau later threatened to do. Her attitude toward her son, Lenau, was always characterized by the strongest emotion and love. She saw his father's traits in her son, and loved him the more for this because her husband died when Lenau was five years old. She idolized him in the most extreme manner. At times she was possessed with the idea that she might lose her son, or that something had happened to him when away from her. She frequently deserted her second husband and children to follow him. She often prepared special food for him, and served it while he was still in bed. His will was always supreme. His mother sowed the seeds of megalomania in many ways; and in adult life these childhood fancies and impressions dictated his entire life. He felt that the world did not recognize his worth, and did not reward him as he deserved. He tried to act the part of a nobleman although his means did not allow it. He could not endure a joke at his expense; and tolerated moods only in himself. He became desperate, when fate did not always deal with him as his mother did. He became intolerably indolent in adult life, because his bodily and mental wants as a child had been so completely satisfied by his mother. He refused to strive or plead for anything as a man because such a course had not been necessary to secure what he wanted from his mother.

He could never love a woman unless the conditions were identical with those of his early home life. He must be the centre of attraction, with no rival in sight. He must be allowed to live the same life of indolence and carelessness as when he was a boy; he must be allowed to come and go

when he pleased, to talk or be silent as his mood dictated. Three families catered to these caprices. The first was that of his sister, Therese, who was attached to him from childhood. She loved him more than her husband and children; her attitude toward him was similar to that of his mother. Emile Reinbeck also gave him the same attention, and he loved her devotedly. But the woman who most nearly embodied his mother's attitude toward him and whose intuition led her to adjust herself more and more to this pattern of his mother was Sophie Löwenthal.

He even demanded good food from his feminine friends as his mother had always catered to his appetite. He never went walking for his health, never took a bath, never ventilated his room. His mother's influence was evident even in such matters as his sleeping with the candle burning, and his invariable habit of taking more personal effects (umbrellas, books, canes) with him than he needed, whenever he travelled.

It is clear that through his mother's mistaken kindness, Lenau was mightily influenced in large matters as in small, in youth as in later life, in his character as in his bodily condition; and this gave direction to his insanity. Every woman, whom the poet could love, must remind him of his mother. In fact, in loving other women, he only loved his mother in disguise.

He had sadistic, as well as homosexual, tendencies, which, however, did not crop out until his insane period when his inhibitions were removed.

His love for his violin and guitar was distinctly erotic. Both these instruments have long been regarded as symbols of the feminine form, and this was plainly the case with Lenau. This was more evident in his insane period, when he adopted the same tender attitude toward his violin that he had previously manifested toward the women of his love. He would allow no one to touch it. Both homosexual as well as heterosexual motives were associated with the violin. When he disliked his violin teacher, he also discarded the instrument for the guitar. But when he found a violin teacher whom he loved, his love for this instrument also returned. At times he played his violin all night long; this long-continued activity gave rise to a state of intense exaltation, but it was followed by a reaction. At times, he played Beethoven with such vigor that drops of perspiration appeared upon his face, and he became completely exhausted. When he was intensely in love with a woman who responded to his love, his interest in his violin decreased, but was again awakened when his love grew cold.

There is evidence for a belief that he masturbated more or less. In conversation he was pains-takingly careful not to allude to anything questionable, and he was mortally offended when others mentioned such subjects to him. This phenomenon is frequently caused by a desire to compensate for a secret vice. The healthy man does not take pleasure in sensual conversation; neither is he deeply affected by it.

His teacher, Kovesdy, stimulated his homosexual nature as his mother did his heterosexual.

His love episode with Bertha left an indelible impression upon him. Her unfaithfulness to him caused him more pain and misery, in his later life, that can fully be accounted for by the fact that he inherited a despondent disposition. This suffering did not begin until after his mother's death. It would seem that he, in some way, identified his mother with Bertha, because in his childhood his mother was untrue to him in that she bore children to his step-father. But more likely there was also an element of self-condemnation in it, because he was untrue to his first love.

His fixed idea to come to America in spite of the protestations of friends and relatives points to unconscious, repressed, erotic feelings. First of all this idea sprang from his identifying himself with Kovesdy, who had failed to carry out his plan of coming to America. Lenau thus wanted to complete his friend's wish, and at the same time to regain his purity in

a strange land, in order thereby to become worthy of his mother again. He carried out his idea faithfully while in America; but, after his return, intense happiness alternated with deep melancholy. It was at this time that Sophie Löwenthal began to exercise her influence over him. His identification of Sophie with his mother was a very gradual process. She first manifested great interest in his poetic works, which enabled her to creep into his affections. Her indifference toward her husband led her to value his attentions. His mother's recent death left a vacant place in his affections which Sophie soon began to occupy, because of her tenderness to him. When he discovered her to be with child there returned the old feeling which he had formerly experienced toward his mother when as a child she became pregnant from his step-father. His love for Sophie now became all the more intense.

From this time on, Sophie refused her husband marital intimacy, and informed Lenau of the fact. This fulfilled a strong childhood wish in reference to his mother and completed the identity of Sophie and his mother.

A peculiar relation existed between his love and his piety. When in love, he was very religious; when not in love, he was sceptical. At times he almost identified God and Sophie. Both he and Sophie believed that, in the hereafter, their fondest hopes and longings would be realized. He pictured the hereafter thus: "My atmosphere will be your breath; my light will be your eye; my drink will be your word; my blood, your kiss; my bed, your heart; my place of abode, the kingdom of God with you, dear Sophie!"

Sophie's hysteria demanded a lover who was satisfied with the satisfaction of the impulse to contrectation without detumescence, to use Moll's terminology. Both were saved from the evil consequences of unsatisfied sexual excitement by a greater ailment. It was a case of a smaller evil being swallowed up by a larger one. In her case, it was hysteria; in his case, his serious affliction which later culminated in insanity. Under similar circumstances, a normal person would have been subject to anxiety neuroses. Sophie's jealousy was in a measure due to this partial dissatisfaction, as is the case with most women who are jealous.

There is little doubt that, if Lenau and Sophie had been free to marry, they would never have lived together long. His inability to attach his attention to any one thing for a long time, as well as her anæsthetic nature, would have tended against this. It was only the thought of constant danger of separation that bound them together. Lenau appreciated this when he said that his misfortune was the greatest joy of his life. In both cases, they obeyed the law of love, and ignored every other consideration. At one time Caroline Unger seemed about to supplant Sophie. Her success was, at first, due to her motherly kindness to him, and her effort to make him happy. But later, she began to ask favors herself instead of giving them; and this was so different from his mother's attitude that he soon cast her off. But the subtle influence of Sophie in portraying her lack of virtue and in pleading with him also had its effect. Here again his despondency upon finding that Caroline was unworthy seems to have made an impression somewhat akin to his Bertha experience, and it may be explained in the same way.

His later life was filled with various premonitions of the final catastrophe which culminated, in 1844, in insanity due to a syphilitic infection which he had acquired some twelve years before. It is very likely that Sophie's love and attention tended to cheer and sustain the poet in his last years of affliction before the final breakdown.

10. FERENCZI, S. *Introjektion und Uebertragung*. Jahrbuch für Psychoanalytische und psychopathologische Forschungen, 1910. Band. I. pp. 1-38.

This article is composed of two parts.

1. *Die Introjektion in der Neurose*, and 2. *Die Rolle der Uebertragung bei der Hypnose und Suggestion*.

The most prominent feature in the psychoanalytical treatment of hysteria is the process known as transference (*Uebertragung*) of emotional activity from some person previously known by the patient, to the physician. However, this transference, or tendency to transference of emotion, is not alone characteristic of the psychoanalytic treatment, but is manifest at all times, and seems to be a fundamental attribute of this form of the psychic mechanism. The apparently unmotivated but extreme expression of love, hate, or sympathy of neurotics is the transference of feeling from some long-forgotten psychic experience to the person under consideration. In these cases, the unconscious complexes which are strongly toned with feeling over-emphasize the emotion manifested towards the person, by being brought into some kind of association with the idea of him. This extreme manifestation of emotion has long been noticed in hystericals; but it was regarded as a simulation of feeling, because there could be discovered no adequate motive for the feeling. The feeling is, however, genuine, and receives its motivation from the unconscious complexes which remain in the background, but use this means of expressing the accumulated emotion that has been waiting for an outlet. The discovery of this mechanism is due to the investigations of Freud. The tendency of psychoneurotics to simulate, and the so-called "psychic infection" among hystericals are not simple automatisms, but find their explanation in the unconscious wishes and desires of the patients. Frequently, a patient assumes the symptoms of another person, because he identifies himself with that person, for one reason or another. Intense sympathy springs from this same source. The impulsive acts of generosity and charity are, also, reactions to these unconscious demands and may, in the last analysis, prove to be egotistical.

The fact that movements of reform or movements of a humanitarian nature often secure recruits, in large measure, from neuropaths is due to the transfer of interests from egotistic, self-condemned tendencies of the unconscious, to subjects in which these interests can find expression without repression and criticism or condemnation. The tendency of hystericals to eat indigestible foods, their desire to eat at a strange table, or to eat food of a peculiar form or consistency all point to a transfer of interest from repressed, erotic tendencies, and reveal a state of unsatisfied sexual impulses.

The business of the psychoanalyst is to provide a means by which the emotion attached to a repressed complex may find expression, by being transferred to some other object; and the physician usually becomes this object. But this is only a temporary make-shift, and the real cure is brought about by leading the patient to resurrect in consciousness the source of his emotions in the repressed unconscious complexes.

The reason why the physician is so often the object toward which the transference is made is that the Œdipus complex is almost invariably present in the patient; and the physician's fatherly care easily leads to the same attitude towards him that was manifested toward the parent in childhood. Sometimes, a trivial factor may bring about the transference, such as the color of the hair, the facial expression, a gesture, the manner of holding the cigarette or the pen, the identity of name with that of a friend of the patient, etc. The sex of the physician is, of course, important. In the case of female patients, this frequently suffices to attach this feeling to a male physician. But the homosexual component, that lies hidden in every male, may lead male patients to make the transference.

This transfer of emotion from one object to another is a fundamental characteristic of neuroticism; it explains conversion and substitution as symptoms of hysteria. All neurotics suffer as a result of withdrawing the *libido* from certain, previously pleasurable-toned complexes of ideas.

If the withdrawal is not complete, the interest in that which was previously loved or hated is lessened. If it is more complete, the complex is wholly repressed and forgotten for consciousness. But it appears that the psychic mechanism cannot endure the *libido* separated from its complex; it is, therefore, transformed into anxiety. Psycho-neurotics have a similar tendency to withdraw the psychic *libido* from certain complexes; and this gives rise to a form of enduring unrest which the patient seeks to mitigate. It may succeed, partially, in conversion,—which leads to hysteria,—and in substitution,—which leads to anxiety neuroses. But this never succeeds completely; and there always appears to be a portion of the impulse which seems to seek satisfaction in the external world. This accounts for the neurotic's tendency to transfer emotion from one object to another.

A comparison of neurotics with those who suffer from dementia præcox and paranoia will throw light upon the former. In dementia præcox the patient loses his interest in the external and becomes autoerotic. The paranoiac projects all interests, which have become painful, into the external world. The psychoneurotic acts in a manner which is diametrically opposite to paranoia. He takes up a great part of the external world into the self, and uses it as a basis for unconscious fancy. It is a sort of attenuation process by which the free, unsatisfied and not to be satisfied unconscious wish stimuli are weakened. This process is called introjection as opposed to projection. The neurotic is constantly in search of objects, with which he can identify himself and to which he can transfer feelings, and which he introjects, or draws into the circle of his interests. The illness is due to an enlargement of the self. Both projection and introjection are extreme forms of psychic processes which are present in normal life. In the child, everything is projected into the external world, and in paranoia the same thing is true, in an effort to minimize the self. The first love and hate are a transference of autoerotic pleasure and displeasure to the object that arouses these feelings. Freud even goes so far as to say that man's philosophy and religious metaphysics are only a projection of his feeling stimuli into the outer world.

But introjection plays an equally great rôle. This is indicated by the fact that so much of possible human experience is reflected in mythology.

The neurotic thus uses a normal mechanism when he attaches his feeling to all possible objects which are not directly related to him, in order to be able to leave in the unconscious the attachment to objects that are closely related to him.

The difference between the normal and the abnormal is one of quantity. The normal person transfers his affection upon much better grounds, and does not dissipate his mental energy in such useless ways as the neurotic. In the normal person the introjection is a much more conscious process, while with the neurotic it is largely a matter of unconscious activity.

This transference of affects from the patient to the physician is at the basis of all cures brought about by electro-, mechano-, hydro-therapy and massage; as well as all other cures wrought by suggestion and hypnotism.

The second part of this article applies the principle of transference to suggestion and hypnosis. The explanation of these phenomena, which assumes that the implanting of the idea of sleep by the hypnotist leads to dissociation, and that ideas presented to the subject will then easily have the right of way over others, does not seem satisfactory. There are certainly deeper psychic forces at work, of which, as yet, no full account has been taken. Evidence is accumulating daily, which points to the fact that the main work in hypnosis and suggestion is done not by the hypnotist but by the subject himself. The existence of auto-suggestion and auto-hypnosis, on the one hand, and the work of the so-called "mediums," on the other, argue that the function of the hypnotist is a subordinate one. Psychoanalysis has shown that even in normal persons in the waking state,

the conditions for dissociation are always highly favorable. It has also shown, that in the course of the development of the civilized individual, many impulses are repressed, and that these repressed impulses, with their accompanying unsatisfied affects, are always ready to transfer themselves to persons and objects in the external world, and to bring the latter unconsciously into touch with the self or to introject them. In hypnotism and in suggestion, the rôle of the hypnotist reduces itself to an object to which the unconscious transfers affects for its own relief.

The significant part which the parental complex plays in the life of each individual is the basis for this transference of emotion in hypnotism and in suggestion. The same complexes are brought into play in the normal individual that are active in psychoneuroses. The hypnotist may turn toward himself certain complexes in the subject's unconscious mental life, that are toned with fear, hate, anxiety, etc., because something about him leads the subject to identify him with some person who has previously aroused these same feelings. This usually goes back to childhood experiences, that were repressed in later life.

It has been found that sympathy and respect greatly enhance the possibility of suggestion and hypnotism. There is much evidence for believing that the unconscious affects play the principal rôle in both suggestion and hypnotism; and that these are, in the last analysis, feelings connected with the sexual impulse, which are transferred from the child-parent complex to the hypnotist-subject complex. Everything points to the fact that, at the basis of every feeling of sympathy, there is an unconscious sexual element; and when two persons meet, the unconscious factors attempt to make the transference. If the transference is successful, be it a purely erotic feeling, or a sublimated one of respect, esteem, friendship, etc., there springs up the feeling of sympathy between the two. If there is objection to this transference on the part of the fore-conscious, other feelings spring up which may lead to antipathy, disgust, etc. The question as to whether any person can be hypnotized depends for its answer upon whether there is a possibility of transference of the unconscious sexual attitude of the subject to the hypnotist. This, in turn, is determined by the parent-complex. The great variation in the proportion of hypnotizable persons, as reported by various authorities, finds its explanation here. Some succeed in only about fifty per cent. of their cases, others reach as high as ninety-six per cent. An imposing-looking hypnotist is much more successful than one of a different type. Long, black beard, great stature, heavy eyebrows, penetrating eyes, forceful but trust-awakening countenance, self confidence, good standing in the community,—all help. Commands given with force and clearness, so that opposition seems impossible, are helpful. Sometimes a surprise, a sudden and loud call, a bright object, a tense and rigid expression of face, clenched fists, succeed when other means fail.

An entirely different method may be used. This requires a darkened room, absolute quietness, friendly talking in a monotone, a mild melodious voice, gentle stroking of the hair and forehead, etc.

These two methods might be considered as making use of anxiety and fear, on the one hand, and of love, on the other. Experts adopt one or the other of these methods, as the case requires. The one method involves the attitude of the father toward his children; and the other that of the mother. In each case the unconscious complexes which were established in infancy are appealed to. These complexes were usually fixed by the parents, in trying to induce sleep in the children. Even holding before the eyes a bright object, or placing a ticking watch to the ear, both of which methods are, at times, employed to induce hypnosis, are excellent means of arousing childhood memories. This child-attitude, on the part of adults, is not so foreign to maturity as might appear, because this attitude plays a prominent part in our dreams.

Forgetting, in the sense of complete disappearance of all traces of a former experience, is as foreign to the facts, as the annihilation of energy or matter is in the physical world. Psychic processes may be revived after decades of oblivescence.

The unconscious, childhood memories tend to make the adult submissive to those persons who resemble, in any way, his parents. There is reason to believe that the hypnotic credulity and docility has its roots in the masochistic compounds of the sexual impulse. Masochism is pleasurable obedience which the child learns from its parents. The parental influence often acts almost like a past hypnotic suggestion upon the later life of the child. Both hypnotism and suggestion are due to the transference of the repressed elements of the sexual impulse from the subject to the hypnotist. This is due to the child-parent complex which becomes active between the subject and the hypnotist.

11. JONES, E. *The Action of Suggestion in Psychotherapy*. Journal of Abnormal Psychology, Dec., 1910, Jan., 1911. pp. 217-254.

This discussion is based upon the conception of suggestion and hypnotism, as worked out by Ferenczi in "*Introjektion und Uebertragung*," and since the main facts of this paper are summarized above it will not be necessary to restate them here. Dr. Jones agrees fully with Ferenczi in giving emotion a prominent place in making suggestion possible. This is in harmony with the views of Bleuler and Lippé. "The peculiar *rapport* between the operator and the subject, so characteristic of the hypnotic state, is identical with that obtaining between physician and patient in the spontaneous somnambulism of hysteria." The basis of this *rapport* is sexual attraction. In the majority of cases, it is unconscious, but not in every case. This was foreshadowed long ago in the theories which postulated a magnetic fluid, vital fluid, nervous fluid or an all-prevailing ether, and lastly a special psychical influence of the hypnotist. This was supposed to be emitted from the eye, because the eye has been symbolical of the male organ and its function.

Janet is quoted to show that hypnosis induces the following changes in the subject: any fear of, or repugnance toward, hypnosis is replaced by a passionate desire for its repetition, and the patient is excessively preoccupied with the physician. At times a period of somnambulant passion lasts until the next *séance*. Janet further writes: "What one most frequently observes is a feeling of affection, which may become extremely intense. The subject feels happy, when he sees his hypnotizer, when he speaks to him; he experiences pleasure when he thinks of him; and consequently soon comes to the point of feeling a strong love for him." Hystericals are very jealous of the physician's attention and interest in them.

Dr. Jones believes that this attitude of "warm affection, dread, jealousy, veneration, exactingness" toward the physician is derived from the psychosexual group of activities. Janet rejects this interpretation; but Jones is convinced that Janet has not traced these conscious emotions to their source; if he has done so, he could not fail to recognize their nature.

The relation of suggestion to psychoanalysis must first be pointed out, before an evaluation of each of these methods of treating psychoneuroses can be made. In both methods, there is a transference of psychosexual affections from the patient to the physician; but, where suggestion alone is employed, the treatment stops here, while, in psychoanalysis, the patient is helped to trace his illness to its source; and then "the wishes, desires, etc., which had previously found unsatisfactory expression in the creation of various symptoms, are now free to be applied, through the process of sublimation, to non-sexual social aims."

Treatment, by means of suggestion alone, really intensifies the transference. The result of this is that the patient never really is cured nor

becomes independent of the physician. If one symptom is removed, another takes its place; and chronic invalidism often results. Psychoanalysis brings permanent relief wherever transference can first be brought about by helping the patient to sublimate the psychosexual emotions to higher ends.

12. BRILL, A. A. *A Contribution to the Psychopathology of Everyday Life*. Psychotherapy, Vol. III, No. 1. pp. 5-20.

Dr. Brill is a disciple of Freud, and employs the terms unconscious, repression, and complex, in the Freudian sense. He conceives all hysterical symptoms to be the expression of a repressed wish, which is active in the unconscious. Unconscious processes are defined as those processes which show active manifestations, but of which the person concerned is not conscious because of repression, due to conflicting impulses. This psychic mechanism in hysteria, Dr. Brill conceives to be common, in a mild form, to all normal minds. This is manifested in dreams, and in everyday actions. The tendency to forget, or crowd out of consciousness all thoughts of a disagreeable or painful nature, is at the basis of these everyday manifestations. These thoughts are not really forgotten; they are repressed, and they remain in the unconscious as complexes. Here they lie dormant until some experience or association taps their feeling content which is always strong. There is always a resistance to their becoming conscious so that the individual is never able to tell just what is actually taking place.

In everyday life these repressed complexes manifest themselves in "lapses of memory, in talking, writing, etc."

Familiar illustrations of this are the forgetting of the names of well known persons, and the like. This is due to the fact that the name is associated with some repressed complex, which prevents recall. Later, when the association is broken the name may come freely.

A woman refers to one of her married friends, but by mistake uses her maiden name instead of her husband's name. Psychoanalysis shows that she does not like her friend's husband, and wishes her friend had never married him. In using her friend's maiden name, she fulfilled a wish and revealed that the husband's name was repressed. She was, of course, unconscious of the motive for this.

A man is urged by his wife to attend a social function, which he does not care for but agrees to attend. In dressing for the occasion he suddenly finds the trunk containing his dress suit locked and the key lost. This compels the sending of regrets. Next day the key is found in the trunk. The husband declares he did not conceal the key intentionally. But the motive is clear; it carried itself out when he was off his guard.

Many so-called meaningless, or automatic, indifferent or accidental actions such as "scribbling with one's lead pencil, jingling the coins in one's pocket, kneading soft substances, etc., conceal sense and meaning for which any other outlet is closed."

A maiden lady wears a wedding ring "because it was grandmother's." A patient, who despairs of life, manifests special interest in Ibsen's "When the Dead Awaken." An embezzler is discovered in a distant city reading the book, "Will I ever go back?"

Dr. Brill gives a wealth of illustrations to show that so-called meaningless actions are symbolical of a deeper meaning, and adds: "These examples show that there is nothing arbitrary or fortuitous in our actions, that, no matter how we may try to conceal things, we always betray ourselves. Our repressed thoughts forever strive to come to the surface; and just as the insane realize their ideals in their insanities, we realize our wishes through our dreams, and in the 'little ways' of everyday life."

If Dr. Brill's contention is true that the determining motives to conduct are often, if not usually, below the threshold of consciousness, it has the very greatest significance for the student of normal psychology. In fact,

it almost becomes revolutionary, and challenges the truthfulness of all so-called introspection. If one cannot tell what motives lead to this or that choice, action or even association, much of what has been accepted as orthodox psychology must be radically revised. The validity of all experimental laboratory psychology that is based on introspection is thus brought into question. It also plays havoc with the contention that consciousness is the only legitimate field for the student of psychology. Dr. Brill is, of course, not alone in his point of view. Evidence in support of this view is being accumulated daily by the whole Freudian school.

The genetic psychologist, too, is taking this stand for reasons other than those of the Freudian school. From an evolutionary view-point, it is fully justified. In fact, it forces itself upon any one who attempts to follow up the evolution of psychic life from primitive forms of life, as leading geneticists are pointing out.

13. PUTNAM, J. J. *Personal Impressions of Sigmund Freud and his Work, with Special Reference to his Recent Lectures at Clark University.* Journal of Abnormal Psychology, Dec., 1909, March, 1910. pp. 1-26.

Dr. Putnam laments the fact that the Freudian theories have been so long neglected, and considers it to be "a reflection on our energy and intelligence that we have not gained a closer knowledge of the claims and merits of his doctrines." He also points out the peculiar prejudices and misconceptions that are current concerning Freud's point of view, and thinks that a better acquaintance with his work would remove much of this unfavorable attitude. The emphasis which he places on "sexual life in the etiology of psycho-neuroses" is largely responsible for this prejudice.

This is itself in need of psychoanalysis. It supports Freud's contention that the motives which actuate conduct are usually below the level of consciousness: and that the individual never gives the real reason for his behavior or attitude of mind. "Motives are made up of 'attraction,' 'desire' and 'acceptance,' on the one hand, and, on the other hand, of 'repulsion,' 'repression,' 'denial,' mixed in equal parts." The very intensity of the opposition to Freud's theory indicates that it touches a tender spot, for the opposition springs up even before the theory is tested.

A strong prejudice often involves the "half-felt but perhaps wholly suppressed truth" of the matter under consideration, which cannot, at present, be put to the test of reason.

Dr. Putnam's aim is to modify this prejudice, which he himself once shared, by setting forth some of these Freudian principles in a manner best calculated to remove misconceptions and invite unbiased consideration. As far back as 1881, Freud and Breuer treated their first case of hysteria, and revealed the germs of Freud's later theories. After a number of years of study with Charcot in Paris, and with other psychiatrists, he continued his treatment of cases of hysteria; and in all of these, he became convinced that the childhood experiences played a very great rôle in producing the later difficulties. The system of psycho-analysis was evolved; and it was found that the emotions of childhood often cropped out in new forms, in later life. Old and forgotten memories were revived; and it was discovered that these were at the basis of the illness, and that when brought up to the level of consciousness the patient recovered. This necessarily gave rise to a new and larger view of the unconscious life than had heretofore been held. The unconscious proved to be the "dwelling-place and working place of emotions that we could not utilize in the personality that we had shaped and rounded."

Dr. Putnam thinks that "looked at broadly and as a whole" Freud's main contribution has been this emphasis of the unconscious phase of life as an active principle rather than the attempt to push forward the sexual

element in our experience. This latter factor is stressed by Freud, but he is fully justified in his conclusions by the evidence bearing upon this point which he secured from his patients. His critics seem to have lost sight of everything else in their "attack against the remarkable and truth-seeking observations of a remarkable man." A plea is made for open-mindedness in considering this phase of Freud's theory, on the ground that it is the first duty of any seeker after truth to hold his prejudices in abeyance and examine the facts in an unbiased manner, even though the subject be disagreeable. The attitude of many people towards the subject of sex is easily explained on Freud's theory of repression. This very repression leads to a denial of its importance. Nevertheless the subject has a "hold on us, or a right to demand our interest and attention," even if "we would persuade ourselves that this was not the case." "This hold upon our attention which we instinctively feel this subject has the right to claim, even when we repudiate this right, constitutes one instance of the 'desire' which is made to play such a large part in Freud's doctrines."

The repression of this instinctive desire may lead to one of three consequences. The repression may be adequate, and the instinctive curiosity may find an outlet in some other channel. The repression may go too far, and produce an over-sensitive individual who is over-refined and over-watchful of himself. Again, the repression may be unsuccessful and the person is then in conflict with himself, and becomes hysterical, or falls a prey to one of the phobias. The conflicting impulses in a human being are so varied and complicated that we are never able to grasp them in their completeness; and perhaps every one could find in himself traces of what would, in a larger scale, be regarded as criminal.

The struggles of the soul are immensely more complex than is generally assumed. "Desire or craving furnishes the motive for many thoughts and acts that seem actuated by sentiments of a different, and even of an opposite, character." The fable of the sour grapes illustrates this.

When desire cannot be satisfied in one way, it is often satisfied in another. This substitution of one situation for another is at the basis of the principle of "conversion" in hysteria, by which the physical symptoms are produced. This principle of substitution is helped by the tendency to forget the unpleasant experiences of life. This latter is a feature of every normal life, although in hysteria it becomes exaggerated. It is due to repression which is an active factor in mental life. In dream life these repressions have a chance to express themselves in a somewhat disturbed form, because they, like psychoses, are in a measure a compromise between conflicting motives.

Freud's therapeutic method is often criticised on the ground that it brings to mind what was unwholesome in the individual's experience and that this should be forgotten. However the best answer to this criticism is that the psychoanalytic method of bringing to consciousness the forgotten memories actually brings peace, comfort and contentment to the patient and that no other method of treatment can effect the same cure.

Freud lays much more stress upon early experiences and environment in producing psycho-neuroses than upon hereditary and nervous instability. While heredity varies greatly in degree of soundness and vigor, it is still the early experiences "which make us sick or well." His theory, therefore, tends to exalt early education as a hygienic measure in the broadest sense of the term education.

In the final section of this article attention is given to the subject which arouses more antagonism to Freud's theory than anything else: namely his emphasis of sex as a causative factor in psycho-neuroses. However, the fact that the unfavorable criticism is of the most contradictory sort indicates that the critics have been moved by deep seated prejudice rather than by cool consideration of the merits of his theory. In spite of the fact "that this immense subject was daily and hourly thrusting itself upon our

notice whether as the cause of terrible suffering, of terrible crimes, of terrible misunderstandings and misjudgments, and that it has played a huge part in the history of religion and of civic progress," there has been a tendency to blind ourselves to the facts and to refuse to study the subject scientifically.

One reason for this is that the term sexual is confused with the term sensual. Hence to assume that sexual influence is basic for psychopathology would be to charge every one so afflicted with immoral characteristics according to this false view. Freud uses the term in a much more comprehensive sense and includes all emotions that have differentiated from the primitive sex impulse. This includes all that has produced the highest and noblest in civilization. The experiences of infancy are causative factors of later neuropathic states but cannot be considered sensual. All students of the subject now agree that the term sex is much more comprehensive than has been customary to regard it and that the distinction between normal and abnormal is not to be too sharply drawn. Freud assumes that civilization has been built up at the expense of sex interests and that the sublimation and repression of the sexual energy is the means of attaining a higher culture. It is in an effort to accomplish this end that repression sometimes goes too far in those persons who have a predisposition to neuropathic states. Since much of this conflict takes place in the unconscious, and the symptoms of over repression are never traceable to their source by the patient, unaided, there is after all no question of moral responsibility. It is an unsuccessful struggle with factors that are clearly beyond the reach of consciousness. The infantile experiences in the sex realm may easily sow the seed of later troubles because the child gives free and full expression to all impulses. In later life these impulses are found to be out of harmony with civilized life and are repressed only to retreat to the unconscious realm where they are still active. The adjustment of the legitimate demands of the procreative instinct on the one hand and the demands of civilization for repression and sublimation on the other is the great problem of modern life. It is just here that the Freudian investigations are most helpful, and Dr. Putnam believes that Freud and his co-laborers have a distinct message for the present age in dealing with this important and ever present problem.

ABRAHAM KARL. *Giovanni Segantini, ein psychoanalytischer Versuch*. Schriften zur angewandten Seelenkunde, Fünftes Heft. Leipzig, 1911. p. 65.

This is another contribution to the psychology of the artist. Giovanni Segantini was a famous Italian painter of the last half of the nineteenth century. His development, his outer and inner life, his artistic capacity and his works were all unique, and challenge an explanation from the point of view of individual psychology. This study applies the principles of psychoanalysis to the life and works of Segantini. The unconscious mechanism of neurotics and artists is similar in many respects and the physician who is acquainted with the method of psychoanalysis of the former has a peculiar advantage therefore in the study of the latter.

Segantini's paintings are in a peculiar sense the expression of his inner soul experiences. His theory of painting was that it should reveal and express the deepest emotions of the artist rather than attempt a true reproduction of any external object or scene. Art, he says, shall glorify work, love, mother, and death. These were the sources from which he derived his inspiration. Although other artists have dwelt on these themes, Segantini has given them a touch peculiar to himself and his genius is limited to these subjects.

His lack of early education and his unwholesome environment did little to exalt these themes. They therefore came largely from his own inner tendencies. Psychoanalysis can throw much light upon the source of these themes because it goes back to childhood and traces the beginnings of the life impulses. Segantini himself thought that a true explanation of his

genius would have to go back to his earliest childhood and analyze all sensations of the soul, even to their faintest beginnings. His mind was free from the burden of traditional schooling and absorbed from his environment whatever it was fitted to assimilate.

The most profound single event of Segantini's early life was the death of his mother, when he was scarcely five years old. After that he lost the influence of a home, was neglected by his father, and became more or less of a wanderer. During all this time the early influence of his mother made her the centre of his thought. In his autobiography he says that he has a clear definite and accurate image of her. He says she was young, tall, and beautiful and compares her to sunset in the spring. This lofty sentiment of love is the sublimated, infantile, erotic attitude toward her. The neurotic and the artist both have abnormally strong impulses which are greatly transformed through repression and sublimation. Both have a very strong fancy. In the case of the neurotic the repressed fancies are converted into symptoms of illness. In the artist they find partial expression in his works. The other part is usually sublimated into some other form of expression. In Segantini this last element was transformed into a compensatory over-emphasis of and admiration for motherhood. This is why Segantini embodied motherhood as the central theme of so many of his paintings. The painting called "The Fruit of Love," was evolved in his fancy by the transformation of a beautiful rose that came from heaven into the form of a mother and child. Here the influence of his long departed mother is seen. He often associated her beauty with that of a rose.

The infantile erotic attitude towards the mother often gives rise to feelings of cruelty against the loved one. This is due to a sort of feeling of revenge for supposed mistreatment. This manifests itself in desiring the death of the loved one; or if death actually takes place in a sort of joy that it occurred. Later when this feeling of cruelty is repressed and sublimated there arises in the mind of the neurotic a feeling of guilt even though no good reason can be given for it. The dead one is glorified and an effort is made to call him or her back to life in fancy.

That Segantini had this feeling of cruelty in childhood is shown by the fact that when twelve years of age he derived real pleasure in trying to paint the face of a dead child at its mother's request and worked for hours at his task. In his later description of this mother, he spoke of her beauty and used the same adjectives that he did in describing his own mother. It was a case of transference of his feeling for his mother to this woman and he thus undertook the task through his mother's unconscious influence. In his effort to please this mother we see the beginning of the sublimation of the feeling of cruelty to a desire to compensate for this feeling. This is a frequent phenomenon in his later paintings. Thus death and motherhood came to occupy his attention during the first thirty years of his life, and this points to his mother's early influence. He was twenty-two years old before he became sufficiently free from her influence to fall in love.

The influence of Segantini's father is not noticeable because of his father's treatment of him. In fact all traces of a father's influence such as conservatism, obedience to authority, reverence for God, etc., are negative in the character of Segantini. Home, mother, nature form a closely knit complex in his life. When he lost his mother he lost home and the native scenery that he loved.

In his adolescent years the repression of his sex impulse had a tendency to make him melancholy and passive. He embodied this emotion and passivity in the paintings of this time. Fancies of death also inspired many of his works at this time.

Later, at about the age of thirty, this melancholy gave place to an aggressive impulse to labor. He moved into the high Alps and there studied the natural scenery as he had seen it in his childhood. Everything seemed to inspire him to greater efforts. He was seized with an impulse

to work and seemed never to tire. The aggressive impulse was sublimated into the impulse to work. Here he did his best work. All this time the mother complex remained the same. It was at this time that he painted the masterpiece called "The Two Mothers."

At this time he acquired a technique of color analysis to a high degree and used it very effectively in his paintings. This was a great triumph. It was, however, not so much a result of his artistic genius as it was a demand of his soul in order to give adequate expression to his emotions. Light and color were to him the source of the highest ecstasy. This was due to the sublimation of that component of the sex impulse known as sex curiosity.

Later there was again a return of melancholy. At this time he painted several works that are difficult to explain. One of these is "The Bad Mothers." All products of the imagination according to Freud have a manifest and a latent content. The manifest content is that which consciousness is concerned with while the latent content escapes its notice although it is the more significant. The latent content is the expression of a suppressed impulse. This was the case with the mystical works of Segantini above mentioned. Only the manifest content has received attention by students of his works. Although he got his idea of the punishment of bad mothers from Buddhistic mythology, this does not account for his interest in the idea. Here again the unconscious motive springs from the repressed infantile anger towards his mother for dividing her love with his rival.

The idea of death seems to have had a peculiar fascination for him, motivated many of his works, and at times there seem to have been unconscious longings for death, which in the end helped disease hasten disintegration. His early acquaintance with death in the loss of his brother and mother does not fully account for the dominance of this idea. We must look deeper for the motive to this and it is found in the impulses of his childhood. His sadistic impulses, his feeling of hate and his desire for the death of a loved one had to be withdrawn from the objects against whom they were directed as he grew older. They were partly transformed to thoughts of his own death and partly sublimated into an impulse to live. This conflict of conscious and unconscious impulses is the secret of the tragedy which ended in his premature death.

TERMINOLOGY IN THE FIELD OF SENSATION.

There is at the present time a great deal of confusion in the scientific terminology of sensory processes. In some cases several words have been manufactured which are used to signify the same thing, and some of the terms are quite indefensible. *Acoumeter* and *audiometer* are examples of one such case. In other cases the same term has been used in entirely different meanings. Perhaps the worst examples of this confusion are the terms *hemeralopsia* and *nyctalopsia*, each of which is currently used to signify both day-blindness and night-blindness. The unauthentic usage of these words possibly results from an erroneous impression that the *-al* is privative. A large list of illustrations might be given.

I suggest the following list of terms to cover a part of the field. Practically all are in use, with the exception of those included in 6, 7, and 8. In these cases innovation is absolutely necessary, and the forms there given are analogous to the forms under the other headings, and are from the roots suggested by Professor C. W. E. Miller as the most logical. The table is not complete, but the terms to cover the remainder of the field should be constructed in accordance with the principles applied here, which are drawn from the best present usage.

1. Taste *geus(ia)* a-, para-, hypo-, hyper-; -imeter, -ic.
2. Smell *osm(ia)* an-, par-, hyp-, hyper-; -ometer, -etic.
3. Sight *ops(ia)* an-, par-, hyp-, hyper-; -imeter, -ic.
4. Hearing *acu(sia)* an-, par-, hyp-, hyper-; -meter, -sic.
5. Touch (h)ap(hia) an-, par-, hyp-, hyper-; -tometer, -tic.
6. Warmth-sense *thalpo(sia)* a-, para-, hypo-, hyper-; -meter, -tic.
7. Cold-sense *rhigo(sia)* ar-, para-, hypo-, hyper-; -meter, -tic.
8. Tickle-sense *gargal-esthe(sia)* -an-, -par-, -hyp-, -hyper-; -tic.
9. Hair-sensibility *tricho-esthe(sia)* -an-, -par-, -hyp-, -hyper-; -siometer, -tic, -sis.
10. Muscular-sense *kinesthe(sia)* a-, para-, hypo-, hyper-; -siometer, -tic.
11. Body-sense *coenesthe(sia)* a-, para-, hypo-, hyper-; -tic, -sis.
12. Pain-sense *alge(sia)* an-, hyp-, hyper-; -siometer, -tic, -sis.
13. Vibration-sense *palmesthe(sia)* -an-, hypo-, hyper-; -tic.

The termination *ia* is of course used only with a prefix. It would be perfectly legitimate to use the suffix *is* to indicate the sense itself, in all cases (as is done in *algesis*, for example), as we are not bound strictly to the Greek precedent; but as a matter of fact no one has ventured to do this.

Special attention might be called to the use of *acusic* and *opsic* to designate the sensational facts; in place of *optic* and *acoustic*, which have special significance. It is very desirable also that the special prefixes *chrom-*, *achrom-*, *monochrom-*, *dichrom-*, etc.; and *hemian-*, *hemeral-*, *nyctal-*, and *ambly-* should be used with *opsia*, and not with *opia*, as the latter combination is illogical, although it is found at present about as often as is the other usage. In place of the color-prefixes indicated, *chromat-*, *achromat-*, etc., are frequently used, but there seems to be no sufficient reason for the *-at*, and it should be dropped to bring these words into harmony with others already established.

Ope is used as a combining form to indicate either the possessor of a certain sort of eye (*myope*, *emmetrope*, etc.) or the subject of a certain form of disorder of sensibility (*amblyope*, *nyctalope*, etc.). This usage is well established and gives rise to no confusion.

The prefix *diplo-* is uniformly applied to *opsia* (although some authors persist in writing *diplopia*!) and to *acusia*. It might legitimately be applied to *aphia* also. *Ambly-* has been applied to *acusia* in place of *hypo*, but the usage does not seem commendable. *Amblacusia* might, however, be logically applied to the lack of accuracy in pitch discrimination.

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BOOK REVIEWS

Das Gedächtnis im Lichte des Experiments. Von A. WRESCHNER. Zweite vermehrte Auflage. Zurich, Art. Institut O. Füssli, 1910. pp. 77.

Here, in the compass of exactly 70 pages,—less than half the space often taken for a single article,—is a clear and well-proportioned account of the experimental work on memory, from Ebbinghaus down to Katz and Révész. Professor Wreschner has earned the gratitude, not only of the teachers for whom his work was originally intended, but of psychologists as well. A translation would be useful.

The two-page bibliography is not wholly free from printer's errors, and, curiously, omits the dates of many of the papers cited. S. POWER

Observations d'un musicien américain. Par LOUIS LOMBARD. Traduit par R. de Lagenardière. Paris, L. Theuveny, 1905. pp. xxi., 198.

Mr. Lombard, the author of *Observations of a Bachelor*, and the founder and sometime director of the Conservatory of Music at Utica, N. Y., has here brought together a number of notes and addresses, dealing with musical subjects. We have strictures on the songs, operatic performances, composers and conservatories of America; strictures also on the music and dancing of modern Spain, and on the Japanese productions of western music; a number of practical recommendations to students of music; and a few theoretical discussions,—of the sense in which music may be termed a universal language, of the adaptation of musical performances to the taste of the people, of the social utility of art, and so forth. It is all readable enough, and the practical advice is sensible; more cannot be said.

J. FIELD

La vie mentale de l'adolescent et ses anomalies. Par A. LEMAITRE. Saint-Blaise, Foyer Solidariste, 1910. Pp. 240. Price fr. 3.

The work of M. Lemaître, who is a professor in the College of Geneva, is well known to students of applied psychology. In 1901 M. Lemaître published a work on colored hearing, the material for which he had gathered among his pupils; and since the foundation of the *Archives de Psychologie* in 1902, he has contributed to this journal a number of articles dealing with the adolescent mind, and especially with what one may call its shadow side. The present volume brings together, in convenient form, the substance of papers published by the author in the *Archives*, in Janet's *Journal* and in the *Rivista di Psicologia*. The titles of the chapters are Adolescent Thinking (students' views on class discipline, on the ideal fellow-student, on spending money), the More Common and the Rarer Forms of Synopsis (three cases of an uncommon type are detailed), Internal Speech, the Forms of Paramnesia, Mental Dissociation, Complex Hallucinations (two cases), Multiple Personalities, Parapsychism (a name given to a transient state of reverie, obsession, or what not, which results from a physiological crisis and may help in the prognosis of a disease like tuberculosis or of the various modes of psychasthenia), Bad Habits, and Adolescent Suicide. In an epilogue, M. Lemaître makes a plea for individual education. There is no single type of the adolescent mind: *ce type moyen est une pure convention, qui ne correspond à aucun sujet pris isolément*; the mental life is in instable equilibrium, and its oscillations are not only uneven but may also be so

brusque as to set up aberrant types of a permanent but wholly unexpected kind. Education will, in the future, be of the individual sort; meantime, the author recommends a system of compensations, whereby excellence in certain studies shall be allowed to counterbalance deficiency in certain others.

FRANCIS JONES

The Dawn of Character; A Study of Child Life. By E. E. R. MUMFORD. London, Longmans Green & Co., 1910. pp. xi., 225.

This is a very practical little book, written for the guidance of mothers, aunts, nurses, governesses,—of all who, without special training, are called upon to take care of young children. "My endeavor has been," the author tells us, "to interpret the child's experiences from his own point of view. Both in the earlier psychological chapters, in which I have tried to trace his own development; and in the later chapters, concerned with his development in relation to us and our attitude towards him; the aim has been to see, as far as possible, with the child's eyes." The object is worthy, and Mrs. Mumford, so far as the mere adult can judge, has attained a very considerable measure of success.

After an introductory plea for the closer study of child life, we have seven psychological chapters: on the contents and the growth of the child's mind, on the growth of imagination, on the law and growth of habit, and on the development and training of the will. For these chapters the writer has had the advantage of the critical scrutiny of Professor Carveth Read. There follow chapters on the place of punishment in education, on freedom within the law, on childish curiosity, on the dawn of religion, on some different types of children, and on the child's point of view. There are few references; the author acknowledges indebtedness especially to McCunn's *Making of Character*, and to the teaching of Dr. Sophie Bryant and the Rev. Stopford Brooke.

An Appendix, on the gaining of voluntary control in the functioning of the bladder in infancy and childhood, closes the book which, unfortunately, is not provided with an index.

O. PERLER

Kleine Schriften. Von WILHELM WUNDT. Erster Band. Leipzig, W. Engelmann, 1910. pp. viii., 640.

Every teacher of psychology has hoped that Wundt might, some day or other, bring together his scattered psychological essays in book form. The essays supplement the books, at many points; if they are less systematic, they are also more human; and their full discussion of controverted issues is often illuminating. It seems, now, that this hope is in a fair way of being realised; we have the first volume of the *Kleine Schriften*—a truly German misnomer!—and though the present instalment is concerned with philosophy, the next will in all probability be psychological.

Here are reprinted, in revised and extended form, the articles *Ueber das kosmologische Problem* (1876), *Kants kosmologische Antinomien und das Problem des Unendlichen* (1885), *Was soll uns Kant nicht sein? Bemerkungen zu Kants Philosophie* (1892), *Zur Geschichte und Theorie der abstrakten Begriffe; eine erkenntnistheoretische Betrachtung* (1885), and *Ueber naiven und kritischen Realismus* (1896). To these is added (1910) a very timely paper on *Psychologismus und Logizismus*, which may be heartily recommended to every serious student of psychology. From it he will learn that the experimental method came in, not simply as an improvement upon, but also as a protest against *Selbstbeobachtung*; he will see Brentano's work in historical perspective, and will understand its enormous influence; he will grasp the psychological significance of Husserl's *Logische Untersuchungen*; he will discover, among many other interesting things, why the physiologist Helmholtz went for his psychology to John Mill's *Logic*. No one but Wundt could have given us this authoritative exposition. It is only to be regretted

that he has not put it upon the market in separate form; experimental psychologists will hardly be attracted by a large volume of philosophical essays.
E. B. TITCHENER

Handbook of American Indians North of Mexico. Edited by F. W. Hodge. Pt. 2. Washington, Govt. Printing Office, 1910. pp. iv., 1221.

Antiquities of Central and Southeastern Missouri. By G. FOWKE. Washington, Govt. Printing Office. 1910, pp. vii., 116.

Chippewa Music. By FRANCES DENSMORE. Washington, Govt. Printing Office, 1910. pp. xix., 216.

The three works above mentioned are Bulletins 30, 37, and 45 of the Bureau of American Ethnology, published by the Smithsonian Institution. The first of them completes the very useful *Handbook of American Indians*, covering the letters N to Z. Dr. Wissler contributes an article on Psychology, the upshot of which is that we know practically nothing of the subject,—surely a strong indictment against those directors of laboratories who have Indian subjects within their reach; and Professor Boas writes, with more to say, upon Religion. There are many other articles of psychological interest in the volume.

The second Bulletin reports the results of mound-excavation in Missouri. The burial vaults found are a new feature in American archæology so far as concerns the region east of the Rocky Mountains. At least two different stages of culture are indicated; dates cannot be given, but the later stage may perhaps be connected with the Siouian Indians.

The third item upon our list gives the transcription and analysis of nearly two hundred Chippewa songs, collected in northern Minnesota. The author finds that rhythm is the essential part of the songs; words, and even the less important melodic progressions, may vary, but the rhythm is constant. The songs are classified as harmonic and melodic: as harmonic, if their accented tones follow the intervals of diatonic chords, as melodic, if their contiguous accented tones have no apparent chord-relationship: of 180 songs, 41 are harmonic and 139 melodic. The work is well illustrated with portraits, photographs of musical instruments, and cuts of the song-pictures.
J. FIELD

Examination of Prof. William James's Psychology. By IKBAL KISHEN SHARGA, Principal S. P. H. College, Srinagar, Kashmir. Allahabad, Ram Narin Lal, 1910. pp. v., 118. Price One Rupee.

When the incoming graduate student is asked what books he has read, the first item on his list is likely to be James' *Principles of Psychology*. And when he is asked, further, whether he understands and can reproduce James' views, the reply is likely to be a cheerful affirmative. But if the enquiring professor go on to ask for James' conception of the psychological self, or for his view of the relation of mind to nervous system, or even for his theory of emotion, the situation may take on an aspect the reverse of cheerful; James' doctrine is not, after all, as clear-cut as it had appeared; passages from the book that seem to speak definitely in a certain sense may be met by passages that seem to speak, no less definitely, in another.

Some of these contradictions are real, some only apparent; and none detract from the greatness of James' achievement or offer a serious stumbling-block to the trained reader. Nevertheless, it is just as well that they be brought out into clear daylight; and the author of the work before us has done psychology a service in publishing the results of a thorough comparative study of James' text. Unfortunately, perhaps, he has combined the internal and the external methods of criticism; he is not content to find James inconsistent, or to show reasons for the inconsistency, but he

attacks, from the outside, some of the tendencies and principles of the Jamesian psychology. The two aims are entirely legitimate; but they are also distinct; and disagreement with a writer's general attitude may easily lead you to overestimate his slips, and to find contradiction where sympathy would have found only change of standpoint, or mere verbal discrepancy. In some instances, our author seems to have fallen into this trap; in most, however, he has his finger on real weaknesses in James' exposition.

The special points discussed are: the relation of brain to mind, the doctrine of the externality of sensation, the doctrine of the indivisibility of states of consciousness, the self as knower and as known, and James' theories of conception, emotion and volition.

S. POWER

An Adventure. By 'ELIZABETH MORISON' and 'FRANCES LAMONT.' London, Macmillan & Co., Ltd., 1911. pp. vii., 162.

The gist of the 'adventure' is this: On August 10, 1901, two English ladies paid their first visit to the Petit Trianon at Versailles. It was, of course, broad daylight; and the visitors, who were in good health, knew practically nothing of the history of the place. They nevertheless saw scenes and met persons of the time of the Revolution; 'Miss Morison' saw the Queen herself. On Jan. 22, 1902, 'Miss Lamont' visited the place alone, and had similar experiences. Subsequent visits, by both the narrators, passed off normally.

Ch. i. of the present account details the events of the various visits, the two authors writing independently; on the two critical occasions they did not see alike at every point. Ch. ii. gives the results of research: identification of the figures seen, the buildings and grounds passed and traversed, the music heard, etc. Ch. iii. answers some of the questions and meets some of the attempted explanations proposed to the writers by sceptical friends. Ch. iv. seeks to account for the whole set of experiences as the reproduction of a memory of Marie Antoinette's. On August 10, 1792, the royal family was penned up for many hours in the little room opening into the Hall of the Assembly; the Queen, exhausted and exasperated, sought a fleeting relief in recalling the simple pleasures and the country freedom of the Petit Trianon; as her thoughts wandered, incident after incident flashed upon her mind,—the incidents re-experienced by the two ladies, more than a hundred years later.

The publishers guarantee "that the authors have put down what happened to them as faithfully and accurately as was in their power;" the names appended to the narrative are the only fictitious things in the book. Now let conjecture do its work!

J. WATERLOW

The Concept of Method. By C. R. LOMER. *Controversies over the Imitation of Cicero as a Model for Style, and Some Phases of their Influence on the Schools of the Renaissance.* By I. SCOTT. Teachers College, Columbia University, New York City, 1910. Contributions to Education, 34, 35, pp. 99; v., 145.

Dr. Lomer's object is "to emphasise the strong necessity, in the educational theory of the present day, for an analysis of the process of experience itself, with a view to realising its organic character, to making apparent its implications, and to maintaining its ultimate reality, in idea, as the method of our existence." Educational theory has been largely occupied either with the materials of education or, from a purely formal standpoint, with special details of educational procedure. We have in fact, as the terminal aspects in the educational process, the materials that are selected as educationally valuable in the school course, and the child itself, with its impulses, instincts, activities and energies. The problem is, then, to see how these two elements are related in actual experience; to understand education as a method of giving form to the experience of the child. From

this point of view, the author first reviews some historical types of method (the Greeks, Bacon, Descartes, Comenius, Kant), and then attempts constructive work on the function and interpretation of method (the idea of development; the interpretation of experience; the function of method). Unfortunately, his style is obscure, and the connection of his thought not always apparent. As, however, he has read widely, and does not fear to face ultimate problems, we may expect from him, later, a systematic treatise that will be better suited to the average reader.

Dr. Scott writes of Ciceronianism, in the sense of "the trend of literary opinion in regard to accepting Cicero as a model for imitation in composition." The work before us has an introductory chapter on the influence of Cicero from his own time to that of Poggio and Valla (c. 1450), when men of letters began a series of controversies over his merits as a model of style; chapters treating of these controversies; and a study of the connection of the entire movement with the history of education. "At the close of the 16th century, the Renaissance spirit in general had furnished to the schools, as the aim of education, the mastery of the Greek and Latin languages; but the cult of the ultra-Ciceronians had wielded so great influence that that aim, so far as Latin was concerned, had degenerated into the purely imitative treatment of the authors studied, among whom Cicero was given by far the greatest prominence. The dialectic of the Middle Ages had been largely supplanted by rhetoric, and some effort had been made to connect this study with life; but, on the whole, the reign of form had been transferred from logic to rhetoric, and was fighting for prestige there under the banner of New Learning." An appendix contains translations of the controversial letters of Pico and Bembo, and of the *Ciceronianus* of Erasmus.

W. FRANCIS

Ueber die körperlichen Begleiterscheinungen psychischer Vorgänge. O. BUMKE. Wiesbaden, J. F. Bergmann, 1909. pp. 16. Price pf. 65.

A popular lecture delivered to the *Naturforschende Gesellschaft* of Freiburg. The writer first touches upon the pupillar reflex, and the expressive changes of pulse, respiration and volume; illustrations are given from Lehmann. All these movements are expressive of feeling or emotion; if they accompany attention or reflective thought, that is because all mental processes whatsoever are attended by feeling. He then turns to Sommer's tridimensional analysis of involuntary finger movements, which he uses to explain certain card-tricks and phenomena of thought-reading. From these it is natural to proceed to table-turning: the motor effect of a definitely directed expectation is illustrated by the pendulum experiment of Bacon and Chevreul, by the mistakes of the self-conscious performer and reciter, by the disasters of a first attempt at bicycle-riding; the surety of movement when there is no interference by expectation is shown in the trance-dancing of the well-known 'Madeleine.' Coming back to thought-reading, Dr. Bumke then outlines the results of Lehmann and Hansen on the unconscious whisper, and the story of the trick-horse Hans, with Pfungst's related experiments. He is doubtful of the promise of Veraguth's psychogalvanic reflex; partly because, like the pupillar reflex, it shows only one single form of reaction, without qualitative differentiation, partly because it is too delicate a test of disturbance of mental equilibrium. Finally, he discusses Berger's observations of the exposed brain, in order to gain light on the question whether the physical changes are co-ordinate with or subordinate to the corresponding mental processes. The brain changes precede the changes in other parts of the body, but are nevertheless themselves of a secondary or subordinate kind; the observations, therefore, tell us nothing of the intimate nature of psychophysical parallelism.

The lecture thus covers a good deal of interesting ground, and the exposition is in the main sound. There is some vacillation as to the mental antecedents of involuntary movement; the general teaching appears to be

that expressive movements are always expressive of feeling; but we are also told that the mere idea (*Idee*) that a movement may occur suffices to set the muscles in involuntary activity. The James-Lange theory of emotion is dismissed with the remark that it was never demonstrable and to-day is refuted: here the writer's logic seems to have gone astray, to say nothing of his psychology. The concluding paragraphs, on Berger's results and their connection with parallelsim, must have been unintelligible to the majority of the audience as they will be unintelligible to most readers of the lecture.

FRANCIS JONES

Sprache, Gesang und Körperhaltung: Handbuch zur Typenlehre Rutz.
 Von DR. OTTMAR RUTZ. München, O. Beck. 1911. pp. vi., 152.
 With plates and tables. Price Mk. 2.80.

All amateur singers have observed that there are certain songs which, though simple in composition and well within the compass of the voice, do not 'suit.' It seems that professional singers have the same experience. And about the year 1860, the German professional singer Joseph Rutz made the discovery that every song demands a very definite modality of voice, can be sung adequately only in one particular way. At first, he sought to find an explanation in the adjustment of larynx, mouth and throat; but repeated trial showed that the essential thing is the carriage of the body, the attitude of the trunk. Joseph Rutz died in 1895, without having committed his results to paper; but his wife and son—the son is the author of the present book, and of a work entitled *Neue Entdeckungen von der menschlichen Stimme*, which appeared in 1908—have worked further upon the subject, and have reached conclusions of great scientific and practical importance. Authorities of no less weight than Wundt and Sievers have given the *Typenlehre Rutz* their approval, and have started enquiry into the scientific aspects of the discovery.

Briefly stated, the thesis is this: that every mode of expression in tone and word—music, poetry, prose, oratory, letter-writing—presupposes in the individual a special bodily attitude, and can be reproduced only by an individual to whom the attitude in question is either natural or by practice familiar. Speech, song and the carriage of the body, are closely interrelated, and are one and all related further to certain fundamental tendencies of the life of mind, the temperamental tendencies that underlie mood and the other forms of affective reaction. Not, of course, that the principle of individuation must be pressed too far; the three great types distinguished by the author are national or racial types, the Italian, the German (which includes the English also), and the French; but these types have sub-types or sub-forms, which may be variously combined, and which may be differently displayed by a given individual at different times. It is important to remark that the Rutz types are exclusively types of feeling, not of character or of intelligence; if a man is able to shift from one type to another, as Schumann shifts from his naturally German type to the French in the *Two Grenadiers*, this is by virtue of a power of imitative or empathic feeling.

The theory of the matter has been set forth in Dr. Rutz' earlier works, in an article in Meumann's *Archiv*, and elsewhere. The present book is practical. It gives a list of the types, and of their sub-forms, with illustrative plates, and draws up rules for the student. It also gives (pp. 60-144) an alphabetical index of authors and musicians, classified by type. The mastery of the directions for the carriage of the body is, at least in the rough, by no means difficult, and the reader will be repaid if he spend a little time upon them, and then test them by extracts from the writers quoted. There can be no doubt that the Rutz types are real, and that the Rutz discovery is destined to play a large part in the psychology of expression. Had Thorndike taken these types into account, he could hardly have written so strongly against the multiple-type theory as he has done in the new edition of his *Educational Psychology*.

A. ISAACSON

Leitfaden der experimentellen Psychopathologie. Vorlesungen gehalten an der Universität Leipzig von Privatdozent Dr. A. GREGOR, Oberarzt der psychiatrisch-neurologischen Klinik, Leipzig. Berlin, S. Karger. 1910. Pp. x., 222. Price Mk. 5.60.

In 1900, Dr. Störring—at that time also a *Privatdozent* in the University of Leipzig, but a *Privatdozent* of Philosophy—published a volume of Lectures on Psychopathology, which has recently been translated into English. This work naturally comes to mind, as one opens the present volume; but a reading of Dr. Gregor's lectures shows that the intention of the authors is entirely different. Störring, it will be remembered, aims to bring out the significance of psychopathology for normal psychology, and outlines the psychological principles of a theory of knowledge. Gregor avails himself of the experimental method (we might say of Sommer's methods) in psychopathology, in order to obtain results that shall be useful for treatment, and in order, at the same time, to enhance the capacity of clinical observation and of diagnosis (p. 13). The older work views psychopathology from the standpoint of the psychologist; the newer views psychology and psychological method from the standpoint of the psychiatrist. In a certain broad sense, therefore, the two series of lectures are complementary, though the lapse of time by which they are separated, and the differences in the authors' training and attitude, make this relation partial and incomplete. At any rate, it is instructive to read the books together.

It may be said at once that Dr. Gregor is a worthy successor to Störring. He is already favorably known by the experimental studies which, alone or in collaboration, he has published since the year 1906, and in which he has applied psychological methods to the study of such functions as the appreciation of time, the apprehension of visual stimuli tachistoscopically exposed, the range of memory, etc., in cases of mental disorder, and notably in cases of what is called Korsakow's disease (a toxæmic neurosis, characterized by defects of associative memory, confusion with a marked tendency to confabulate and to indulge in pseudo-reminiscences, hallucinations and delusions, a marked fluctuation of the affective life, and oftentimes disturbance of function of peripheral nerves). The results of these studies, together with those of other investigators, are here brought together in a systematic way. An introductory lecture deals in general terms with the relation between psychology and psychiatry. Then follow lectures on the psychopathology of the time-sense, on reaction experiments, on visual perception (*Auffassung*, in the sense of Kraepelin and Cron), association (2), memory (2), the psychology of testimony (2), attention (2), the external voluntary action, the bodily expression of psychical states, the formal aspects of mental work, and tests of intelligence. In every instance the technique and results of normal experiments are first set forth, and then we have an account of method, as modified for application to the patient, and of the results so far obtained. The exposition is clear, and the author has a good knowledge of the normal work. In Lect. VI. he outlines his position to Freud's psychoanalytic method: the procedure is personal, and the material not altogether objective; nevertheless, if used with caution, psychoanalysis is a valuable instrument (p. 76). The view of attention taken in Lect. XI. is that of Dürr: the motor attitude of readiness for stimuli is a concomitant phenomenon only; the essential thing in attention is clear and definite apprehension of objects, vividness and compelling character of conscious contents (p. 137).

The book has two external defects which call for notice. In the first place there is no index. In the second, there is neither a paged table of contents nor any sort of page heading! The consequence is, that if one wants to find, say, the experiments on testimony made with abnormal subjects, one has first to look through the table of contents; there one discovers that Lect. X. is the place required; and then one has to turn the pages of

the book at random, till one happens to strike the title *Zehnte Vorlesung* on p. 123. Why the reader should be exposed to these indignities, only a German publisher could explain. A useful bibliography (pp. 215-222) is not mentioned in the table of contents.

W. ASHER

The Dweller on the Threshold, by ROBERT HICHENS. New York, The Century Co. 1911. pp. 273. Price \$1.10 net.

It is seldom that a psychologist is called upon to review the Latest Novel. The present reviewer has read and enjoyed other works by Mr. Hichens,—*The Garden of Allah*, and *Bella Donna*; this newer work he has read without enjoyment.

The story has to do principally with the Rector of a London parish and his senior curate. At the beginning of their relationship, before the narrative opens, these men stand in sharp contrast: the Rector is talented, ambitious, self-confident, the Curate is industrious, dutiful, humble-minded. On the other hand, the Rector is troubled by sceptical doubts, and is betrayed by grossness of fibre into occasional lapses from right-doing, while the Curate, amiable and easily led as he is, has at any rate the strength that comes from an unshaken faith and personal purity of living. The Rector now conceives the idea of using the Curate as a medium whereby he may obtain communications from the spirit world; he thus satisfies his lust of power, and at the same time hopes to settle his religious doubts. The Curate, however, has to be inveigled into 'sitting'; and the Rector gains his point by the lying assurance that the whole object of the proceedings is to strengthen the Curate's will, to inspire him with something of the mental power that he admittedly lacks and that he admires in his superior. So the sittings begin. But the Rector fails after all, to 'entrance' his weaker-minded colleague,—who, on his side, feels himself strengthened in the manner promised. And so it presently comes to pass that the Curate is the dominant and strong-willed, the Rector the dominated and suggestible member of the duo; the parts have been reversed or exchanged. But here is the mysterious consequence: the Rector remains consciously what he was, the Rector, only that he is now a weakling, aware of his weakness and trending steadily down hill; the Curate, who has sucked the Rector's strength from him, becomes a dual personality, in whom the original Rector predominates and the Curate is entirely subordinate. In other words, the Curate henceforth is the 'double' of the Rector, knows and feels himself to be in the main identical with the Rector, while his own curate's nature remains largely in abeyance, though it is not wholly lost; he therefore watches the Rector, fears on his behalf, suffers with and for him, seeks to guide or direct him, precisely as a man would act and suffer in his own interest; and the Rector, harassed by this perpetual scrutiny, this ever-present influence to which he must yield while he fails to understand it, breaks down with a completeness that ends in death. The Curate, as the watchful and critical double, is thus—as one may suppose—the 'dweller' on the Rector's 'threshold.' The Rector's death dissolves the bond between the two men; the Curate reverts at once to his original, sequential state; sincerely mourns the loss of his hero; has no memory of the insight into the Rector's character and motives that he gained from the sittings; and loses, once and for all, the foreign personality that had well nigh ousted his proper nature.

That is the story. The remaining persons of the drama are a Professor who, in the quest of scientific fact, devotes himself to psychical research, and whose watchwords seem, hitherto, to have been telepathy and nervous dyspepsia; a Gentleman of Independent Means, who is somewhat more human than the Professor, but shows a like devotion, and has worked under the Professor's direction; and the Rector's Wife, a lady whose fate it is to worship at the shrine of masculine success, and who therefore, after an interlude of keen dislike of the Curate, definitely transfers her admiration

from her broken husband to his masterful coadjutor. The Professor is wont to refer to this lady as the Link, though her connective office is not clear; the confessions of the Rector to the Independent Gentleman, and of the Curate to the Professor, make the story plain enough without her.

More interesting than the novel itself is the psychology of its author. Why did he write it? To point the moral that, if we could but see ourselves with a perfect vision, we should be horrified at the revelation? But that is trite morality; and most readers, it may be assumed, will compare themselves favorably with the Reverend Marcus Harding. To plead the cause of psychical research, on the ground that there are more things in heaven and earth than science dreams of? But an imaginative tale will not convince any who are not convinced already. As a *tour de force*, to prove that the modern novelist can make plausible use of the 'supernatural'? Perhaps: the title seems to point to some such intention. But then—plausibility is a relative matter, and the book should not be sent for review to a psychologist.

WM. ERSKINE

The Evolution of Mind. By JOSEPH McCABE. London, A. & C. Black. New York, The Macmillan Co., 1910. pp. xvii., 287.

In this fluently—at times brilliantly—written essay, Mr. McCabe seeks to solve the cosmic problem of the birth and development of mind. It is usual, he tells us, to postulate two evolutionary series: the material, where "all varieties of energy and matter arise out of the abysmal womb of ether," and the mental, which "set in when the earth reached a certain stage of its development." Is this dualism tenable? When, and in what form, did consciousness first appear? Can mind be brought into the cosmic unity by tracing its gradual emergence from the etheric matrix? These are the questions which the present work essays to answer.

All living matter, whether plant or animal, shows, when it has freedom of movement, two properties which we may, if we will, term 'mental' or 'psychical': namely, sensitiveness or irritability or responsiveness to stimulation, and spontaneous or self-initiated movement. But sensitiveness is also a widespread attribute of inorganic matter; and spontaneous movement always turns out, on careful scrutiny, to be a response to environmental stimuli. Here, therefore, is no evidence of consciousness; if we speak of 'mind' at all, we are stripping the word of the distinctive significance that it has in our own experience. What we are looking for is proof of consciousness.

But what, then, the reader may ask, is consciousness? "I make no attempt to define consciousness," replies our author, "partly because it defines itself more clearly than words can do, partly because all attempts to define it have proved abortive." Nevertheless, he knows very well what he is in search of. "The question to be answered is not, can we find any actions in a lower animal which are consistent with a theory of consciousness, but can we find any which are inconsistent with a purely neural action. The question of consciousness does not arise till then." "What I am chiefly seeking to determine is whether a new reality, or agency, besides ether, intervenes at some point in the earth's story." "The plain purpose I have in view is to see whether, and when, a new reality, other than ether and its products or aspects, enters into the tissue of our planetary life." And so he works up the scale of organic evolution, and reaches one negative conclusion after another. "There is no proof that consciousness had appeared before the Devonian period, or has since developed in any of the modern representatives of Pre-Devonian animals." "We have no clear or cogent indication of conscious states in the whole invertebrate world, or in any type of animal that lived before the Permian revolution in the earth's history." "We have not found a single pre-Tertiary animal whose activities cannot be explained without an assump-

tion of consciousness." The whole history so far is a history of the progress of mechanism.

Of course it is! But then, so is the subsequent history, that of man included. Mr. McCabe is the victim of a false antithesis. The opposite of conscious is unconscious,—not mechanical or neural; the opposite of reflex or automatic is complex or delayed,—not conscious. And this criticism holds, on any definition of 'consciousness.' "Inferences from external manifestations are precarious," says our author. One may reply that, on his own principles, they are impossible; for external manifestations can never give evidence of a new reality, different from ether; they must, just because they are external, belong wholly and solely to the material sphere. The most highly deliberative action of the civilized adult, no less than the simplest tropism of the unicellular organism, must be explicable, as an 'etheric' derivative, in physico-chemical terms. The logic of this issue is so elementary, and the point has been so often made, that one wonders to find Mr. McCabe the victim of the pitfall.

But now, for Mr. McCabe as for all of us, human consciousness is a brute fact, and all the ether in the universe cannot away with it. What is to be done? "I submit that the only way to come to any conclusion is to compare the organ of consciousness in ourselves with the presumed organ" in other creatures. The criterion, therefore, is to be external, after all! The fishes have an archipallium; possibly then, a dull glow of consciousness may accompany their activities; but its presence is disputable, and its nature (if it is present) must be insubstantial. The part of the brain which in higher animals is associated with consciousness is in the amphibia and reptiles extremely small, and cannot with any confidence be regarded as an index of consciousness. In the birds, the cerebral hemispheres have at last gained conspicuously on the other parts of the brain; here, accordingly, we may assume some consciousness, though its degree must remain unknown. Wherewith we pass to the mammals, and are on firmer ground.

And consciousness itself? "It seems to me quite hopeless to speculate on the origin of consciousness, so long as its organ is so wrapped in obscurity. And precisely for the same reason I decline to see in it the emergence or accession of a new reality, other than ether, or ether-compacted nerve. Until we know the cortex sufficiently well to say that its structure throws no light on the nature of consciousness, the question must be left open. At present, our knowledge of the cortex, the most transcendently important thing that science approaches, is appallingly meagre." "It is the most reprehensible dogmatism to say that consciousness may not have arisen in, and be a function of it." "Any further discussion of the point would take us into metaphysical considerations." Apparently it would,—if we are not in the realm of such considerations already.

Here the general argument ends. It is clear that Mr. McCabe is at least under obligation to distinguish between the two current uses of the term 'consciousness;' to say whether, for him, it means 'awareness' or whether it is identical with 'mind' or 'mentality' at large. It is clear, too, that he has not fully understood his authorities: he is not at home with Wundt's theory of the instinct, or with Thorndike's doctrine of free ideas; and, indeed, his conception of modern psychology seems to be that of an objective body of arguments rather than that of a group of empirical and subjectively verifiable observations. These, however, are minor points: the great fault of the essay is the logical fallacy which we have noted above.

The two concluding chapters, on the Dawn of Humanity and the Advancement of Mind in Civilization, presuppose the appearance of consciousness, and need not be discussed. They, like all the book, are written with a nerve and swing that fascinate the reader. Mr. McCabe has an unusually wide range of knowledge, and a delightful style; it is a pity that his great powers of popularization are not exercised by a more logical mind.

P. E. WINTER

Ueber den Traum: experimental-psychologische Untersuchungen. Von Dr. J. MOURLY VOLD, weil. Professor an der Universität Kristiania. Herausgegeben von O. Klemm, Privatdozent an der Universität Leipzig. Erster Band, Leipzig, J. A. Barth, 1910. Pp. xiii., 435. With portrait of the author. Price Mk. 11.

John Mourly Vold, professor of philosophy in the university of Kristiania, died in 1907, at the age of fifty-seven. For twenty-five years, Mourly Vold had been engaged in the observation of dream phenomena; some of his results are published in articles, in the *Revue de l' Hypnotisme et de la Psychologie*, 1896, in the *Zeitschrift für Psychiatrie*, 1900, and especially in the *Zeitschrift für Psychologie*, 1897. He left, at his death, besides a large body of scattered notes, the manuscript (in German) of a large work on the experimental study of dreams; the first volume of this work, edited by Dr. Klemm, lies before us. Dr. Klemm has, no doubt, been well-advised to print the manuscript as it stood; the author's German, though not always idiomatic, is always intelligible; and while the book might have been very considerably condensed, the reader would have missed, in the abbreviated form, a valuable lesson in scientific method. On the other hand, the editor should, by all means, have supplied an index. It is true that index-making is mechanical work; but then it is also mechanical work of the expert kind; and if an editor shrinks from it, he should decline outright the editorial duties.

The book is, as we have said, a valuable lesson in scientific method. The author takes us painfully and point by point through all the difficulties of dream-observation; shows us all the successive improvements in his own manner of working; and discusses in all detail the interpretation of dreams experimentally aroused or influenced. His method is, in every sense, comparative: he compares the experimental dreams with dreams of the preceding normal night; he gathers the dreams of a large number of practised and unpractised observers,—of several hundred university students and school teachers (both sexes); he has, to fall back upon, a personal experience that is probably unequalled; he does not neglect the comparison of the dream with the waking state. The first 58 pages, on method, should be read by advanced students of psychology, whatever their interest; only the specialist, we fear, will read critically through the remainder of the book; more particularly now that Freud's interpretation of the dream-consciousness has turned inquiry in a different direction. Yet the remainder, though it fill nearly 400 octavo pages, is well worth reading.

The experimental dreams here reported are but a fraction of the whole: they are dreams whose peripheral motive consisted in a cutaneous-muscular stimulation of the lower extremities, more especially of the ankles. The first of the two principal chapters gives an account of experiments in which a band is placed about the left ankle, and remains in position all night; the resulting dreams are compared with those of the preceding normal night. In other experiments, a band is tied around sole and instep, as well as about the ankle; in yet others, the foot is encased in a sock. Both types of experiments are, in one case, carried to the point of habituation. The second principal chapter gives an account of experiments in which the two ankles are separately bound, either for the whole night or for the evening only. In other experiments, the two ankles are not only separately bound, but are also tied together by a third band. The resulting dreams are compared with those of the normal nights, and also with those reported in the preceding chapter. A promised Appendix, on the part played by the hip joint (p. 216), is not mentioned in the table of contents and does not appear in the text.

The net result of the investigation can best be shown in the form of a Table.

- I. Ideas of *pressure* and *temperature* are aroused only in slight measure.
- II. *Motor* ideas are aroused most commonly.
 1. a. Free active movements of high intensity are the specific result of the stimulus, and outweigh in number all the other motor ideas put together.
 - b. Parallel movements of the feet cannot with certainty be brought into connection with the stimulus.
 2. The same thing holds of inhibited movement.
 3. Static conditions show a clear causal relation to the stimulus; they are by no means intensive, yet stand in number next after the free active movements.
 4. Passive movements of the whole body cannot with certainty be related to the stimulus.
 5. Motor objects and
 6. Abstract motives to movement are causally related to the stimulus, though they are dream-factors that rarely appear in isolation.
- III.
 1. Ideas coincident in time with the experiment or with the discussion of it are aroused only in slight measure.
 2. The unpleasurable common sensations cannot with certainty be attributed to the operation of the stimulus alone; the free active movements were accompanied by a pleasurable organic complex.

This meagre statement of the outcome must here suffice. We may expect, in the second volume, a full theoretical discussion of the dream-consciousness; meanwhile, the remarks made on pp. 9 f., 416 ff., are significant. The questionnaire used by the author is printed on pp. 31 ff. J. FIELD

Parenthood and Race Culture; An Outline of Eugenics. CALEB W. SALEEBY. Moffat, Yard and Company, New York, 1909. Pp. xv, 389.

This book lays claim to the distinction of being the first to survey the whole field of eugenics. The author states in the preface that there is need to-day of a "general introduction to eugenics which is at least responsible;" and adds that he is "indebted to more than one pair of searching and illustrious eyes, . . . for reading the proofs of this volume." The present discussion, it would seem, is a continuation of the author's previous campaign of advocacy. Further, the book is to be regarded as an exposition, not as a contribution of original material. Dr. Saleeby seeks to review and arrange the results of Galton and of the other investigators; still, the author is himself a man of opinions, and he devotes much space to his own particular crotchets. Unfortunately the book is swelled by some turgid writing. It stands in this regard in strong contrast with Galton's own condensed, close-knit manner of utterance. Had the present volume been boiled down to half its size, its effectiveness would have been doubled.

The contents are divided into two parts: "The Theory of Eugenics" (Part I) and "The Practice of Eugenics" (Part II). Part II falls into two complementary themes, "negative" and "positive" eugenics;—perhaps "restrictive" and "constructive" would have been better terms. Negative eugenics, as the author defines it, seeks to discourage the parenthood of the least desirable. Positive eugenics is the effort to encourage parenthood on the part of the most desirable.

Only one of his chief tasks does Dr. Saleeby perform with thoroughness. That task is destructive. Errors and illusions are mercilessly slaughtered. On page 28, the author sweeps the Nietzschean view of selection off the boards. The superstition of maternal impressions is quashed on page 128. Farther along Mr. Bernard Shaw's erratic proposals for a stud-farm to be devoted to race-culture are dispatched. It is shown in Chapter X that eugenics does not propose a destruction of the family; that it endorses, indeed, exalts monogamy. And so throughout. The tone of the book is

distinctly controversial. Unfortunately the author over-reaches himself. He is too combative, lacking the calm temper of the scientist. He assumes ignorance, misconception and indifference in his audience; and the consequent attitude of defiance is at times unpleasant.

Eugenic endeavor, declares Dr. Saleeby, centres about "selection for parenthood." Parenthood the unfit must be denied. The lowering of the death-rate among infants (and adults as well) tends to keep alive until the reproductive age many inherently weak constitutions which reduce the average vitality of the stock; this fact emphasizes the need of man's further interference with the processes of selection.

The Chapters on "Heredity and Race Culture" and "Education and Race Culture" define the relative importance of nature and nurture and demonstrate the need of progressive improvement of the germ-plasm. The section on "Lines of Eugenic Education" is excellent, although it should be transposed to Part II of the book. In his discussion of terminology, Dr. Saleeby appears to be trying to clear up his own ideas, and, on the whole, he succeeds. Yet it is rather curious, after his demand that "conceptional" be substituted for "congenital," to come across the word "congenital" (p. 201, near bottom) used in the very sense which the writer had before violently repudiated.

The author's proposals are invariably mild. He desires no revolution of moral or marital relations. Motherlove, he thinks, should have survival-value in the minds of eugenists to the same degree as physique, ability and character. "I confess myself opposed to the principle of bribing a woman to become a mother, whether in the guise of State-aid or in the form of eugenic premiums for maternity." Equally repugnant are the German projects for a "eugenic" universal polygamy and polyandry (echoes of Plato!) and Chesterton's definition of eugenics: "that people should be forcibly married to each other by the police." Monogamic marriage has survived and become dominant because of its supreme services to motherhood, and hence to the race. The conclusion is that the best form of sex-relation secures the *common parental care* of the offspring; the support of motherhood by fatherhood.

Society must prevent propagation of the criminal, the insane, the epileptic, and the feeble-minded. Means to this end, however, the author leaves undefined. Permanent detention is mentioned; surgery rejected. Although Dr. Saleeby has enormous faith in the 'power of public opinion,' he puts little trust in the formal embodiment of it—legislation.

In general, it is true that acquired characters, or modifications, are not inherited. A few virulent diseases and substances, however, sink deeply enough into the bodily constitution to damage the germ-plasm. In such cases the offspring suffer. The more common of these "racial poisons," as Dr. Saleeby names them, are alcohol, lead, narcotics and syphilis. The discussion of racial poisons, though inexcusably prolix, constitutes one of the most original contributions to eugenic literature in the book.

Nowhere does Dr. Saleeby speak out positively enough for constructive eugenics. He says rather lukewarmly, "positive eugenics must largely take the form, at present, of removing such disabilities as now weigh upon the desirable members of the community, especially the more prudent sort." Surely this is not the utterance of a soldier in the "moral crusade" for children which Professor Karl Pearson emphasizes. In this connection a line from Galton is apropos: "The possibility of improving the race or a nation depends on the power of increasing the productivity of the best stock. This is far more important than that of repressing the productivity of the worst." The present book disagrees with Galton, not overtly but implicitly.

Eugenics, for Dr. Saleeby, is the final arbiter of all disputes. He cares not whether a "proposal is socialistic, individualistic, or anything else" so long as it is eugenic. "When by means of eugenics we give education the

right materials to work upon we shall have a Utopia, and as for forms of government they may be left for fools to contest." Here we have the ardor of the reformer, bordering on fanaticism! Eugenics the only salvation! This kind of enthusiasm seems to be responsible for many of the faults of the book.

The volume is, without doubt, suitable for popular consumption. If it is verbose, it is, in the main, clear. If it hammers and scolds, it meets enough opposition and inertia to justify its censoriousness. If certain details are questionable, the main outline is reliable. It will help, not hurt, the eugenic propaganda. Nevertheless, it cannot be regarded as a definitive exposition of eugenics as that science at present stands. It should be superseded before long by a far abler treatise. C. R. HUGINS.

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The Science of Poetry and the Philosophy of Language, by HUDSON MAXIM.
New York, Funk & Wagnalls Company, 1910. pp. XIII + 294.

The core of this book is that language both *expresses* and *impresses* thought (p. 84). Thought may be so abstract that it cannot be expressed except in the most literal way; again, it can be figuratively expressed. The latter gives poetry (Chap. III, IV). Poetry is non-emotional (Chap. III, IV); it is also what separates man from the brute (Chap. II). Language impresses thought, on the other hand, by the moving power of sound. Here is the source of all emotion in letters (Chap. V). This power can be assigned largely to the four properties of sound, each of which is connected with a specific phase of emotion and is traced by analysis to a physiological process (Chap. I, VI, VII); indeed, everything mental is physiological (Chap. I). This impressiveness is given the name *potentry* (Chap. V), a word that carries in its train a new nomenclature for all varieties of linguistic arts, to a treatment of which later chapters are devoted. From the tone of the whole book, one judges that science is the panacea for all mysticism; those who see anything mysterious in poetry are belabored right and left with much ridicule.

In its foundation principles the book is dogmatically materialistic. Surely much may be said against consciousness being a physiological process merely. And while it is true that "consciousness is the sense of awareness of the other senses" (p. 1), it is also aware of more than the psychological elements into which it can be resolved. This principle holds with the analysis of all compounds; and the failure to see it gives a false tone to the whole book. No one will deny that "there is a science of poetry" (p. 44), but there is something in poetry which eludes us if we analyze it scientifically. Let men try to tell just what any familiar substance really is; their statements will be as mysterious as the definitions of poetry criticised by Mr. Maxim from the standpoint of science. The quarrel then is not with those who find a touch of mystery in poetry, but with those who, taking their own restricted view of experience for the whole of it, refuse to countenance the revelations of that experience from any other viewpoint. There has been no "coalition against the scientific investigation of poetry" (p. 191), but Mr. Maxim does not see the significance of admitting (p. 44) Coleridge's claim, that poetry is the antithesis of science. For science seeks the relations of experience apart from subjectivity, is objective; poetry—an art—expresses experience linked with life, is subjective. One's attitude to bread when he is hungry (p. 66) is quite different from his attitude of curiosity as to the chemical constituents of bread; the latter gives us science, is intellectual; the other gives us art, is emotional. Poetry is a form of art. *The fundamental unsoundness of the whole book then in its treatment of poetry is evident in the statement that "as we go away from the emotions and in the direction of thought at the expense of emotion . . . the more poetry we get"* (p. 66); this is in the direction of science and gives us, not poetry, but mathematics—the multiplication table. This antithe-

sis of poetry and science gives to many of the statements about poetry quoted in the book under review a relevancy not to be broken by the cheap playing to gullibility that characterizes much of Mr. Maxim's criticism of them. (See, for instance, Chapter IV, bottom of page 51.) Poetry is not then the expression of mere ideas (p. 91). The use of trope itself implies a heightened idea. The source of the moving power of any poetry worth the name must lie primarily in a certain enthusiasm of the personality. Remove this and poetry is gone. It was this ardor of life the Greeks symbolized in Pegasus; Mr. Maxim has tamed Pegasus, but he has killed poetry.

The theory that language both expresses and impresses thought is not after all so brand-new. Men in reality have taken this for granted. Every idea has some feeling tone,—every sound some power to attract the hearer. This is admitted (p. 80). Moreover, ideas assume naturally the form best fitted both to express and impress themselves, giving us the poles of science and art according as we emphasize intellect or personality. Looked at from the standpoint of its impressiveness, language has long been known as the pleasure-giving art of letters. In this pleasingness lies what Mr. Maxim calls the impressive power of language,—“the conversion of energy into pleasurable emotions, which serve to energize perceptions (p. 79).” Where then is the need for coining the word *potentry* and all that rignarole of nomenclature that depends on it? Better to have called a much needed attention to the practical value of art.

The valuable part of the book discusses this impressive power of language (Chap. V, VII, VIII). The four properties of sound—loudness, duration, pitch and timbre—are linked with specific phases of emotion; loudness and duration with importance; pitch with intensity; and timbre with pleasure and pain (Chap. VII). The theory is suggestive, but lacks—especially that timbre expresses pleasure and pain—scientific confirmation. Spencer is followed largely, but Chap. V links to his principle of economy the need for added expenditure of energy in impressiveness of utterance. Spencer says the vocal apparatus should be simple and do its work with the least expenditure of energy. Maxim would say, Use economically as much energy as you like, so long as it produces pleasurable emotions with the thought conveyed. Both practice economy; Maxim does additional business in another field. This is very important and valuable, but Mr. Maxim does not see (1) that he is here in the province of art as opposed to science, and (2) that the principle of economy is here applied only in the very loosest way, if at all. Science looks at the apple and finds economy. Think though of the blossoms that do not fructify. This, too, is nature. What economy of energy is in the play of a healthy boy? Or in his work either, if he enjoy it? Nature plays and works, better plays in work. Art in literature corresponds to this play. In giving Macbeth six lines to ask the doctor a question easily expressed in six words, the artist reveals a healthy natural indifference to any law of mere parsimony in speech. In the utterance of science there may be economy of energy; a certain generosity characterizes the larger utterance of art.

Chap. VII also gives a theory of rhythm, which Spencer did not attempt. Rhythm is the ebb and flow of nerve impulse according as muscles are contracted or relaxed. “The beats of the verse are in harmony with the beats of the nerve spasms which the nerve potential of passion tends to induce” (p. 147). “Under emotion, then, vocal phenomena must necessarily be rhythmical.” The theory is not conclusive. No reason is given for the fact that the tension and relaxation of muscles is regular in poetical rhythm and irregular in that of prose. How account, moreover, for the rhythm of the wheels on the rail joints to the unmoved passenger? The theory also implies an unsatisfactory explanation of *time*. The feelings of muscular contraction and relaxation are themselves *in time*; to conclude

then that these are the data of our feeling of time seems to be begging the question.

The book contributes nothing to the problem of the origin of language. In taking the position that the use of trope and not articulate language separates man from the animals (Chapter II), it seems to be using *articulate* in the sense of *uttered, spoken*; but what men mean by articulate speech when they deny it to animals is that orderly grouping of words corresponding to ideas articulated logically so as to produce an intended end. It is true animals do not use metaphor; it is almost equally evident they do not form concepts,—the first requisite in reasoning. Nor is the discussion of the development of speech in the race in the least fruitful. The development of the child linguistically contributes little to our knowledge of that general development; for there is no meaning to the babblings of an infant until the mother has by gestures or in other ways aroused an association in the child's mind between certain sounds and certain objects. The statement, "Every mother in the world, of whatever race, can understand the baby talk of any child of the race" (p. 20) is in its extravagance typical of the book. Much less space, indeed, might have expressed all that is valuable in it either as science or as poetry.

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- (1) *Les dégénérescences auditives*. Par A. MARIE. 1909. pp. 111.
- (2) *Rééducation physique et psychique*. Par H. LAVRAND. 1909. pp. 123.
- (3) *Les folies à éclipse*. Par LEGRAIN. 1910. pp. 120.
- (4), (5) *Les rêves et leur interprétation*. Par P. MEUNIER et R. MASSELOU. 1910. pp. 213.
- (6) *La suggestion et ses limites*. Par BAJENOFF et OSSIPOFF. 1911. pp. 119.
- (7), (8) *La psychologie de l'attention*. Par N. VASCHIDE et R. MEUNIER. 1910. pp. 199.

These six volumes form nos. 12-19 of the *Bibliothèque de Psychologie expérimentale et de Métapsychie*, edited by Dr. Raymond Meunier and issued by the Librairie Bloud et Cie of Paris.

(1) Dr. Marie, senior physician at the Asile de Villejuif, published in 1908 (as no. 3 of the present series) a little book entitled *L'Audition morbide*, in which he briefly discussed the pathological physiology of hearing in cases of mental and nervous disease. The work before us is concerned with the principal anatomical anomalies of the peripheral or central auditory apparatus. After a general introduction, treating of the difficulties of diagnosis, the diagnostic value of symptoms, etc., the author takes up in order, from without inwards, the various divisions of the auditory mechanism. To the chapter on the external and middle ear he contributes a table of auricular measurements, with their craniological complements. The chapter on the internal ear is sketchy; in particular, the problem of heredity should have been approached in the light of the Mendelian hypothesis. In the chapter on central lesion and cortical hearing, the author quotes, apparently with approval, the opinion of Dr. P. Marie that isolated sensory aphasia, and especially pure verbal deafness, does not occur. He here describes a case (with autopsy) of dementia with motor verbal aphasia, agraphia to dictation, and verbal deafness. A final chapter deals with arrest of auditory development, physical and mental. Dr. Marie insists strongly on the necessity of a precise diagnosis of the cause of deaf-mutism, and pleads for systematic education of such patients as are educable. It seems clear that the appeal to public sentiment made in this chapter was the author's chief motive in writing the book.

(2) According to Dr. Lavrand, who is professor at Lille, mind and body are not separate and separable phenomena, but constitute a 'substantial unity'; mind therefore acts upon body, body upon mind. There is, indeed, a constant interaction among all organic functions, the conscious included;

and since the symptoms of functional disturbance far outrun, in most cases, the actual lesion of the organism, there is good hope of a successful re-education. The model for this is, of course, given with the primary process, that of education; the author accordingly outlines the genesis of ideas in the child, the growth in complexity of bodily movements, and the co-function of ideas and movements in what he calls psychomotor acts; he finally formulates the end of education as the transformation of conscious and attentively executed actions into subconscious or automatic activity. Passing from theory to practice, he first takes up the question of mental reeducation, *i. e.*, of the effect of psychotherapeutics upon various forms of mental disorder, from hysteria down to a practically normal psychasthenia. Next follows a chapter on motor reeducation,—locomotor ataxia, paralysis, tics, speech derangements, aphasia, deaf-mutism,—which contains much empirical material, plainly of the writer's own observation. Dr. Lavrand then proceeds to discuss sensory reeducation, with special reference to Rousselot's method of treating deafness; organic reeducation, with reference to imaginary dyspepsia; respiratory reeducation, with reference to the alleviation of asthma; and ends with a brief mention of circulatory reeducation, and of the reeducation (or rather education) of the idiot by Bourneville's method.

(3) The third volume on our list, from the pen of Dr. Legrain, senior physician of the hospitals for the insane in the Department of Seine, is mainly taken up with the symptomatology of what the author terms eclipsed insanities. There is, says, Dr. Legrain, between the conscious and the unconscious, a wide region of subconsciousness; it is manifest in the phenomena of instinct and habit, and covers the whole field of the forgotten. This subconsciousness is not inactive; it has its own life and activity, even though it does not come to consciousness. Wherever, now, there is mental disorder involving hallucinatory experiences, the contents of the hallucination may disappear into the subconscious; the patient is then not cured, though he is free of the obsessing ideas; the hallucinations are under eclipse, but may emerge again. (A second volume is to be devoted to this fact of resuscitation.) The impermanence of the cure is favored by general mental weakness; and the condition of eclipse is evidenced by the fact that the hallucinations are accepted by the apparently normal patient as real items of past experience. In conclusion, the author recommends to experimental psychologists the study of the hallucinatory idea, especially under the headings of strength and duration of impression.

(4), (5) The work of Drs. Meunier and Masselon, on dreams and their interpretations, opens with a psychological analysis, couched in general terms, of the nature and sources of the dream-consciousness. The writers regard all dreams, except those that appear in the hypnagogic state just before or after sleep, as in some measure pathological. They attach special importance to dreams of coenæsthetic or organic origin, which are of two kinds: in the one case, the organic derangement is clearly localized, and the dream-images bear directly upon it; in the other, the organic state is intellectualized by way of a diffuse emotion, and the dream-images are emotively suggested. These dreams constitute "a veritable microscope of sensibility; they throw into relief slight disturbances that escape the notice of the waking consciousness." A review of dreams in general pathology, in infections and intoxications, in neurotic conditions, and in the various forms and stages of insanity—this review makes up the body of the book—shows, in fact, that they may reveal a functional disturbance which is not apparent in the waking life, and which may be the indication either of some organic disease or of a hitherto latent mode of mental disequilibrium. Dreams are thus a touchstone of the stability of the psychophysical organism. If they are ordinarily neglected, in prognosis, this is only because they are considered too delicate and too variable a reagent for the physician's purpose. In fact, however, there are certain

characters that make them available. There is first of all distress, especially when intense (as terror) and sharply localized (as physical pain); this may lead to the sleeper's actual arousal in the middle of the night; and the arousal, if really due to the distress, is unquestionably a pathological symptom. The homogeneity of the dream, shown perhaps in the recurrence of leading motives, is also evidence of the persistence of the causal substrate. The stereotyped dream attests the presence of an identical cause, organic or psychic, which exerts its influence at recurrent intervals of time. (The writers emphasize the importance of this phenomenon of stereotypism of dreams, and devote a special chapter to its consideration.) The fact that a dream is remembered on waking is also significant. The contents of the dream, finally, must always be taken into account. No one of these characters, it is true, stamps the condition of the dreamer as at all gravely pathological; but each and all of them point to an anomaly, to a nervous susceptibility, and so suggest a closer study of the patient's bodily and mental state. In a word, dreams have a very real prognostic value; but they are indicative only, and not demonstrative; and the indication should not be acted upon till it has been confirmed by other and more technical methods of examination.

(6) Professor Bajenoff and Dr. Ossipoff, leading alienists of Moscow, write upon the facts and theories of suggestion. The six chapters which make up the book, entitled respectively the history of hypnosis and suggestion, psychological automatism, hypnotism and suggestion, collective suggestion, current theories of therapeutic theory, and the psychological mechanism of suggestion, contain little more than a brief résumé of the work done and the views expressed by other investigators; but the authors command a clear and vivid style, and have the happy knack of literary illustration,—as when they draw upon certain of Tolstoi's characters to exemplify the procedure of psychotherapy. The central aim of the book is the divorcement of suggestion from hypnosis. Suggestion is not the essential characteristic of hypnosis; it may be exerted more effectually in the waking state (p. 11). It is ordinarily supposed that the subject in profound hypnosis is peculiarly liable to suggestion; "this opinion is absolutely erroneous" (p. 36). Hypnotic suggestion has its definite limits (pp. 39 ff., 114); while suggestibility itself is a psychophysiological phenomenon of practically universal occurrence (pp. 58, 112). The mechanism of suggestion is described, schematically, as "the disaggregation of psychical activity, the rupture of the normal co-ordination and subordination of the elements of the mental life, and, as a result, the more or less complete dissociation of the personality" (pp. 35, 113).

(7), (8) *The Psychology of Attention*, written in collaboration with the late Dr. Vaschide by Dr. R. Meunier, psychopathologist at the Asile de Villejuif, is a companion volume to the *Pathology of Attention* (no. 5 of this series) published by the same authors in 1908. It is not a text-book in the psychology of attention; the writers' intention is at once narrower and wider than that of the compiler; they present, first, a report of carefully selected experimental data, and on the basis of this approved material they rise to an inclusive theory of attention in dynamic terms. The first two chapters discuss the technique of the study of attention and the results of experimental investigation; the reader—unless he recall the contents of the previous volume—will be surprised to find that the great bulk of the space is given to the reaction experiment. However, the complete programme of an experimental enquiry would cover, in the authors' judgment, ten tests; those of cutaneous sensitivity, of muscular strength voluntarily exerted, of speed of movement, of voluntary attention (cancelling letters, discrimination of forms applied to wrist or palm, grasp of the sense of a printed page by a rapid glance over it), of color-vision and extent of the visual field, of audition and extent of the auditory field, of rapidity of thought (reaction experiment in various forms), of memory of words and

figures, of mental arithmetic, and of association of ideas. "The examination of a subject by means of these ten experimental series will show that it is possible to catch the attention at work, to seize its dynamic character. And theoretical conceptions will in so far be modified" (p. 61). It seems to the reviewer that some of the tests would require a great deal of psychological interpretation before they could be turned to account for the characterization of attention; at all events, the writers do not justify their statement. Chapter 3, on attention during sleep, reports an experimental study (made by Vaschide) of the ability to wake at a set time in the morning. Out of 40 chosen subjects, of different sex, age (20 to 76), occupation, education and nationality, 33 proved available for the test. The tendency was to wake too early; the amount of error, for 26 subjects, was in rough average 21 minutes; the error might, however, be as great as an hour and a half, and might reduce to 12 seconds. The chapter gives many interesting facts, objective and introspective, but offers no connected theory of the phenomenon. Ch. 4, on suggestibility and attention, reports Binet's experiments on the suggestibility of school-children (lines, weights), and concludes that, while there is no direct relation between suggestibility and attention, suggestibility may be considered as a state of emotive disturbance, the first effect of which is a disturbance of attention. Ch. 5, on hypnosis and attention, is mainly occupied with an account of Beaunis' well-known experiments. No theory of hypnosis is at present possible; it is, however, characterized rather by paraprosopxia than by hyperprosexia, —that is, if the reviewer understands these terms, rather by diversion of attention than by extreme concentration of attention,—and by a high development of the 'forces of automatic attention.' Ch. 6 reviews and criticises the prevailing theories of attention, under the rubrics peripheral, motor, affective (Ribot, Bain), and sensory, voluntaristic, perceptive, central (Marillier, Kreibig, Rageot, Nayrac). The authors conclude that attention is intimately related to emotion; that it is a phenomenon of central origin; and that it is essentially a dynamic function. "It is to the intellect what reflex irritability is to the nervous system; it is not a state, but an act." Let us hope that they find this conclusion satisfactory!—

As, now, we glance back over this series of books, we realize that, while they leave much to be desired on the score of systematic presentation, they are none the less readable and valuable, since every writer has some personal contribution to make to the existing stock of knowledge. A good part of the contents strikes the reader as perfunctory; but there is always some central chapter which brings new material or original ideas. Whether, under these circumstances, it is worth while to publish books rather than special articles is, perhaps, a question of taste; the reviewer, for his part, would prefer to dispense with the second-hand discussions.

The proof-reading is usually poor. The punctuation is erratic; the line-divisions show such monstrosities as *ins-upportable*, *o-bservation*; and names are massacred (Ét. Slonon for E. E. Slosson, etc.).

JAMES FIELD

The World of Dreams, by HAVELOCK ELLIS. Boston & New York, Houghton Mifflin Company, 1911. pp. xii., 288. Price \$2.00 net.

There are at least four different ways, Mr. Ellis tells us, of writing a book on dreams. There is the literary method, which may be dismissed at once as wholly unscientific; there is the clinical method, followed for instance by de Sanctis in his *I Sogni*; there is the experimental method, of which Mourly Vold has recently given us an excellent example; and there is the introspective method, for a special form of which we are referred, rather curiously, to Freud's *Traumdeutung*. However, we need not split hairs about classification. The field of dreams is, in fact, the playground of all sorts of psychological opinion; the time has not yet come for anything like a final synthesis; and so long as an author appeals to actual observa-

tion, and so long as he takes up a definite position with regard to the phenomena observed, we may be grateful for what he gives us. Mr. Ellis has been noting down his dream-experiences for more than twenty years, and in the present volume throws the outcome of his studies into popular form. Let us see what his standpoint is.

Perhaps the first question that one asks of a writer upon dreams is that of his attitude to the Subconscious. There is, indeed, no question upon which the psychologists of this generation are more sharply divided; there is also no question which more imperatively demands a positive and clear-cut reply; either one explains the conscious by a subconscious which is still mental, or one draws the line of mind at the boundary of consciousness itself. Mr. Ellis seeks the middle way, where the faring is least secure. Consciousness is for him—as for the definers of the term in the *Dictionary of Philosophy and Psychology*—‘the distinctive character of whatever may be called mental life.’ Subconscious is for the *Dictionary* ‘not clearly recognized in a present state of consciousness, yet entering into the development of subsequent states of consciousness,’—a definition that is obviously equivocal, and that is immediately particularized. Mr. Ellis cites it without comment, adding only that subconscious states are ‘slightly, partially, or imperfectly conscious,’ and that “any objection to so precise and convenient a term (!) seems to belong to the sphere of personal idiosyncrasy” (p. 4). What then does the term precisely mean? Consciousness covers, by definition, the whole of the mental life; is the subconscious not-mental? No: it is slightly, partially mental, or imperfectly mental. But, if it is the former, is the remaining ‘part’ of it material? Then, surely, our author’s objection to ‘dispositions of brain cells’ falls to the ground. Or, if it is the latter, can Mr. Ellis explain how a phenomenon may show ‘imperfectly’ the distinctive character that makes it what it is?

A second question, of a more topical nature, that the modern reader asks of a book on dreams is the question of the writer’s attitude towards the psychoanalytic school. And here again Mr. Ellis strikes a middle path which, to the reviewer, seems to lose itself in equivocation. “Freud’s subtle and searching analytic genius has greatly contributed to enlarge our knowledge of this world of sleep. We may recognize the value of his contribution to the psychology of dreams while refusing to accept a premature and narrow generalization” (pp. 174 f.). But is not this an attempt to eat one’s cake and have it too? We are to accept all the Freudian analyses, as holding for the dreams analyzed; but we are to reserve a large body of unanalyzed dreams, as belonging to ‘quite distinct’ types. Mr. Ellis disclaims in his Preface (p. vii.) any use of the psychoanalytic method; how, therefore, does he know that the unanalyzed dreams would not have submitted to a Freudian interpretation? And, in larger terms, how shall one gain the right to declare a generalization premature and narrow, save by producing negative instances within the universe of discourse to which the generalization belongs? The reviewer, be it noted, is not here arguing on behalf of Freud, as he was not arguing just now against any and every doctrine of the Subconscious; he is urging, simply, that unclear definition and dogmatic statement are out of place in science, however popular the form in which the scientific presentation may be cast.

What is the mechanism of dreaming? Mr. Ellis’ view is not easy to expound: partly because his own exposition is spread serially over a number of chapters, partly because he is by no means careful in expression. He inclines strongly to the opinion that all dreams are peripherally initiated; “we seem entitled to say that in all dreams there is probably a presentative element,” that every dream has “received an initial stimulus from some external or, at all events, peripheral source” (pp. 72 f.). The dream-consciousness is, therefore, always thrown into gear, so to say, by a peripheral stimulus, external or organic. When the gearing has been effected, movement may be kept up, even after the energy of the stimulus is exhausted,

by an inherent tendency of images to assert themselves in consciousness. A 'more or less spontaneous procession of images' is the elemental stuff of dreams (pp. 24 f.). In the most elementary form of dreaming, in which the peripheral element plays its largest part, we have a 'seemingly spontaneous,' 'mechanical flow of images, regulated by associations of resemblance' (p. 27). The stimulus, however, is never presented directly to consciousness, as it would be in the waking life, but "serves to arouse old memories and ideas which the dream consciousness accepts as a reasonable explanation of it" (p. 73). This circumstance would appear to determine the character of our dreams; and the author admits that the dream-consciousness may show "what we call a deliberate subconscious selection of imagery," so that a 'real subconscious link' connects any two successive images. Nevertheless, he insists that there may be sheer discontinuity; "mental imagery is deeper and more elemental than any of the higher psychic functions even when exerted subconsciously. Discontinuous images may arise from a psychic basis deeper than choice, their appearance being determined by their own dynamic condition at the moment." "If we hold to the belief that dreaming is based on a fundamental and elementary tendency to the formation of continuous or discontinuous images, which may or may not be controlled by psychic emotions and impulses, we shall be delivered from many hazardous speculations" (p. 24).

The passages are not clear. If the occurrence of the initial stimulus is universal, it is probably also necessary; yet, if images have a tendency to spontaneous irruption into consciousness, there is no reason why a dream should not be initiated at the centre; no reason, indeed, why we should cease from dreaming at all. Contrariwise, if the images are held together by associations of resemblance, then the initial stimulus, *plus* the law of association of ideas, is adequate to the result; the spontaneity of the images is illusory.

It seems that Mr. Ellis has found certain dreams which suggest a subconscious elaboration, and certain others which have a mechanical or disconnected look; he has accordingly called in, as explanatory principles, both the Subconscious and a dynamic *Bereitschaft* of images. His anti-physiological bias, leading him to emphasize the Subconscious and the Image as psychological terms, has then prevented the enquiry whether the 'dynamic condition' of the images is not an alternative to, or an equivalent of, the hypothesis of the Subconscious, and has also encouraged him to traverse the accepted doctrine of psychology that *Bereitschaft* is always strictly conditioned. This, at any rate, is the interpretation which the reviewer has put upon a puzzling subject.

The primary stimulus, we have learned, does not come to consciousness in its own right, but arouses old memories and ideas. The arousal, however, is again not direct. The stimulus, if it appeal to one of the higher senses, suggests motor activities which cannot be carried out; the excitation, entering motor channels, is impeded, broken up, scattered; and this process "is transmitted to the brain as a wave of emotion." If the stimulus is internal,—alimentary, cardiac, respiratory,—then the organic sensations which it sets up themselves constitute emotional excitement. While, therefore, the elemental stuff of dreams is a procession of images, "the fundamental source of our dream life may be said to be emotion" (p. 107). How does the stimulus, after its emotive transformation, arouse a dream? "The chief function of dreams is to supply adequate theories to account for the magnified emotional impulses which are borne in on sleeping consciousness. This is the key to imagination in dreams . . . Unable to detect the origin" of the emotive waves, sleeping consciousness "invents an explanation of them" (*ibid.*). This "craving for reasons is instinctive" (p. 8); "every dream is the outcome of this strenuous, wide-ranging instinct to reason" (p. 57); "all dreaming is a process of reasoning" (p. 56). Reasoning, however, is a "synthesis of images suggested by resemblance

and contiguity," and "the whole phenomenon of dreaming is really the same process of image formation, based on resemblance and contiguity" (p. 57). The possibility of subconscious elaboration is here given with the instinct to explain. But will not the hand of the instinct be forced, so to speak, time and again, by the inherent self-assertiveness of the images?

The reviewer has, perhaps, dwelt unduly upon obscurities which Mr. Ellis will, doubtless, be able to clear up. At the same time, the obscurities are here, a stumbling-block to the reader. And they appear again in connection with another question. What do we mean when we ask whether a particular conscious function or formation 'occurs' in dreaming? We may mean: Does it operate or appear as it does in the waking life? Is the mechanism of the dream consciousness, in its regard, to be considered as normal? Or we may mean: Do we ever dream of it as operating or appearing? Mr. Ellis is not careful to distinguish these two forms of the question. When he declares, for instance, that the attention of dreams is for the most part involuntary attention, he means, of course, that the general function of attention in dreams is of the same kind as the function of involuntary attention in waking; we are 'really' only involuntarily attentive, whatever we may dream ourselves to be. Since, however, he has just described two dreams in which the state of voluntary attention, as dream-phenomenon, is well marked (a dream in which a particular kind of postage stamp is looked for among the contents of a pocket-book, and a dream in which a particular hat is sought in a row of hats of all shapes and sizes), it would have been worth while to make the distinction explicit. Again, there is no doubt that many persons have, not infrequently, the dream-experience that they are dreaming. Mr. Ellis comments on the evidence as follows: "I have never detected in my own dreams any recognition that they are dreams. I may say, indeed, that I do not consider that such a thing is really possible" (p. 65). Truly, it is not 'really' possible to 'recognize' in dreams the dream-character of one's experience; for that, one must have waked. But just as in the waking life one may say, on the ground of specific conscious experience, 'I was thinking,' 'I was trying to remember,' 'I must have been dreaming,' so may one dream, specifically, that one is thinking, or trying to remember, or dreaming. To dream 'I am dreaming' is no more remarkable than to dream 'I am looking for stamps in a pocket-book.' Indeed, if the word 'occur' is taken in its second sense, there is no mode or item of waking experience that may not occur in the dream-consciousness: voluntary attention, deliberative thought, high resolve appear on equal terms with involuntary attention, overwhelming emotion, or the dream-state itself.

Mr. Ellis is not at his best in discussions of a technically psychological sort. On the other hand, his wide reading and practised fluency of writing stand him in good stead when he turns to special subjects. There is, *e. g.*, an interesting chapter on Aviation in Dreams. Mr. Ellis rejects Stanley Hall's theory of a hydro-psychosis, and explains the flying dream in the orthodox way (though he achieved orthodoxy unawares, by his own observations) as reflecting the rise and fall of respiration. It is odd that he has not thought of the possibility of a dendro-psychosis. If the falling dream suggested to one of Mr. Hutchinson's correspondents (*Dreams and their Meanings*, 1901, 108), the fear of falling from trees in sleep, the flying dream suggests no less definitely the swing of our arboreal ancestor, Mowgli-like, from tree to tree. This derivation would further account for the fact—a difficulty to Mr. Ellis—that the dream of flying is usually agreeable, the dream of falling usually disagreeable. The start from the sensations of breathing is not hereby denied; but on the writer's own principles some reason must be given for their imaginative dream-interpretation as the movement of flight.

Another chapter, on Dreams of the Dead, is based upon a paper published in the *Psychological Review* in 1895. At that date, Mr. Ellis could not,

of course, have read Mr. Kipling's wonderful story of *They*; but it might have been worth while, in the present recasting of his material, to raise explicitly the question whether 'one never sees a dead person's face in a dream.' The reviewer made some enquiry on this matter, in 1904, and found (in accordance with his own experience) that the dead face not uncommonly appears, as clearly and vividly as the face of a living person.

The chapter on Memory in Dreams contains a long excursus on false recognition or paramnesia. The author believes that the necessary preliminary to paramnesia is a general condition of temporary or chronic nervous fatigue, though no sense of exhaustion need be felt. An externally aroused perception begins, in this state, without sufficient strength to afford the realization that it is beginning; it is brought down to a lower and fainter stage, at which it is on a level with an internally aroused perception or memory-image; and when consciousness has become sufficiently developed to apprehend the nature of the perception, it also becomes aware that the experience has been continuing for an indefinite time (pp. 251, 258). "The mind has become flaccid and enfeebled; its loosened texture has, as it were, abnormally enlarged the meshes in which sensations are caught and sifted, so that they run through too easily. They are not properly *apperceived*. To use a crude simile, it is as though we poured water into a sieve. The impressions of the world which are actual sensations as they strike the relaxed psychic meshwork are instantaneously passed through to become memories, and we see them in both forms at the same moment, and are unable to distinguish one from the other" (p. 259). The difficulty in this hypothesis is that the 'actual present' reaches consciousness in the 'enfeebled shape' of a *memory*; for an enfeebled perception is not, *ipso facto*, an image of memory. Mr. Ellis has spared no pains to acquaint himself with previous attempts at explanation, but he has missed the ingenious analysis offered by Linwurzky in Meumann's *Archiv*.

OTTO PERLER

BOOK NOTES

Unsoundness of mind, by T. S. CLOUSTON. London, Methuen & Co., 1911. 361 p.

The author is of the conviction that unsoundness of mind is a topic that urgently claims the attention not only of medical men but of intelligent laymen; this on account both of the vagueness and vastness of its problems but also on account of the odium with which ignorance and prejudice have surrounded it. Thus, medical specialists to-day owe a duty to the public as well as to the profession, and it is to discharge this duty that the author writes this book into which he has put the results of a long life rich in experience with the insane. He has taken the broadest view of the topic, dealing with such themes as the hygiene of mind, education, the tragedy of mental unsoundness, its relation to crime, borderland phenomena, etc.

The origin of life: being an account of experiments with certain super-heated saline solutions in hermetically sealed vessels, by H. CHARLTON BASTIAN. London, Watts & Co., 1911. 76 p. (with ten plates).

This is a reproduction of an article lately submitted to the Royal Society and which it did not consider suitable for acceptance. To this, the author replies that very few believe that there was any non-natural cause of life. Most think that there were certain conditions early in the history of the earth that made abiogenesis possible. This work represents six recent years of investigation upon the same subject which the author wrought on in the years ending in 1872, under the title Heterogenesis or Archebiosis. We are reminded that the same society turned down Joule's "The Mechanical Equivalent of Heat," but published Tyndall's rather unsystematic studies.

Famous impostors, by BRAM STOKER. London, Sidgwick & Jackson, Ltd., 1910. 349 p.

This book, with ten illustrations, briefly characterizes five pretenders, three practitioners of magic, the Wandering Jew, John Law, six cases of witchcraft, Tichborne claimant, women as men, ten hoaxes, Chevalier d'Eon and the Bisley Boy. The tales are very crudely told, with very disturbing affectation of scholarship. The only really valuable article is the last one and the longest, on the Bisley Boy, where the author tries to make out the case that Queen Elizabeth was practically sequestered from her father and died at about the age of nine, while her keepers, fearing the king's wrath, found the only possible substitute that would pass muster for her in a boy nearby who assumed her rôle and lived it out through life. He proved to be very able and remarkably adapted to his rôle, so that, if this view were true, Queen Elizabeth was a man. His-her remarkable and unreasonable devotion to the interests of two or three people, otherwise unworthy, is explained by the fact that they alone knew her secret which had to be guarded in every possible manner.

The nervous life, by G. E. PARTRIDGE. New York, Sturgis & Walton Company, 1911. 216 p.

The author uses this term for two conditions: first, the nervous social industrial life best typified by the stress and strife of the great cities; and secondly, as expressed in the temperament in nervous individuals. Both these elements of nervous life are on the increase and each produces the other.

The problem is more pressing than ever before. After stating some biological laws and the need of self knowledge, the author discusses the principles of control, the optimum life, food, skin, exercise, sleep, rest, work, recreation, emotions, intellect, suggestion and mental healing.

Some mental processes of the rhesus monkey, by WILLIAM T. SHEPHERD. From the Psychological Laboratory of the George Washington University. The Psychological Monographs, Vol. XII, No. 5. Nov., 1910. Whole No. 52. 61 p.

The author studied these monkeys with reference to the formation of habits in releasing fastenings, in visual discrimination of brightness and color, auditory of noise and pitch, inhibition of habits, imitation, ideation, reasoning, adaptive intelligence and memory. He found that monkeys discriminate brightness but take a long time to do so unless there is a direct incentive to their work, but do so very readily if connected with objects they are familiar with. So too with colors, if of their food. Habits are rapidly formed if there is good inducement, and they inhibit former habits easily. In this respect, they are superior to raccoons, dogs, cats, elephants, otters, or any other animals yet tested. They have retentive number memory. Their higher powers are rudimentary, but they have what may be called practical ideas. Two learned by imitation, six did not appear to. All seemed to reach a generalized mode of action in dealing with problems without attaining true general notions. They have an adaptive intelligence and lower forms of reason of a mental status inferior to true reason.

The value and dignity of human life as shown in the striving and suffering of the individual, by CHARLES GRAY SHAW. Boston, Richard G. Badger, 1911. 403 p.

This book is written with the conviction that a change is taking place in our notion of human ideas and activities and, indeed, of the value of life. It is dedicated to Professor Eucken who has the same conviction. In the first part, entitled the problems of human life, the author discusses the striving of humanity, the continuity of the former, the human world. In part two, he takes up the naturalistic view of life, that of humanity and sense in pleasure, desire, self, transmutation of naturalism and moralism, eudemonism. The third part is characterized ethics of the life of humanity and the will, conscience, rectitude, freedom, practical demands, rigorism, destiny of man, etc. Part four is humanistic ethics, major and minor, morality, category of virtue, virtue as an ethical sanction, human dignity in the ethical category, the dignity of selfhood, the triumph of humanity in major morals.

Three thousand years of mental healing, by GEORGE BARTON CUTTEN. New York, Charles Scribner's Sons, 1911. 318 p.

The writer has given us a rather hasty but interesting sketch of mental healing from the very earliest civilization before Christianity down to Schlatter, the Holy Ghost and Us, Dowie, Mrs. Eddy, and Emmanuelism. with interesting chapters on relics and shrines, healers, talismans, amulets, charms, royal touch, Mesmer and after, with eight interesting illustrations. The book is popular and does not attempt to go into details of scholarship. It is a little difficult to know just what the author aimed at. Perhaps it was to show the community of all these different types of healing or to show the persistence of the type down all the ages. This is, at any rate, an impression the book gives us. The author does not attempt either criticism or defence of the movement and indeed he has left us very uncertain what his own attitude toward it is, unless the reader infers, as perhaps he will inevitably do, that a writer who would spend so much time upon such a topic must believe that there is something of great consequence involved in his theme. The book is strangely pragmatic, non-committal,

attempting almost nothing in the way of psychological or philosophical explanation. Perhaps the author intends fuller treatment later.

Die Mimik der Kinder beim künstlerischen Geniessen, von RUDOLF SCHULZE. Leipzig, Voigtländer, 1906. 34 p.

The author showed a series of pictures of very diverse character to a group of 12 girls, and a few seconds after the exposure of each picture photographed their faces in order to show the effect of the pictures. The character of the pictures ranged all the way from very comic scenes to very serious including the Crucifixion, and the faces of these girls are very expressive, the picture and the expression being given on the same page and described.

The soul of the Indian; an interpretation, by CHARLES ALEXANDER EASTMAN (CHIVESA). Boston, Houghton, Mifflin & Co., 1911. 170 p.

This is one of the most charming and fascinating of books. The author, in spite of the high civilization he has attained, has remained loyal to, and sympathetic with, the faith of his people and give us a most interesting account of how the great world looks to them. He treats the great mystery, the family altar, ceremonial and symbolic worship, barbarism, the moral code, the unwritten Scriptures, on the borderland of spirits. We have no space here to do justice in a psychological journal to this work. It should be in the hands of every one who has any interest in the Indian, and particularly in the hands of those who have to deal with him officially.

Beitrag zur Aetiologie der Melancholie, by EMIL VILLIGER. Basel, Schweizer, 1898. 77 p.

This thesis leads the author to conclude that melancholy is a psychosis that may attack any age but is more common in men from 45 to 55 and in women from 30 to 50. It is more common among women than men and more among the unmarried than the married, more common among country than city people. While there are many causes, heredity and the psychopathic constitution are the chief. The psychic causes are shock and illness.

Affe und Mensch in ihrer biologischen Eigenart, von ALEXANDER SOKOLOWSKY. Leipzig, Theod. Thomas, 1911. 147 p.

This work is by an assistant director of the Zoological Garden in Hamburg and contains a number of interesting characterizations. The first part is devoted to apes, the last to primitive man, and a few, though it must be admitted, rather superficial resemblances in the mode of life between the two are pointed out.

Magical titbits, by LOUIS HOFFMANN. N. Y., E. P. Dutton & Co., 1911. 221 p.

The first part of this volume describes a few items of magic that are new, and in the second part the author has put into more permanent shape a number of ingenious inventions of his old friend Hartz which have hitherto been accessible only in serial form.

The beginning of speech: a treatise on the uni-radical origin of Indo-European words, by A. L. SNELL. London, Kegan Paul, Trench, Trübner & Co., 1910. 267 p.

This work is the result of twenty-five years' study of some fifty languages, chiefly Indo-European. It has involved not merely a study of lists of words but a great deal of reading and note-taking. The author is evidently not familiar with modern philology, and so apologizes for not quoting. He found, as the book was going to press, that one or more of his important conclusions had been anticipated by J. W. Donaldson's *New Cratylus*. He holds that the number of simple, underived words in these languages is small and that these consist of three letters and can be traced from one language to another, not by permutation of initial consonants but by

considerations of basal meaning; that each of these words is but one of a vast number of modifications of one primal, photo-mimetic utterance. His five laws are as follows. Monosyllabic words ending in l, m, n, or r, were once disyllabic and have been shortened by dropping an internal guttural after its weakening to the aspirate. Words now beginning with l, m, n, or r, have lost initial wa, and so by the above law have retained no part of the original word. Words now beginning with a vowel have lost a consonant and those now ending with a vowel have lost an original final guttural. Words now containing an internal n or m must, for philological purposes, be written without these letters, which are merely nasal symbols and no part of the original word.

The main thesis of this whole book is that each of these words is but one of a vast number of modifications of one primal photo-mimetic utterance. The original word from which all others have been derived must have been something like vig, wag, jag, twav, jaw, and so on, 36 words which are basal. These 36 he reduces to 6 and these 6 are all reduced by his laws to waw, the primal, photo-mimetic utterance from which every word in all the Indo-European languages has arisen. To the eye this word conveys little, but when pronounced it is obviously the common cry of many birds which the genius of primitive man transmuted into the foundation or nucleus of human language. This is not humiliation for we are of lowly origin.

Der Einfluss psychischer Vorgänge auf den Körper; insbesondere auf die Blutverteilung, von ERNST WEBER. Berlin, Julius Springer, 1910. 426 p.

The writer discusses first the various physiological methods of registering the accompaniments of psychic changes, especially the blood which is without the brain and within it in men and animals. He takes up the effect of the concepts of movement upon the distribution of blood in the human body. He seeks to prove from experiments on animals the independence of the brain in regulating its own blood supply. On this basis he shows the changes of the volume of blood in the human brain in connection with different psychic processes, the reversal of the normal blood distribution by physiological and pathological fatigue, the significance of the variations of the blood distribution within the body under the influence of psychic processes. The work closes with an excellent and extended bibliography.

Bilderatlas zum ersten Bande der Grundzüge der Sprachpsychologie, von OTTMAR DITTRICH. Halle a. S., Max Meiner, 1903. 95 p.

This atlas is by far the best now extant for the study of speech physiology and defects. There are many very ingenious modes of representing the various types of both normal and abnormal speech physiology which will commend themselves to all who have to teach the subject.

Truth on trial: an exposition of the nature of truth. Preceded by a Critique on Pragmatism and an Appreciation of its Leader, by PAUL CARUS.

This work is dedicated to William James. It discusses Pragmatism, the philosophy of personal equation, the rock of ages, the nature of truth, with an appendix on Pragmatism.

Personality with special reference to super-personalities and the inter-personal character of ideas, by PAUL CARUS. Chicago, Open Court Publishing Co., 1911. 68 p.

This work first discusses the following topics: significance of personality, the word persona and its history, problem of unity ideas, inter-personal super-personalities, trinity conceptions, the super-personal God. These titles will in general give sufficient intimation of the content of the book to those who are familiar with the Monist.

Technique de psychologie expérimentale, par ED. TOULOUSE et H. PIÉRON. Paris, Octave Doin et Fils, 1911. Tome premier, 303 p., et Tome second, 288 p. (Encyclopédie scientifique.)

These two volumes constitute a very convenient manual for the laboratory student and practitioner. The authors have well availed themselves of the work of their predecessors, Sanford and Titchener, although their book, from the nature of a subject growing so fast, will not be considered by all as up to date.

An introduction to experimental psychology, by CHARLES MYERS. Cambridge, The University Press, 1911. 156 p.

This little primer of psychology discusses touch, temperature, pain, color vision, the Müller-Lyer illusion, memory, mental tests, and has a good bibliography, index and a few colored plates.

Clever Hans, by OSKAR PFUNGST. With an introduction by Professor C. Stumpf. Translated from the German by Carl L. Rahn. New York, Henry Holt & Co., 1911. 274 p.

Every one will thank the author of this volume for bringing together in the characteristic German and thorough way the whole story of the rise and fall of this remarkable episode in the history of animal psychology. The author himself examined the horse and reached his own conclusions and describes in great detail how the trainer, Von Osten, directed his actions unconsciously, and what was still more marvellous, how other people did the same.

Experiments with drosophila ampelophila concerning evolution, by FRANK E. LUTZ. Pub. by the Carnegie Institution of Washington, Washington, D. C., 1911. 40 p.

These very interesting experimental studies lead the author to the conclusion that "there is no evidence that the constant disuse of wings during forty generation has had any effect" in modifying the venation or otherwise affecting the form of the wing of the sand fly.

Among friends. By S. McC. CROTHERS. Boston and New York, Houghton Mifflin Co. 1910. pp. iii., 278. Price \$1.25.

A collection of nine essays, in which literary reminiscence is blended with shrewd and kindly criticism of current social attitudes. "The Anglo-American School of Polite Unlearning," "The Hundred Worst Books," "The Romance of Ethics," "The Merry Devil of Education:" such titles speak for themselves.

The Corsican: a diary of Napoleon's life in his own words. Compiled by R. M. Johnston. Boston and New York, Houghton Mifflin Co., 1910. pp. vi., 526. Price \$1.75.

It was a happy idea which led Professor Johnston to bring together, in a single volume, the recorded utterances of the great Napoleon. Conversations, letters, notes, proclamations are here arranged in chronological order, with enough of explanatory comment to make the narrative continuous for any reader who possesses an elementary knowledge of the period. The whole forms a human document of extreme interest.

Publication of the Massachusetts General Hospital: Medical and Surgical Papers. Boston, 1910. 374 p.

The adolescent, by J. W. SLAUGHTER. With an introduction by J. J. Findlay. London, Swan Sonnenschein & Co., 1911. 100 p.

Introduction to philosophy, by WILLIAM JERUSALEM. Authorized translation from the fourth edition by Charles F. Sanders. New York, The Macmillan Co., 1910. 219 p.

- Otto Weiningers Tod*, von HERMANN SWOBODA. Vienna, Deuticke, 1911. 100 p.
- Pubertät und Auge*, von RUDOLF SCHNEIDER. München, Otto Gmelin, 1911. 17 p.
- Festschrift zum sechzigsten Geburtstag Richard Hertwigs*. Jena, Gustav Fischer, 1910. 3 vols. (Arbeiten aus dem Gebiet der Zellenlehre und Protozoenkunde.) 674, 624, 308 p.
- The maturation of the egg of the mouse*, by J. A. LANG and E. L. MARK. Washington, Published by the Carnegie Institution of Washington, 1911. 72 pages and 6 plates.
- Das Wesen der Vernunft*, von ADOLF KELLER. Gross-Lichterfelde, J. Univerdorben & Co., 1911. 12 p.
- L'analyse physiologique de la perception*, par ÉDOUARD ABRAMOWSKI. Paris, Bloud & Cie, 1911. 121 p. (Collection de psychologie expérimentale et de Métaphysique. Directeur Raymond Meunier.)
- Kleine Schriften*, von WILHELM WUNDT. Leipzig, Wilhelm Engelmann, 1911. 496 p. (Zweiter Bd.)
- Creative evolution*, by HENRI BERGSON. Translation by Arthur Mitchell. New York, Henry Holt & Co., 1911. 407 p.
- Die Methode der historisch-völkerpsychologischen Begriffsanalyse*, von ABRAHAM SCHLESINGER. Sonderabdruck aus Archiv für die gesamte Psychologie, XX Bd, 2 Heft. Leipzig, Wilhelm Englemann, 1911. pp. 150-185.
- The place of movement in consciousness*, by W. B. PILLSBURY. Reprinted from the Psychological Review, March, 1911. Vol. XVIII, pp. 83-99.
- Psychotherapy from a psychological standpoint*, by DAVID S. BOOTH. Reprinted from The Alienist and Neurologist, February, 1911. Vol. XXXII, No. 1, 24 p.
- Notebook of American Indian languages*, by FRANZ BOAS. Washington, Government Printing Office, 1911. 1069 p. (Smithsonian Institution. Bureau of American Ethnology Bulletin 40.)
- Psychic phenomena, science and immortality*, by HENRY FRANK. Boston, Sherman, French & Co., 1911. 556 p.

In the first book, the author deals with psychic phenomena and has much to tell us concerning the soul's secret scroll, sub-conscious mind, correspondence, superphysical senses, Sir William Crookes, the sleepless self, spirit forms and materializations. The second book is entitled scientific interpretation. Here he deals with ultimate matter and vital energy, occult forces, the subtle seat of human intelligence, biology of the soul, its body, radio-activity, telepathy and substantiality of thought. The third book deals with problems of immortality. He deems that the studies which he enumerates demonstrate beyond all question the survival of the soul after death.

Influencing men in business, by WALTER DILL SCOTT. New York, The Ronald Press Co., 1911. 168 p.

This book assumes that we can increase our ability to influence men by mastering a few simple laws for influencing their minds. To find these, he analyzes deliberation and suggestion and tells how to decide questions and reach conclusions, when to use arguments and when suggestion, and how to make both effective.

Précis d'auto-suggestion volontaire, par GÉRAUD BONNET. Paris, Jules Roussett, 1911. 297 p.

After preliminary explanations, the author discusses hypnotism and

auto-suggestion, the education of the will, the influence of the self, concentration of thought and personal power.

Les syncinésies, par G. STROEHLIN. Paris, G. Steinheil, 1911. 147 p.

In the first, clinical part, the author discusses movements associated with normal states, those with motor debility, those with volitive syncinésias, those with hemiplegia, and then gives us his scheme of diagnosis. In the second, physiological part, he follows the same fourfold division of his material and gives a few general conclusions.

Das kranke Gedächtnis, von PAUL RANSCHBURG. Leipzig, Johann Ambrosius Barth, 1911. 138 p. Mit 6 Kurven und 27 Abbildungen im Text.

This work is divided into two parts. The first is the result of experimental psychopathology in the study of memory. First the retrospective, and then the anterograde direction of memory is considered. The second part describes the ways of experimental investigation in the pathology of memory, how to investigate recognition, reproductive activity and those of investigating morbid memories by means of special apparatus. The work has many cuts and contains an excellent bibliography.

The psychology of education, by J. WELTON. London, Macmillan & Co., 1911. 507 p.

The chapters are as follows: the relations between education and psychology, the study of mental life, bodily endowment, general mental endowment, variations in mental endowment, nature of experience, development of interests, direction of activity, learning by direct experience, learning through communicated experience, critical thought, ideals, character.

Scientific method in animal psychology, by ROBERT M. YERKES. 13 p. Extrait des Comptes rendus du VI^e Congrès international de Psychologie. (Genève 1909. Pages 808-819).

The psychological aspects of illuminating engineering, by ROBERT M. YERKES. (A lecture delivered at the Johns Hopkins University, October-November, 1910.) pp. 575-604.

Do kittens instinctively kill mice? by ROBERT M. YERKES and DANIEL BLOOMFIELD. Reprinted from the Psychological Bulletin, August, 1910. Vol. VII, pp. 253-263.

Psychology in its relations to biology, by ROBERT M. YERKES. Reprinted from The Journal of Philosophy, Psychology and Scientific Methods, Vol. VII, No. 5, March 3, 1910. pp. 113-124.

The method of Pawlow in animal psychology, by ROBERT M. YERKES and SERGIUS MORGULIS. Psychological Bulletin, Vol. 6, No. 8, August 15, 1909. pp. 257-273.

Modifiability of behavior in its relations to the age and sex of the dancing mouse, by ROBERT M. YERKES. Reprinted from The Journal of Comparative Neurology and Psychology, Vol. XIX, No. 3, June, 1909. pp. 237-271.

Die Spuren interessebetonter Erlebnisse und ihre Symptome, von OTTO LIPMANN. Leipzig, Johann Ambrosius Barth, 1911. 96 p. (Beihefte zur Zeitschrift für angewandte Psychologie und psychologische Sammelforschung., hrsg. von William Stern und Otto Lipmann. 1)

Untersuchungen über Geschlechts-, Alters- und Begabungs-Unterschiede bei Schülern, von JONAS COHN und JULIUS DIEFFENBACHER. Leipzig, Johann Ambrosius Barth, 1911. 213 S. und drei Tafeln. (Beihefte zur Zeitschrift für angewandte Psychologie und psychologische Sammelforschung., hrsg. von William Stern und Otto Lipmann. 2)

DR. EDMUND MONTGOMERY

The following note by Mr. B. F. Underwood, of the Quincy, Ill., *Journal*, refers to a writer whose work is familiar to many of our readers.—

At Hempstead, Texas, died a few days ago Dr. Edmund Montgomery. Probably there was some mention of his death in the local papers of the community, in which he was a well-known citizen; but the writer of this article has seen no reference to the event in any *Journal*. Yet Dr. Montgomery was the author of original scientific and philosophical works,—some of them written in English, others in German,—which are in all the great libraries of the world. In his fields of thought he had an international reputation. He wrote on "Theories of Knowledge," "Our Ideas of Time and Space," "The Formation of So-Called Cells," "Vital Organization," "The Unity of the Organic Individual," "The Dual Aspect of Our Nature," "Protoplasm of the Muscles," "Transcendentalism," "Vital Motility," etc.

Dr. Montgomery was for years a contributor to *Mind*, the *Popular Science Monthly*, and the *Boston Index*, besides other journals in this country.

Dr. Montgomery was born in Edinburgh, Scotland, in 1835, of Scotch parents. He was taken to Paris in care of a French nurse so early that the first language he learned to speak was French. At nine he was taken to Frankfort, Germany, where, educated in German, he began early the study of the natural sciences and philosophy.

He was acquainted with Feuerbach; and at Heidelberg he attended the lectures of Moleschott and Kuno Fischer. He used to see Schopenhauer, with his poodle, daily, and was much interested in the philosopher of pessimism. At Bonn he attended Helmholtz's famous lectures on the "Physiology of the Senses." He studied at German universities—Heidelberg, Berlin, Bonn, Würzburg (where he received the M. D. degree), Prague and Vienna. He wrote in German a reply to Kant's "Critique of Pure Reason," at Munich in 1871. From 1860 to 1863 he was lecturer on Physiology in St. Thomas, Hospital, London, where the effects of a dissecting wound put an end to his work in that institution, where he used to meet and converse with Darwin.

For six years he practised medicine at Madeira, Mentone, and Rome; and in 1869, with a competence, he retired to give his whole attention to science.

In 1871 he went to Texas and bought the Liendo plantation, paying for it \$40,000. In a letter to the writer he wrote: "The first seven years here in the South were devoted to laborious biological research; no writing at all."

Dr. Montgomery's wife was Elizabeth Ney, a grandniece of Marshal Ney of France. She acquired a reputation as an artist, and designed and executed some of the finest pieces of sculpture in the state capital at Austin.

Late in the eighties Dr. Montgomery, by request, sent a paper to be read before the "Concord School of Philosophy," whose programme that season included lectures by Dr. W. R. Alger and Dr. W. T. Harris, whose terminology caused no little merriment among those unacquainted with scientific and philosophic thought. In the *Boston Record* from some bright reporter appeared the following, indicating the impression Dr. Montgomery's paper had produced among those not so much interested in the thought as they were confounded by the language:

"A Texan has floored the Concord crowd,
 Sing high, and sing ho! for the great Southwest;
 He sent 'em a paper to read aloud,
 And 't was done up in style by one of their best.

"The Texan, he loaded his biggest gun
 With all the wise words he ever had seen,
 And he fired at long range with death-grim fun,
 And slew all the sages with his machine.

"He muddled the muddlers with brain-cracking lore,
 He went in so deep that his followers were drowned,
 But he swam out himself to the telluric shore,
 And crowed in his glee o'er the earthlings around.

ENVOY

"Oh Plato, dear Plato, come back from the past!
 And we 'll forgive all that you ever did to vex us,
 If you 'll only arrange for a colony vast,
 And whisk these philosophers all off to Texas."

In scientific and philosophical circles the paper attracted wide attention, and is included among his published writings. Dr. Montgomery was in personal appearance as handsome and impressive and in manners as courtly and courteous as he was intellectually brilliant.

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THE PSYCHOPATHOLOGY OF EVERYDAY LIFE¹

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I. INTRODUCTION

Under this title Freud has written an interesting volume² dealing with a number of mental processes that previously had received little or no attention from psychologists. The material of this kind that lends itself to study, like that of dreams, is very extensive, and is accessible to every one; it is, therefore, of importance to those who wish to test Freud's general psychological conclusions, and who have not the opportunity of investigating the more obscure problems of the psycho-neuroses. Freud's study of the mental processes in question is of especial interest as showing that mechanisms similar to those observable in the abnormal also occur in the

¹Elaborated from an address delivered before the Detroit Academy of Medicine, May 16th, 1911.

²Freud, S.: Zur Psychopathologie des Alltagslebens. Dritte Auflage, 1910.

normal; indeed from a psychological point of view these processes may be termed symptoms, although they occur in perfect health. They may be further likened to neurotic symptoms in that they represent flaws in the normal functioning of the mind.

Freud's principal thesis in this connection may be thus stated: Certain inadequacies of our mental functioning, and certain apparently purposeless performances, can be shown by means of psycho-analysis to have been determined by motives of which we were not at the time aware. The occurrences in question have the following characteristics in common: They belong to what may be called normal behavior. They are only temporary disturbances of a function which at another moment would be correctly performed. Their incorrectness is at once recognized as soon as attention is drawn to them. We can trace no motive for them at first, but always attribute them to "inattention," to "chance," and so on.

It will be seen from this that, according to Freud, our mental processes are more rigorously determined than is commonly believed, and that many of them generally thought to be causeless have in fact a very precise and definable cause. The same remark applies to many mental processes where we believe we have a perfectly free choice. A typical instance of this is afforded by the child game "think of a number." Whereas at first sight it would appear that we are free to choose any possible number, careful analysis shows, as was first pointed out by Adler¹ a few years ago, that the number actually chosen is always connected with some mental process of considerable personal significance, though this may never have been realized by the subject, and that the choice has been determined by definite preceding mental constellations. I may relate an example of this, obtained from an unbelieving acquaintance. He produced the number 986, and defied me to connect it with anything of especial interest in his mind. Using the free-association method he first recalled a memory, which had not previously been present to him, and which was to the following effect. Six years ago, on the hottest day he could remember, he had seen a joke in an evening newspaper, which stated that the thermometer had stood at 986 deg. F., evidently an exaggeration of 98.6 deg. F. We were at the time seated in front of a very hot fire, from which he had just drawn back, and he remarked, probably quite correctly, that the heat had aroused this dormant memory. However, I

¹Alfred Adler: Drei Psycho-Analysen von Zahleneinfällen und obsidierenden Zahlen. Psychiatr-Neurol. Woch. 1905. Jahrg. VII. S. 263.

was curious to know why this memory had persisted with such vividness as to be so readily brought out, for with most people it surely would have been forgotten beyond recall, unless it had become associated with some other mental experience of more significance. He told me that on reading the joke he had laughed uproariously, and that on many subsequent occasions he had recalled it with great relish. As the joke was obviously of an exceedingly tenuous nature, this strengthened my expectation that more lay behind. His next thought was the general reflection that the conception of heat had always greatly impressed him, that heat was the most important thing in the universe, the source of all life, and so on. This remarkable attitude of a quite prosaic young man certainly needed some explanation, so I asked him to continue his free associations. The next thought was of a factory stack which he could see from his bedroom window. He often stood of an evening watching the flame and smoke issuing out of it, and reflecting on this deplorable waste of energy. Heat, flame, the source of life, the waste of vital energy issuing from an upright, hollow tube—it was not hard to divine from such associations that the ideas of heat and fire were unconsciously linked in his mind with the idea of love, as is so frequent in symbolic thinking, and that there was a strong masturbation complex present, a conclusion that he presently confirmed. His choice of the number was therefore far from being a free one, being in fact related to a very significant personal constellation.

II. FORGETTING

One of Freud's most notable contributions to psychology, and a conception fundamental in his study of the present group of mental processes, was his discovery that, in addition to the other causes of forgetting, "repression" (*Verdrängung*) plays a most important part. Others before Freud had realized the existence of this, but it was reserved for him to demonstrate the extent to which it is operative in both normal and abnormal mental life.

Freud regards repression as a biological defence-mechanism, the function of which is to guard the mind from painful experiences. He holds that there is in the mind of every one a tendency to forget the things that the person does not like to be reminded of, in other words, painful or disagreeable memories. It is true that we often remember against our will matters that we would rather forget, but there are two explanations for this. In the first place, such disagreeable haunting memories are frequently themselves only the replacements

of buried and still more disagreeable ones, with which they are associated, an occurrence allied to that concerned in the genesis of true obsessions. In the second place, the capacity to forget painful experiences is only of a certain strength, which differs greatly in different people, and is not always successful in achieving its aim. It is but rarely that one can forget the death of a dear relative, however desirable that might be, for the associative links to other conscious memories are too well formed. In such cases, what happens is that trivial memories, which by association might serve *unnecessarily* to remind us of the painful event, are apt to get forgotten, the name of the medical attendant, details as to the fatal malady, and so on; the tide of amnesia covers the base of the hill, but cannot reach the summit. By this means an economy is effected in the number of times that the painful memory is recalled to consciousness. Further, it must be remarked that, for reasons which cannot here be gone into, repression acts much more extensively in causing forgetfulness of internal, extremely intimate, and personal, mental processes than of what may be called external memories, known to the world, such as failure, grief, and so on. As is well known, Freud has applied his conception of repression to a number of other fields, notably to the explanation of infantile and hysterical amnesias,¹ which do not here concern us.

A good instance of the recognition of the part played in everyday life by repression has been furnished by Darwin, in a passage that does equal credit to his scientific honesty and his psychological acumen.² He writes, in his autobiography: "I had, during many years, followed a golden rule, namely, that whenever a published fact, a new observation or thought came across me, which was opposed to my general results, to make a memorandum of it without fail and at once; for I had found by experience that such facts and thoughts were far more apt to escape from the memory than favourable ones." Pick³ quotes a number of authors who more or less clearly recognize that a defensive striving against painful memories can lead to their becoming forgotten, but, as Freud remarks, no one has so exhaustively and at the same time so incisively described both the process itself and the psychological basis of it as has Nietzsche in his *Jenseits von Gut und Böse*; "Das habe ich getan, sagt mein Gedächtnis. Das

¹Freud: Selected Papers on Hysteria, and Three Contributions to Sexual Theory. Transl. by A. A. Brill.

²Life of Charles Darwin. Ed. by Francis Darwin. 1902. p. 42.

³Pick: Zur Psychologie des Vergessen bei Geistes- und Nervenkranken. Arch. f. Kriminal-Anthropologie u. Kriminalistik. 1905. Bd. XVIII S. 251.

kann ich nicht getan haben, sagt mein Stolz und bleibt unerbittlich. Endlich—gibt das Gedächtnis nach.”

The class of forgotten thoughts in everyday life to which this mechanism applies is of course that where the other causes of forgetting do not provide adequate explanations; in other words, it principally concerns matters that we should normally expect to remember. For instance, one would expect some hidden reason in the case of the name of a near relative or friend being forgotten much more readily than in the case of a casual acquaintance. The examples of the mechanism may conveniently be divided into two groups: (1) forgetting to carry out some intended purpose (Vergessen von Vorsätzen), and (2) forgetting a given memory.

(1) Forgetting to carry out an intention

A field in which some counter-will frequently leads to forgetting is that regarding the making or keeping of appointments. A man unwillingly feels that he should invite a given acquaintance to a social function he is giving in the near future. He says to him, “You will be sure to come, won’t you. I am not absolutely certain of the date at this moment, but I will send you a written invitation and let you know.” He forgets, until it is too late, and his excessive self-reproach betrays his unconscious culpability and shows that the forgetting was not altogether an accident. Maeder¹ relates the case of a lady who forgot to keep her appointment with the dressmaker to try on her bridal gown the day before the wedding, recollecting it only at eight in the evening. One must suppose that her whole heart was not in the marriage, and in fact she has since been divorced. In my own life I have noted numerous instances of a purposeful forgetting of appointments, particularly with patients. If a given patient is very tedious and uninteresting, I am very apt to forget that I have to see him at a certain hour, and if a doctor telephones to ask me whether I can see an interesting case at that hour, I am more likely than not to tell him that I shall be free then. Indeed I can recall several annoying quandaries that this habit has led me into. One is perhaps worth repeating, as showing how complete can be the divorce between two memories when an *Unlust* motive is in action. Some years ago, when in a junior position at a certain hospital, I was asked by my chief to see his out-patients on Friday, as he wished to attend an important luncheon at the time. It was an exceptional request, for the rule was that approbation of the committee had to be obtained before a substitute was allowed to act,

¹Maeder: Contributions à la psychopathologie de la vie quotidienne. Arch. de Psychol. 1907. t. VI. p. 150.

and I gladly consented, quite forgetting that I already had at the same time an appointment which I was very desirous of keeping, and which would have been particularly inconvenient to postpone. On several occasions during the week, while going over my future engagements, I thought of both these, but never together; the thought would come, "let me see, at one on Friday I have to be at such-and-such a place," and a few hours later a similar thought would come concerning the other place. The two intentions, both of which I was anxious not to forget, were kept distinct from each other, as if in water-tight compartments. When the time came I forgot the hospital appointment, and to my intense chagrin heard that my chief was very annoyed about being called away from his luncheon on account of my apparent unpardonable remissness. At the present time my memory chiefly fails in this respect in regard to visiting patients in nursing-homes, a duty I find irksome on account of the time consumed. Often when I am busy I conveniently forget, and recently I left a patient without her daily visit for nearly a week. The self-reproach one feels on recollecting the forgotten duty on these and similar occasions is indicative of the true significance of the occurrence. This significance is intuitively realized in the case of lovers. A man who has failed to appear at a rendezvous will seek in vain to be forgiven on the plea that he had forgotten about it, will indeed with this plea only increase the lady's resentment. Even if he falls back on the customary psychological explanations, and describes how urgent business had filled his mind, he will only get as reply, "How curious that such things did n't happen last year; it only means that you think less of me." Similarly, when a man begins to be forgetful about paying accustomed attentions to his wife, overlooks her birthday, and so on, she correctly interprets it as a sign of a change in their relations.

Another field where forgetting occurs to an untoward extent is in giving, a fact that indicates a more wide-spread objection to giving than is agreeable to our altruistic conceptions. Most of those who have filled secretarial positions have been astonished to find the difficulty there is in collecting subscriptions as they fall due, and the ease with which people with otherwise good memories "overlook" such matters. It is far from rare for them even to falsify their memory, and to assert firmly that they have already paid. A few, dimly conscious of their weakness, compensate for it by forming the habit of promptly paying every bill as soon as it arrives. In general, however, there is a striking difference between the ease with which one remembers to send to the bank incoming cheques, and that with which one forgets to pay incoming bills. The

same tendency is the explanation of the constant "forgetting" to return borrowed books that seems to afflict so many people, a habit which must have distressed most people who have a good library. This observation will be confirmed by any one who has tried to establish a permanent library in an institution where many coming and going students have ready access to it.

Almost as common is the habit of forgetting to post letters. Here, also, unconscious motives can sometimes be detected in individual instances. Sometimes one leaves a letter on one's desk for several days, forgetting each time to take it with one; in such cases it may be reckoned on that there is some secret opposition to sending the given letter. In one instance of the kind I ultimately posted the letter, but forgot to address the envelope. It was returned to me through the dead letter office, I addressed it, and again posted it, but this time without a stamp. I was then forced to recognize that there was in me an unconscious opposition to the sending of the letter, one of which I had previously been unaware, but which manifested itself in external inhibitions. One does not forget to post a letter that one's mind is in full harmony about sending; for instance, a love letter. One is more apt to forget to send a letter containing a cheque than one containing an account. Often the resistance is of a general order. Thus a busy man forgets to post letters entrusted to him—to his slight annoyance—by his wife, just as he may "forget" to carry out her shopping orders. Inhibitions of this kind sometimes betray a veiled antagonism towards the person whose behests we forget to fulfil. They constitute a way of depreciating the importance of the other person for ourselves, and when pronounced in general they indicate a lack of consideration for others, based on an excessive self-absorption or abnormally high self-estimation.

In examples similar to these preceding the counter-impulse that inhibits the memory is as a rule directed immediately against the conscious intention. In a more complicated series of cases, which the Germans term *Fehlleistungen*, it is directed against some other mental process, which, however, stands in associative relation to it; this mental process is, so to speak, symbolized in the conscious intention. The following are two examples of the kind. Maeder¹ relates the case of a hospital interne who had an important business appointment in the town, but who was not allowed to leave the hospital until his chief, who was out for the evening, returned. He decided to leave his post, nevertheless, and on getting

¹Maeder: Une voie nouvelle en psychologie; Freud et son école. Coenobium. Gennaio 1909. Anno. III. p. 100.

back late in the evening, was astonished to find he had left the light burning in his room, a thing he had never done before during his two years of service. He at once perceived the reason for his omission; his chief always passed by the window on his way to his own house, would see the light burning and conclude that the assistant was at home. The cause for the inhibition having passed, the subject readily appreciated it. A patient of mine on a number of occasions made the remarkable omission of forgetting to shave the right side of his face. It was always the same side, and it was the one that was turned towards me during the treatment. Analysis of the occurrence showed that it was determined by a number of unconscious processes, of which the following was one. The idea of hair was connected with various sexual ideas, and the non-shaving of the side turned to me symbolized a disinclination to lay bare his sexual life, the occurrence always synchronizing in fact with an outburst of resistance against the treatment.

2. Forgetting a given memory

We are concerned only with striking lapses in memory, namely, regarding matters that as a rule we can easily recall. An instance, which is hard to credit, though I can vouch for the accuracy of it, was related to me by a medical friend. His wife was seriously ill with some obscure abdominal malady, and, while anxiously pondering over the possible nature of it, he remarked to her, "It is comforting to think that there has been no tuberculosis in your family." She turned to him very astonished, and said "Have you forgotten that my mother died of tuberculosis, and that my sister recovered from it only after having been given up by the doctors?" His anxiety lest the obscure symptoms should prove to be tubercular had made him forget a piece of knowledge that was thoroughly familiar to him. Those accustomed to psycho-analysis will surmise that there is more to be said about the matter, but the example will serve to illustrate the influence affective processes have in connection with forgetting.

It is with proper names that one observes the most striking instances of this process. In the majority of cases the counterwill that prevents a familiar name from being recalled is directed against some mental process that is associated with the one to be recalled, rather than against this itself. On account of some disagreeable experience we would rather not recall a given name; we may actually succeed in forgetting it, but more often the tendency is shown indirectly in our being unable to recall other names resembling it and which

might bring the undesired one to our mind. In other words, we have to think of the undesired name at times, but we guard ourselves against doing so more often than is necessary. A hospital interne got to know a nurse, whom he of course addressed by her surname, and in his work saw her daily for about a year. They later got more intimate and he now experienced great difficulty in recalling her surname so as to address envelopes to her. On one occasion he was unable to write to her for three weeks; recourse to her letters was of no use, for she always signed only her Christian name in them. Investigation of the matter brought to light the fact that her Christian name was the same as that of a girl he had previously jilted, and also of another girl he had been passionately in love with throughout his boyhood. This name he could not forget. What had happened was that he had successively transferred his affections from one girl to the other, the three being unconsciously identified in his mind. He was thus always true to his love, and did not wish to recall any fact, such as the different surname, that would tend to remind him of his faithlessness. The surnames in no way resembled one another.

Brill¹ relates the following example from his own experience. When working at Zurich he wished to recall the name of an old patient of his, on whose case he had specially worked for some months, but was totally unable to do so. He had painstakingly prepared an account of the case for publication, but at the last moment his chief intervened, and decided to report it before a local society. He was unexpectedly prevented from doing so, and Brill was sent to read the paper at the meeting, this being credited to the chief. In trying to recall his patient's name, the name of another patient, Appenzeller, who was suffering from the same disease, persistently presented itself. In the psycho-analysis undertaken one apparently irrelevant memory kept recurring over and over again. This was an actual scene, in which the chief in question had aimed with a shot-gun at a rabbit, and had missed, to the amusement of Brill and the bystanders. The sought for name ultimately flashed up—Lapin (rabbit), the patient being a French-Canadian. The example is instructive in illustrating the associative replacement-formations that come to the mind instead of the proper memory. The sound of the first part of Appenzeller's name resembles the French pronunciation of Lapin, and the scene that kept recurring, the failure of the chief to bag the rabbit, symbolized the whole incident that was the cause of the inhibition.

¹A. A. Brill: A Contribution to the Psychopathology of Everyday Life. Psychotherapy, 1909. p. 9.

The following instance is rather more complex, but shows how fine are the threads connecting unconscious mental processes. A lady was unable to recall the Christian name of a near friend. The full name was Isabell Brown, but she could only recall the surname; instead of the other the name Isidore presented itself, to be at once rejected as incorrect. Thus the failure in memory consisted only in the replacement of the syllable Bell by Dore. I asked her to associate to the word Brown, and the two names Owlie and Leen at once came to her mind. It will be noticed that the first two letters of the first word and the last one of the second word are contained in Brown; the only foreign ones in each case form the syllable "ly" in pronunciation, a fact to be borne in mind. The two words were pet names of two common friends, who used to live together with the subject, and it was in their company that she used to see Miss Brown. Concerning the first one she said that she was at present pregnant for the first time, and that she was anxious as to the outcome, because certain characteristics in her figure had led her to suspect that pelvic narrowing might give rise to difficulties in the confinement. She also mentioned another friend, Dora D., who had similar characteristics, and Isadora D., a famous dancer, whom she knew personally, and whose perfect figure she greatly admired. The name Isidore, which it will be remembered was the replacement-memory, reminded her of the poem by Edgar Allan Poe, Beautiful Isidore *Lee* (ly). I told her that the correct name of the poem was Beautiful Annabel Lee; some inhibition was therefore acting against the syllables Anna and Bell. Thought of the name Annabel brought to her mind the name of Owlie's sister Annie Sybil, which is a sound-contraction of Anna Isabell, and at once Miss Brown's proper name Isabell, which I personally did not know, came to her mind. The subject had recently had a painful quarrel with Annie Sybil, in which also the latter's sister had unfortunately become involved; she had always thought it a pity that the sister she disliked had a better figure, and was more suited for matrimony, than the one she was so fond of. There were thus two painful thoughts at the bottom of the amnesia, one the anxiety about Owlie's confinement, and the other that in this respect the disliked sister was more favorably situated.

The names first recalled by the subject, namely, Isidore Brown, one incorrect, the other correct, were both directly associated to the syllable "ly." The suppressed syllable was "Bell." In view of the fact that the word "belly" summarized the whole situation, it is difficult to avoid the inference that the amnesia for the syllable "Bell" had thus

proceeded: One must suppose that the thought of Miss Isabell Brown had unconsciously reminded the patient of their common friend and her sister; the diphthong in the surname further is identical with that in the former's name, Owlie, and the Christian name resembles the second part of the latter's name, Annie Sybil. The first part of the latter name, Annie, reminded her of "Beautiful Annabel Lee," making the word "belly" which symbolized the painful thoughts in question. These thoughts nevertheless came to expression in the false replacement-memory. First the accent was shifted from the first syllable, "bell," of the objectionable word to the second, "ly," which was also the second syllable of Owlie's name. This, however, was unsuitable for forming a name by being added to the remembered part "Isi," so that a further shifting took place in which it was replaced by "dore." Dora was the name of a friend with similar characteristics to Owlie's, but in combination with "Isi" it was the name of another person, Isadora D., who was strikingly free from them. The subject, therefore, invests her friend with the beautiful and healthy attributes of the famous dancer. One might even go farther and surmise that the reason why Dore had appeared rather than Dora was because the word "door," which is constantly used symbolically for any exit (for instance, of the body, as in the Song of Songs) was better adapted to symbolize the suppressed complex than the word Dora is. To many readers this reconstruction will probably appear as too fine-spun. In my opinion, however, they underestimate the combination of delicacy and rigor with which unconscious and foreconscious processes are determined, a conclusion which can readily be confirmed by a painstaking study of similar material.¹

A simple illustration of the way in which a strong affect will cleave to a name, and be transferred to any other person bearing the same or similar name, is afforded by Shakspeare in Julius Cæsar (Act. III Sc. 3):

First Citizen. Your name, sir, truly.

Cinna. Truly, my name is Cinna.

First Citizen. Tear him in pieces, he's a conspirator.

Cinna. I am Cinna the poet; I am not Cinna the conspirator.

Second Citizen. It is no matter; his name's Cinna; pluck but his name out of his heart, and turn him going.

A field in which significance is apt to be intuitively attributed to the forgetting of names, is that where our own are for-

¹In the *Zentralbl. f. Psychoanalyse*, Jahrg. 1, Heft 9, this analysis is carried to a farther stage.

gotten. Few people can avoid feeling a twinge of resentment when they find that their name has been forgotten, particularly if it is by some one with whom they had hoped or expected it would be remembered. They instinctively realize that if they had made a greater impression on the person's mind he would certainly have remembered them again, for the name is an integral part of the personality. Similarly, few things are more flattering to most people than to find themselves addressed by name by a great personage where they could hardly have anticipated it. Napoleon, like most leaders of men, was a master of this art. In the midst of the disastrous Campaign of France, in 1814, he gave an amazing proof of his memory in this direction. When in a town near Craonne he recollected that he had met the mayor, De Bussy, over twenty years ago in the La Fère regiment; the delighted De Bussy at once threw himself into his service with extraordinary zeal. Conversely there is no surer way of affronting some one than by pretending to forget his name; the insinuation is thus conveyed that the person is so unimportant in our eyes that we cannot be bothered to remember his name. This device is often exploited in literature. In Turgenev's *Smoke* (p. 255) the following passage occurs. "'So you still find Baden entertaining, M'sieu—Litvinov.' Ratmirov always uttered Litvinov's surname with hesitation, every time, as though he had forgotten it, and could not at once recall it. In this way, as well as by the lofty flourish of his hat in saluting him, he meant to insult his pride." The same author in his *Fathers and Children* (p. 107) writes, "The Governor invited Kirsanov and Bazarov to his ball, and within a few minutes invited them a second time, regarding them as brothers, and calling them Kisarov." Here the forgetting that he had spoken to them, the mistake in the names, and the inability to distinguish between the two young men, constitute a culmination of disparagement.¹ Falsification of a name has the same significance as forgetting it; it is only a step towards complete amnesia. The word-contamination in this instance shows a striking psychological intuition of the process termed by Freud "identification;" it indicated that in the Governor's eyes the characteristics of the young men were so little marked, and the men so unimportant, that he did not think it worth while to make the effort of differentiating one from the other. Sensitiveness about the correct

¹In literature disparagement is often indicated by the forgetting of other matters besides names. Thus in Bernard Shaw's "*Cæsar and Cleopatra*," Cæsar's indifference to Cleopatra is depicted by his being vexed, on leaving Egypt, at having forgotten something he has to do; finally he recollects what it is—to say Good-bye to Cleopatra.

spelling of one's name is extremely frequent; we all know the profound difference that members of Scottish clans see between Mc and Mac, and a practical psychologist realizes the importance of being sound on the matter every time he writes such a name. I had thought personally that I was free from such sensitiveness until a little occurrence some time ago taught me the contrary. An article of mine had been published in a German journal; only my surname was printed, with the letters M. D. (which are not used professionally in Germany) attached, as if they were the initials. The same morning I had occasion to fill up a lunacy certificate, and was surprised at the secretary laughing when I handed it in; I had signed it with my Christian name only, thus compensating for the omission in the article. This sensitiveness has sometimes deeper roots than mere personal self-esteem; Stekel¹ has traced it to infantile complexes relating to the giver of the name—the father.

The following two instances in my own experience are similar to those quoted from Turgenev. The first relates to Mr. Mayo Robson, the eminent gastro-intestinal surgeon, after whom was named a bobbin he had invented for the operation of entero-anastomosis. Another surgeon, almost equally eminent in the same field of work, and living in the same town, remarked one day in a lofty and contemptuous manner, "This patient had previously been unsuccessfully operated on by a man called Rayo Bobson, or Bayo Dobson, or some such name." His motive was evident, and, of course, quite conscious. In the second instance the mistake in the name was quite unconsciously made as the result of a falsification of memory, but the significance was very similar. It was at a university graduation ceremony, where a number of visitors were present arrayed in multi-colored and imposing robes. Those so attired formed a procession in double file. A friend of mine, a foreigner, remarked as Professor Titchener passed, "Let me see, who is that? Is n't it Kitchener?" Many would be inclined to see no significance in the mistake, although my friend knew the names of Lord Kitchener and Professor Titchener fairly well. I have, however, to add these two additional facts. A few minutes before, while talking about experimental psychologists in general, he allowed himself to make the scurrilous remark that in his opinion they should be called the pantry-cooks of psychology on account

¹Stekel: Warum sie den eigenen Namen hassen. Zentralbl. f. Psychoanalyse, Jahrg. 1, Heft 3, S. 109. See also his article, Die Verpflichtung des Namens. Zeitschr. f. Psychother. u. med. Psychol. Feb., 1911. Bd. III. S. 110.

of their menial field of work; the passage from "cook" to "kitchen" is obvious. Secondly he had also commented on the martial appearance of this dazzling procession, and I can readily imagine his being especially struck by Professor Titchener's soldierly bearing. It is difficult to avoid the inference that these two trends of thought, present in his mind so recently, played their part in the falsification of the name, which thus betrayed his private opinion of the field of work in which Professor Titchener¹ is so eminent.

Many people have a strikingly bad memory for names, even when their memory is otherwise good. This is generally explained by saying that proper names are among the latest acquired knowledge, so that our memory of them is especially fragile; in accordance with the law of dissolution these memories are among the first to be lost, a process that constitutes one of the most characteristic signs of approaching senility. This explanation is difficult to harmonize with two facts, first that in many cases the memory is weak in this connection when it is notably good in regard to other more complex and later acquired matters, such as scientific formulæ, and so on, and secondly that the characteristic in question is much more pronounced with some people than with others. When the opportunity of making a psycho-analysis with some one of this type presents itself two other matters are brought to light with considerable constancy, namely, that for various reasons the person's own name has acquired an unusual psychical significance, so that it becomes invested with the feeling-tone of the whole personality, and that there is a strong ego-complex present. It would seem, therefore, that the general inability to bear other people's names in mind is an expression of an excessively high estimation of the importance of one's own name and of oneself in general, with a corresponding indifference to or depreciation of other people. In my own experience I have most often found this characteristic with people having either an extremely common or an extremely rare name, both contingencies leading to undue sensitiveness in the matter, but I cannot put this forward as being a general rule. It further seems to me probable that the increasing difficulty of retaining names that is such a frequent accompaniment of advancing years, may in part at least be attributed to the growing self-esteem brought by success and by cessation from the turmoils and conflicts of youth.

¹I trust that Mr. Robson and Professor Titchener will pardon my sacrificing the personal privacy of their names in the cause of science. I have purposely selected, from a large number of similar instances, two in which the contrast between a rare individual disparagement and an otherwise universal respect is especially striking.

Falsification of memory, as was indicated above, is closely related to forgetting, and is influenced by the same motive. A common instance is the mistakes made with regard to the day of the week. Some one who is eagerly anticipating an event at the end of the week is very apt to think it is Wednesday when it is only Tuesday, and so on; their impatience at the slowness with which the week is passing manifests itself as an error—in the desired direction—as to the present date.

Other mental operations, besides recollecting, may be falsified in the same way, a process designated by Freud as an "*Irrtum*." Several examples related elsewhere in this paper might be classified in this group, so that one here will suffice. I was buying some flowers, and put two dollars, the exact price of them, on the counter. While they were being got ready, I changed my mind about one bunch, and told the woman serving me to leave it out; it should be said that she was the owner of the shop. On taking the money a few moments later she said, "that bunch cost forty cents, so that will make two dollars forty." Her wish that I were making the order larger instead of smaller was probably concerned in the mistake.

A few concluding remarks may be added on this mechanism of forgetting. The main points may be summarized in the statements that forgetting is often determined by a painful mental process (*Unlust*) of which the subject is unaware, either at the time only or permanently; that this inhibiting mental process may be a counter-will to recollecting the matter in question or may be associated to this in a more complex way; and that a false memory presenting itself in the place of the true is a symbolic substitute of this, standing in associative connection with it. Two general considerations indicate that acts of forgetting, of the type illustrated above, are not, as is commonly supposed, accidental or due to chance. First is the fact that the same one tends to be repeated. If we forget to carry out a given intention, or are unable to recall a given name, the failure is apt to recur, thus suggesting that it has a specific cause. Secondly is the fact that in at least two spheres of life it is universally recognized that remembering is under control of the will, so that a failure to remember is regarded as synonymous with a not wanting to remember. Freud¹ writes: "Frauendienst wie Militärdienst erheben den Anspruch, dass alles zu ihnen Gehörige dem Vergessen entrückt sein müsse, und erwecken so die Meinung, Vergessen sei zulässig bei unwichtigen Dingen, während es bei wichtigen Dingen ein Anzeichen davon sei, dass man sie wie

¹Freud: *Op. cit.*, S. 83.

unwichtige behandeln wolle, ihnen also die Wichtigkeit abspreche." A soldier who forgets to perform a given duty is punished regardless of the excuse. He is not allowed to forget, and whether his not wanting to perform the duty is openly expressed, or indirectly, as by his forgetting, is considered by his officer as comparatively irrelevant. The standard set by women is equally severe; a lover who forgets his lady's wishes is treated as though he openly declared them unimportant.

III. LAPSUS LINGUÆ

The everyday occurrence of the defect in psycho-physical functioning popularly known as a slip of the tongue has not received much attention from psychologists. The attempts made, by Meringer and Mayer and others, to explain on phonetic grounds the particular mistake made have signally failed, for on the one hand many cases are to be observed where no phonetic factors are in operation, and on the other hand careful study shows that such factors are at the most accessory or adjuvant in nature, and are never the essential cause.

According to Freud the word said in mistake is a manifestation of a second suppressed thought, and thus arises outside the train of thought that the speaker is intending to express. It may be a word, or phrase, entirely foreign to this train of thought, being taken in its entirety from the outlying thought, or it may be a compromise-formation, in which both come to expression. In the latter case the false word may be a neologism; a common example of this is where a speaker, intending to use the word "aggravating," says "How very aggravoking," the word "provoking" having intruded itself; many malapropisms are formed in this way, being the result of uncertainty as to which is the most appropriate word.

The secondary thought that thus obstructs itself on the intended speech may, like the motives of repressive forgetting, be of two kinds: (1) a general counter-impulse (*Gegenwillen*) directed immediately against the speech, or (2) another thought accidentally aroused by it. In the latter case it can represent either a continuation of a theme previously in the speaker's mind, or a thought aroused, through a superficial association, by the theme that is intended to be spoken; even when it represents a continuation of a previous theme it will generally, if not always, be found that there is some association between this and the theme of the speech. It will readily be understood that in many cases the disturbing thought is not evident, but can be revealed only by investigation, sometimes a searching psycho-analysis being necessary.

Cases where the disturbing thought is a direct counter-impulse are usually easy to interpret. One instance will suffice. A President of the Austrian Reichstag finished his introductory remarks by declaring the session closed, instead of opened; as the particular session promised nothing but fruitless wrangles, one can sympathize with his wish that it were already at an end.

Some cases where the disturbing thought is nearly related to the intended theme are equally simple. A French governess in Dr. Stekel's family¹ asked his wife that she might retain her testimonials, saying: "Je cherche encore pour les après-midis, pardon, pour les avant-midis." The slip betrayed her feeling of dissatisfaction with the afternoon engagement, and her intention to look for another situation for the afternoons as well as the mornings, an intention she proceeded to carry out.

A friend of mine was driving his motor-car slowly and cautiously one day, when a cyclist, who was riding with his head down, furiously, and on the wrong side of the street, ran into him and damaged his bicycle. He sent in a bill for \$50.00, and, as my friend refused to pay, he sued him in court. When I enquired as to the result of the action my friend said, "the judge reprimanded the prisoner for careless riding." I corrected him, "You mean the plaintiff, not the prisoner." "Well," he replied, "I think the fellow should have been arrested for furious riding."

A lady when speaking of Bernard Shaw's works said to me, "I think very highly of all my writings," instead of "all his writings." She was an amateur writer of short stories.

An unmarried man, a patient, remarked, "my father was devoted to my wife." He meant, of course, either "his wife" or "my mother." This is a typical instance of a lapsus that would pass as being entirely accidental and devoid of significance. I must add, however, that one of the main causes of the patient's neurosis was an unconscious incestuous attachment to his mother, so that his unsuppressed thoughts on the subject of the remark would run in full, "My attitude towards my mother is the same as that of my father." No alteration is too slight to have a meaning. The instance narrated above, in which the first letter only of Titchener's name was replaced by a K,² belongs to the subject of lapsus linguæ equally as much as to that of forgetting.

¹Related by Freud. *Op. cit.* S. 48.

²This replacing of the initial letter of a word by that of another word, typically from the same sentence, is known in Oxford as a Spoonerism, on account of a distinguished professor who had the habit of committing the particular slip.

Such self-betrayals as those just related sometimes afford valuable insight into character and motive. I was present at the International Congress in Amsterdam when the following curious episode occurred. There was a heated discussion regarding Freud's theory of hysteria. One of the most violent opponents, who is noted as having worked long and fruitlessly on the subject of hysteria, was grudgingly admitting the value of the earlier work of Breuer and Freud—the conclusions of which he had himself discovered to be true—as a prelude to a vehement denunciation of the dangerous tendencies of Freud's later work. During his speech he twice said, "Breuer and *ich* haben bekanntlich nachgewiesen," thus replacing Freud's name by his own, and revealing his envy of Freud's originality.

The following example is more complicated. In talking of the financial standards so prevalent in modern civilization I said, "In yesterday's newspaper there were the headings 'Ten million dollar fire in Halifax; six lives lost.'" It was at once pointed out to me that I had said Halifax instead of Bangor, Maine. Analysis brought the following free associations. Until a few years ago I was disgracefully ignorant of the existence of Bangor, Maine, and I remember in college days being puzzled by the reference to Maine in the well-known student song "Riding down from Bangor," as in my ignorance I supposed that this related to Bangor, the university town of Carnarvonshire, Wales. The name Bangor essentially stands in my mind for the original Bangor. It brought up a memory of the recent controversy as to whether the new National Welsh Library should be established at Bangor, at Swansea (my home), or at Cardiff (the university town where I studied). This reminded me of interests I have in the contents of this library, in Celtic mythology, which naturally carried me to the valuable library of mythological books that I possess myself. Then I remembered that what had especially struck me in reading about the recent fire was the fact that a valuable collection of books had been destroyed in it, and that this had made me enter a note not to forget to renew my fire insurance, which had recently lapsed, before leaving in the coming week for a fortnight's visit to the United States.

The meaning of my lapsus is beginning to emerge. A library fire at Bangor was too near home for my peace of mind, and my unconscious had consolingly relegated it to some other spot. The next problem is to discover the motive for the replacement of Bangor by Halifax, a process that was greatly "over-determined." Maine is from its geographical position closely associated in my mind with the Maritime

Provinces of Canada, and only on the preceding day a Canadian had been demonstrating to me on a map, for the *n*th time, how Maine should rightfully have formed part of these Provinces. Still that does not explain why I selected Halifax rather than St. John, the other town I know the name of in the Maritime Provinces. One reason doubtless was the fact that at the time I was treating a patient from Halifax, Nova Scotia, who had recently been telling me that the houses there were mostly built of wood, and therefore were exposed to the danger of fire. The name Halifax, however, is better known to me as an English euphemism for Hell, as in the expression, "Go to Hal-ifax." This called up the memory of half-forgotten childhood fears, for, like most Welsh children, I was carefully nurtured with a proper dread of what was called "the burning fire;" as I grew up I was comforted to learn the groundlessness of this particular dread. My slip of the tongue, therefore, registered my desire that any library fire should be in some other place than in my home, and if possible in a non-existent locality.¹

An example, for which I am indebted to Dr. A. A. Brill, is peculiar in that the slip of the tongue represented a resolution in opposition to the conscious intention. A man, who on account of homosexual practices was in constant fear of coming into conflict with the law, invited two lady friends to spend an evening at the theatre. They expressed a wish to see a play called "Alias Jimmy Valentine," which deals largely with convicts and prisons. He was far from comfortable at the idea of spending an evening with such thoughts, but could not well avoid it. On getting into the cab to drive to the theatre, however, he accidentally gave the driver the name of another theatre, and did not notice the mistake until they arrived there, when it was too late to rectify it. At this theatre the play was about the cleverness with which a daughter outwitted her selfish old father. It was not without significance that the subject's attitude towards his own father was one of pronounced hostility, so that his slip of the tongue had the effect of exchanging an evening with a painful topic for one with a topic that he greatly enjoyed.

Several non-scientific writers before Freud had noted the psychological significance of accidental slips of the tongue. Freud² quotes examples of this from, for instance, Brantôme and Wallenstein. Shakspeare himself furnishes a beautiful one in the Merchant of Venice (Act. III Sc. 2). It occurs in the scene where Portia is expressing her anxiety lest the fa-

¹This analysis led further into previously unconscious thoughts, which are too intimate for me to describe here.

²Freud: *Op. cit.*, S. 50, 58.

vored suitor should fare as badly as the distasteful ones in the hazard set for them by her father. She wants to tell Bassanio that in the event of his failure she would nevertheless belong to him, but is prevented by her promise to her father. In this mental discord she speaks:—

There is something tells me (but it is not love),
 I would not lose you; and you know yourself
 Hate counsels not in such a quality.
 But lest you should not understand me well,
 (And yet a maiden hath no tongue but thought)
 I would detain you here a month or two,
 Before you venture for me. I could teach you
 How to choose right, but then I am forsworn;
 So will I never be; so may you miss me;
 But if you do, you 'll make me wish a sin,
 That I had been forsworn. Beshrew your eyes,
 They have o'erlooked me, and divided me:
 One half of me is yours, the other half yours,—
 Mine own, I would say; but if mine, then yours,
 And so all yours.

Rank¹ comments on this passage: "Gerade das, was sie ihm also bloss leise andeuten möchte, weil sie es eigentlich ihm überhaupt verschweigen sollte, dass sie nämlich schon vor der Wahl *ganz* die seine sei und ihn liebe, das lässt der Dichter mit bewundernswertem psychologischem Feingefühl in dem Versprechen sich offen durchdrängen und weiss durch diesen Kunstgriff die unerträgliche Ungewissheit des Liebenden sowie die gleichgestimmte Spannung des Zuhörers über den Ausgang der Wahl zu beruhigen."

Our greatest novelist, George Meredith, in his masterpiece, *The Egoist*, shows an even finer understanding of the mechanism. The plot of the novel is, shortly, as follows: Sir Willoughby Patterne, an aristocrat greatly admired by his circle, becomes engaged to a Miss Constantia Durham. She discovers in him an intense egoism, which he skilfully conceals from the world, and, to escape the marriage, she elopes with a Captain Oxford. Some years later Patterne becomes engaged to a Miss Clara Middleton, and most of the book is taken up with a detailed description of the conflict that arises in her mind on also discovering his egoism. External circumstances, and her conception of honor, hold her to her pledge, while he becomes more and more distasteful in her eyes. She partly confides in his cousin and secretary, Vernon Whitford, the man whom she ultimately marries, but, from a mixture of motives, he stands aloof.

¹Otto Rank: Zentralbl. f. Psychoanalyse. Heft 3, S. 110.

In a soliloquy Clara speaks as follows: "If some noble gentleman could see me as I am and not disdain to aid me! Oh! to be caught out of this prison of thorns and brambles. I cannot tear my own way out. I am a coward. A beckoning of a finger would change me, I believe. I could fly bleeding and through hootings to a comrade. . . . Constantia met a soldier. Perhaps she prayed and her prayer was answered. She did ill. But, oh, how I love her for it. His name was Harry Oxford. . . . She did not waver, she cut the links, she signed herself over. O brave girl, what do you think of me? But I have no Harry Whitford, I am alone" "the sudden consciousness that she had put another name for Oxford, struck her a buffet, drowning her in crimson."

The fact that both men's names end in "ford" evidently renders the confounding of them more easy, and would by many be regarded as an adequate cause for this, but the real underlying motive for it is plainly indicated by the author. In another passage the same lapsus occurs, and is followed by the hesitation and change of subject that one is familiar with in psycho-analysis when a half-conscious complex is touched. Sir Willoughby patronizingly says of Whitford: "False alarm. The resolution to do anything unaccustomed is quite beyond poor old Vernon." Clara replies: "But if Mr. Oxford—Whitford . . . your swans coming sailing up the lake, how beautiful they look when they are indignant.¹ I was going to ask you, surely men witnessing a marked admiration for some one else will naturally be discouraged?" "Sir Willoughby stiffened with sudden enlightenment."

In still another passage Clara by another lapsus betrays her secret wish that she was on a more intimate footing with Vernon Whitford. Speaking to a boy friend she says: "Tell Mr. Vernon—tell Mr. Whitford."

In relation to these two literary passages I made a personal slip of the tongue that illustrates the probity of the unconscious mind as contrasted with the duplicity of the conscious one. Expounding the subject of lapsus linguæ to some one I said that I had come across two interesting literary examples, in Meredith's *Egoist* and in Shakspeare's *Love's Labour Lost*; when detailing the second I noticed that I had named the wrong play. Analysis of the mistake brought the following memories. On the preceding day, while talking of the sources of Shakspeare's plots, I had made the remark that the only one he had not taken from previous authors was that con-

¹The nature of the change of the subject here accurately betrays the content of the underlying affect, *indignation* at Patterne's disparagement of Whitford, just as a mediate association reaction indicates the nature of the complex stimulated.

tained in *Love's Labour Lost*. Some six months before, Professor Freud had told me that he had heard from Dr. Otto Rank that there was in the *Merchant of Venice* an example of *lapsus linguæ* attributed to the disturbing influence of a suppressed thought, but he could not tell me where it occurred. On looking back I realize that I felt just a touch of pique, though I did not pay any attention to it at the time, at not having observed it myself, and took the first opportunity to re-read the play, when of course I came across the example. The one in the *Egoist* I had really observed myself. My statement that I had discovered the two examples in question was therefore only three parts true. The fact, which I had suppressed,¹ that Dr. Rank deserved some credit, leaked through to external expression in my error of naming the wrong play, substituting Shakspeare's only original one. An interesting feature of the example is the fact that a few minutes before I had been relating how a man not over-scrupulous in the matter of priority had betrayed his dishonesty in a treacherous slip of the tongue. No doubt deeper factors than interest in scientific priority were also operative in my own case, such as rivalry and an "English" complex, both of which are matters that play a very subordinate part in my conscious mental life.

IV. LAPSUS CALAMI

The introductory remarks made on the subject of slips of the tongue apply almost literally to slips of the pen. One principal difference is that the delay interposed by the mechanical acts of writing enables disturbances of co-ordination to occur with especial readiness, as can be illustrated by a glance over any author's manuscript. The necessity for numerous corrections indicates that, whether owing to the intricacy of the subject-matter or to a lack of clearness in the author's mind, a harmonious flow is far from being attained. General perplexities mirror themselves in half-conscious hesitations as to the choice of individual words. Thus, a correspondent, who could not decide as to the advisability of a given proposal, wrote to me that it might turn out to be "umpracticle," evidently a contamination of "impracticable" and "unpractical."

A field of frequent errors is that of dates. Many people continue to write the date of the previous year throughout a great part of January. Not all such mistakes are due to the fixation of habit, as is readily assumed; sometimes they signify a disinclination to accept the fact that yet another by-gone

¹Naturally I excused this to myself on the ground that pedantic accuracy is uncalled for in conversation; but the facts remain.

year has brought them nearer to old age, a reflection that is apt to be prevalent at the turn of the year. Regrets that such and such a date is already past, or impatience that it has not yet arrived, are common motives of such unconscious mistakes. A student dated a letter to me April 11, 1911, instead of April 22. An examination was due in the first week of May for which he was very unprepared, and I attributed his slip to the wish that there was twice as much time ahead of him in which to get ready. That the date he actually wrote was the 11th was no doubt influenced by the presence of these ciphers at the end of 1911, but it is to be noted even in this connection that his mistake consisting in writing them earlier than he should, *i. e.*, in putting the date earlier. As with the phonetic factors entering into slips of the tongue, the fact that the part wrongly written occurs elsewhere in the same line only predisposes to the mistake; such factors do not cause the mistake, they only make it easier to assume that particular form.

For the following example I am indebted to Dr. A. A. Brill. A patient wrote to him on the subject of his sufferings, which he tried to attribute to worry about his financial affairs induced by a cotton crisis: "my trouble is all due to that damned frigid wave;¹ there is n't even any seed." What he really wrote, however, was not "wave" but "wife." In the bottom of his heart he cherished half-avowed reproaches against his wife, on account of her sexual anæsthesia and childlessness, and he dimly realized, with right, that his life of enforced abstinence played an important part in the genesis of his symptoms.

As with slips of the tongue, no mistake is too slight to be significant. The following four are instances, selected from a considerable number of similar ones, in which it consisted only in the replacement of one letter by another.

A correspondent of mine had published a scientific paper on a sexual subject, and was writing to me about a virulent criticism of it that had appeared; the critic had used such passionately denunciatory language as to make it evident that the topic of the paper had aroused some strong personal complex. My correspondent's first sentence was "Have you seen X's satyirical criticism of my paper?", plainly indicating by his unconscious substitution of "y" for "i" his estimate of the nature of the criticism.

Some two years ago I was writing to an old friend, whom I had always called by his surname. On account of family ties it became more appropriate to address him by his Christian

¹Meaning in the money-market.

name, and, after a momentary embarrassment natural under the circumstances, I took up my pen and began, "Dear Fred." To my amazement, however, I saw that I had slipped in a "u" before the final letter of the name. This may seem a very trivial mistake, and due to the similarity of the two words, but a psycho-analytic conscience tends to be more unsparing in the criticism of its owner, as it is more sparing in that of others. Two memories at once rushed to my mind. One was of a dream I had had two years before, at a time when I was debating with myself whether it would be politic openly to defend the Freudian principles, the truth of which my experience had made me accept. In the dream I was in a swiftly-moving motor-omnibus, the driver of which was a composite figure (*Sammelperson*),¹ bearing mostly the lineaments of my friend. An angry crowd surrounded us, and threatened the driver for "going so fast." It became necessary for me to decide whether to stand aloof or to side with the driver, and I did the latter. I need not give the other details of the dream, but the analysis showed it to be a presentation of my waking dilemma, the driver being a replacement-figure for Professor Freud. I had recently been taken for a long motor ride by my friend, who by the way has a German surname, and though at first I had qualms as to the recklessness of his driving I soon perceived, to my relief, that this was only apparent and that he was really an exceedingly skilful and reliable driver. Before the incident of the *lapsus calami*, therefore, he had long been unconsciously associated in my mind with Professor Freud. The second memory was of a letter I had recently written to a Canadian Professor of a subject allied to my own. On coming to Canada I had felt very awkward and constrained at the American custom of formally prefacing a man's title to his name when addressing him, and it was a long time before I got accustomed to being spoken to by both younger and older colleagues as Dr. Jones or as Doctor. It embarrassed me to have to speak to even fairly intimate friends in this way, and in the case of the gentleman in question I frankly told him, in the letter referred to above, that my English prejudices would not let me do it with any degree of comfort. As he was some fifteen years older than myself I wondered afterwards whether he might resent a younger man taking the initiative of addressing him simply by his surname. The slip of the pen now began to take on a different aspect, and I was obliged to recognize in it the manifestation of a snobbish wish that I was on sufficiently close personal terms with Professor Freud to allow such a

¹See *Amer. Journ. of Psychol.*, April, 1910, p. 287.

familiar mode of address. I feel certain that no thought of the kind had ever entered my consciousness, to which it is quite strange, though my intense reaction of shame convinced me of the reality of its existence. The circumstances of the slip of the pen were extraordinarily favorable to its occurrence, the similarity in the names, the previous identification of the men, the occasion of the letter following so soon after the other one, and so on. If it were not for this, I hardly think that such a deeply repressed wish could have come to expression, at least not so flagrantly.

I am indebted to Dr. A. A. Brill for the following personal example. Although by custom a strict teetotaler, he yielded to a friend's importunity one evening, in order to avoid offending him, and took a little wine. During the next morning an exacerbation of an eye-strain headache gave him cause to regret this slight indulgence, and his reflections on the subject found expression in the following slip of the pen. Having occasion to write the name of a girl mentioned by a patient he wrote not Ethel but Ethyl.¹ It happened that the girl in question was rather too fond of drink, and in Dr. Brill's² mood at the time this characteristic of hers stood out with conspicuous significance.

Some three years ago I was writing to a friend in England, and gave the letter to a member of my family to post. Fortunately she noticed I had made a mistake in the address, having written as the street number 19 instead of 55. The two numbers do not even resemble each other, so that the customary explanations are here more than ever in default. I will relate a few of the associations as they occurred. The name of the street, Gordon St., brought "Gordon Highlanders—the Highlands—the thought that my friend is an ardent mountaineer—the thought that Professor Freud is very fond of the mountains—*Berg* (=Mountain)—*Berggasse*, the street in Vienna in which Professor Freud lives,—the number of his house, 19." The friend's name, Morris brought "morris—dancers—maypole—phallus—sex—Professor Freud's works on sexual subjects." In desperation I started again with Gordon, which now brought "the regiment called the Gay Gordons—gay women (the London euphemism for prostitutes)—the

¹Ethyl alcohol is of course the chemical name for ordinary alcohol.

²In writing my manuscript I made the slip of replacing the word Brill by that of Bree, the name of another medical friend. The mistake is evidently a contamination derived from the word-picture of "Brill on the spree," and is determined by the memory of tenuous jests relating to Berlin on the (river) Spree; both the vowel and the consonants of Brill are contained in the word Berlin. It is only right to add, however, that the thoughts of both Dr. Brill and Dr. Bree are intimately connected in my mind with Berlin in ways that discretion prevents me from describing.

German equivalent, *Freudenmädchen*—a cheap joke I had heard in Germany in this connection on Professor Freud's name;" as a matter of fact I had on the previous evening read a passage in his *Traumdeutung* where he refers to jokes on names. Turn which way I would I arrived at the same end-point, and I began to suspect that this was not chance. It might be said that for some reason or other, whether from the number coinciding with that in the *Berggasse* or what not, thoughts relating to Professor Freud were at the time occupying my mind to the exclusion of all else, in reply to which I have to say that I do not find this so in other analyses, and that in my experience, whenever free, unforced associations constantly lead in the same direction there is some good reason for it; in such cases there is invariably some essential, significant connection between the starting-point and the end reached. Further, the more far-fetched and strained the associations appear, as in this example, in other words the more superficial they are, the more important is the underlying essential connection found to be. This conclusion, clearly demonstrated in Jung's experimental work, was fully confirmed in the present instance. Although I could see no possible connection between my friend and Professor Freud, of whom he knew nothing, I was led to investigate the contents of the letter I had sent him. To my amazement I found that the main feature of it could be applied to Professor Freud in the same sense, and that I must unknowingly have harbored a wish to send it to him; in the slip in writing I had expressed my unconscious wish to send the letter to another man by addressing the envelope partly to him and partly to the one I consciously intended it to go to. There can be no question as to the intense personal significance of the complex covered by the superficial associations of the analysis, for wild horses would not tear from me the contents of that letter.

Mistakes in addressing envelopes, as in the example just mentioned, are generally manifestations of some disturbing thought that the writer does not mean to express. A young lady was secretly engaged to a medical man, whom we will call Arthur X. She addressed a letter one day not to Dr. Arthur X., but to Dear Arthur X, thus expressing her desire to let all the world know of their relationship.

Not long ago I was treating a case of exceptional interest in a patient who lived some sixty miles from Toronto. On account of the distance the patient, who could not leave his work, was able to visit me only twice a week. I found it impossible to treat him on these conditions, and wrote to tell him so. Instead of writing the name of his town on the

envelope, however, I wrote Toronto, displaying my wish that he were more conveniently situated.

V. MISPRINTS

Misprints may of course arise from errors made by the writer, the editor, the proof-reader, or the printer. From time to time the press records amusing instances of a disagreeable truth unintentionally leaking out in the form of a misprint; in Freud's book several examples of this are related.¹ Unlike the other kinds of failure under discussion one here is rarely in a position to obtain an objective verification of a given interpretation, but sometimes this in itself reaches a high grade of probability. At all events general principles indicate that the mistake made must be determined by personal constellations of whoever made it, and cannot be altogether accidental.

In a recent number of the *Zentralblatt für Psycho-analyse*² the title of a book of Gross' is wrongly given as "Das Freud'sche Ideogenitätsmonument" instead of Ideogenitätsmoment. As both the writer of the article, and the editorial staff (Drs. Adler and Stekel) regard the conception as a monumental one, it is possible that the overlooking of the mistake is to be correlated with this fact.

In a paper of my own on nightmare I wrote the sentence, "The association in general between the sex instinct and the emotions of fear and dread is a very intimate one." This was correctly rendered in the proof, but on the second occasion of reading it the proof-reader was shocked to think that I could make such an obviously outrageous mistake, and altered the word "intimate" to "distant," in which form it appeared in print.

In a brochure of mine that appeared as a German translation a mistake was made of a less unfortunate kind. One of my main theses was that the conception of Hamlet represented a projection of the most intimate part of Shakspeare's personality, and so thoroughly did the translator absorb my view of the identity of the two that, when he came to a passage on the death of Shakspeare's father, he substituted the name Hamlet for Shakspeare and rendered the passage as referring to "the death of Hamlet's father in 1601." The substitution was overlooked in the proof by two other readers thoroughly familiar with the subject.

In the notorious Wicked Bible, issued in 1631, the word "not" was omitted from the Seventh Commandment, so

¹S. 66, etc.

²Jahrg. 5, Heft. 16, S. 197.

that this read, "Thou shalt commit adultery." The possibility is not to be excluded that the editor had a personal interest in the subject of the commandment. At all events he was heavily fined, it being empirically recognized that whether his purpose was conscious or unconscious he was equally responsible for it, and that he had no right, even "accidentally," to impute such commandments to Jahve.

Type-writing, being a form of writing, is subject to the same influence as this. Mistakes made may be due to either a "Verschreiben" or a "Verlesen," in any case being determined by the previous mental constellations of the typist. Thus my typist, having worked long in a lawyer's office, is fond of replacing "illogical" by "illegal," and, being of a very proper turn of mind, makes such mistakes as changing "a vulgar word" to "a regular word." I have found that distinctness of calligraphy is powerless to prevent such mistakes.

One practical aspect of this matter is generally recognized, namely, that accuracy in correcting proofs can be attained only by getting some one else to do it for one. A mistake once made in the manuscript, and then copied, is very apt to get overlooked by the person who made it. The affective blindness that enabled him to make the mistake, or, more strictly, that enabled an unconscious impulse to come to expression, will very likely continue its action by preventing him from recognizing it.

VI. FALSE VISUAL RECOGNITION

In visual perception the same mistakes of affective origin that were discussed in connection with memory are frequently to be observed, and here also they are of two kinds, a failure to see something that for various reasons we do not want to see, and a falsification of perception in the light of personal complexes. Examples of the former kind are very common in connection with reading the newspaper. Thus, just when a relative was crossing the Atlantic last year, I saw in the news-headings that a serious accident had happened to a liner, but I had the greatest difficulty in finding the account of it in the paper, overlooking it again and again.

False perceptions perhaps consist most often in catching sight of one's name where it really does n't occur. As a rule the word that has attracted one's attention is very similar to one's name, containing perhaps the same letters differently arranged. Professor Bleuler¹ relates an example where this was not so, and where, therefore, the essential cause of the mistake must have been of a greater affective intensity; the

¹Bleuler: *Affektivität, Suggestibilität, Paranoia*, 1906, S. 121.

word was really "Blutkörperchen," only the first two letters being common to the two words. He explained it thus: "In diesem Falle liess sich aber der Beziehungswahn und die Illusion sehr leicht begründen: Was ich gerade las, war das Ende einer Bemerkung über eine Art schlechten Stiles von wissenschaftlichen Arbeiten, von der ich mich nicht frei fühlte."

Freud¹ quotes an example from Lichtenberg: "He always mistook "angenommen" for "Agamemnon," so thoroughly had he read his Homer." In searching an American newspaper for English political news at the time of the Navy scare, my attention was caught by the heading "German danger;" on looking nearer I saw that it was "General danger."

Similar observations can be made in regard to the perception of other objects than written matter, and especially with the recognizing of other people. False recognition is quite commonly due to a pervading desire to meet the person in question; a lover who has a rendezvous with his mistress fancies he sees her coming many times over, when really the women he mistakes for her bear only the faintest resemblance to her.

The failure to greet a friend or acquaintance in the street is not always due to not seeing them, and one knows how gradual are the shades between a direct "cut," where one person consciously pretends he does not see the other, and a not seeing that is due to a not wanting to see.² Women intuitively feel that the difference between the two is unimportant, and are as much offended by the one as by the other; some one who thinks highly of them has no right not to see them when they pass.

A striking instance of this affective blindness occurred to me not long ago. It is part of my routine duty to check the invoices for laboratory apparatus as they come in, and hand them over to the assistant superintendent to see that they get paid. On one occasion I had neglected to do this until a small number collected. I then went through them, and took them with me into the assistant superintendent's office. I was very pressed for time, and hoped he would n't be there so that I could simply deposit them on his desk; especially so, as there was a small error in one of them that I had to point out to him, and I realized that his over-conscientiousness would mean a tedious investigation of the error. I felt, however, that I ought to try to find him, and explain

¹Freud: *Op. cit.* S. 64.

²One might invert the familiar proverb and say: "What the heart doesn't grieve over, the eye doesn't see."

the point to him. On going into his office I saw several men there, went up to one of them who had his back to me, and said, "Do you know where Dr. X. is?" To my astonishment he replied, "Why, I am Dr. X." My not recognizing him was facilitated by the fact of his having an unfamiliar hat on, but the actual cause of it I knew well enough.

The phenomenon of "fausse reconnaissance," or "déjà vu," which has perplexed so many psychologists, is closely allied to the same category. Freud has finally solved this riddle,¹ but as the explanation of it is of a more complex order than with the other occurrences under consideration, I shall not go into it here.

VII. MISLAYING OF OBJECTS

It is probable that objects are never accidentally mislaid. The underlying motive manifests itself in two ways, in the act of mislaying the object, and in the subsequent amnesia; in other words a "Verlegen" is a composite of a "Vergreifen" and a "Vergessen," the latter being the main feature. As before, the motive may be a counter-impulse directed against the use of the object, or against an idea associated with the use of it. Instances of both will be given, first of the former.

We are all more apt to mislay bills rather than cheques, and in general objects that we don't want to see rather than those we do. Apparent exceptions to this rule, such as the mislaying of valuable objects, come under the second category, where our objection is not to the thing itself, but to what it can remind us of.

A common experience, which has often occurred to me personally, is the following: Whenever I suffer from the effects of over-smoking, I notice that it is much harder to find my pipe; it has got put behind ornaments or books, and in all sorts of unusual places that it normally does not occupy.

A patient of mine was recently very put out at having lost an important bunch of keys. He told me that he urgently wanted them that afternoon to open the lock of a minute book at a meeting with his auditor and solicitor. I enquired as to the purpose of the meeting. It appeared that an important resolution had been passed at an annual directors' meeting, and that he had omitted to enter it in the minute book. He was the managing director, and it became a question legally whether a certain action could be taken without the formal consent of the other directors, or whether possibly the minute could be subsequently added by private arrangement with them. At all events it was an annoying situation,

¹Freud: *Op. cit.* S. 139.

and I felt sure that his dislike of having to face it was connected with the loss of the key. Further enquiry showed that he had used the keys only once that morning, to open his office desk; after doing this it was his custom immediately to replace them in his pocket, the desk being provided with an automatic closing lock. He had missed the keys as soon as he got into the street car to come to see me, and had telephoned a message for a clerk to search the short distance between his private office and the car line. The surmise was near that he must have flung the bunch into his desk behind some papers, later closing it in the usual way; on telephoning to have the desk forcibly opened, this was found to be correct.

The following example is a little more complicated. A lady had lost the key of a box containing phonograph records, and had thoroughly ransacked her rooms for it many times during six weeks, but all in vain. The records belonged to a correspondence college, and were a means of learning French pronunciation. They had been put away early in the summer, and now, in the autumn, she wanted them for the purpose of renewing her French studies. Her whole heart was not in these, however, for it happened that she was fond of singing and hoped to get accepted in an orchestral choir, the rehearsals of which would leave her no time for other studies. As time went on she despaired more and more of being accepted, and fell back on the French as the next best way of occupying her winter evenings. Soon after her definite rejection by the choir she discovered the lost key, which had been carefully stowed away in the corner of an attic. She recollected locking the box in the early summer, and thinking that she would not need it again for a long time, but had no memory of putting the key away. She was extremely proud of her voice, and had built on her application being successful. Taking up the French studies connoted failure of her hopes. Her inability to find the key thus symbolized her lothness to believe that her vocal reputation would be slighted.

To lose or misplace a present, especially if it happens more than once, is not generally considered a compliment to the giver, and with right, for it often is an unconscious expression of disdain, disregard, or indifference. When a wife repeatedly loses her wedding ring during the honeymoon, it does not augur well for the future happiness of the marriage. Freud relates an example of misplacing where the motive was of this kind, and which, like the last mentioned example, is interesting in regard to the circumstances under which the object was again found. It concerned a married couple who lived rather aloof lives from each other, any marks of tenderness being of a distinctly lukewarm nature; the fault, according to the

husband, lay in the emotional apathy of his wife. One day she made him a present of a book that would interest him. He thanked her for the attention, promised to read it, put it aside, and could n't find it again. In the next six months he made several vain attempts to find it. At the end of this time his mother, to whom he was devoted, got seriously ill, and was very tenderly nursed by his wife. His affection for his wife rapidly increased, and one evening, coming home from the sick bed with his heart filled with gratitude towards her, he went to his desk and, without any conscious purpose, unhesitatingly opened a drawer and took out the lost book.

Leaving things behind one is a common type of mislaying. To do so in the street or in a public conveyance has a very different significance from doing so in the house of a friend. In the latter case it often expresses the person's attachment, and the difficulty he has in tearing himself away. One can almost measure the success with which a physician is practising psychotherapy, for instance, by the size of the collection of umbrellas, handkerchiefs, purses, and so on, that he could make in a month.

VIII. ERRONEOUSLY CARRIED OUT ACTIONS

A secondary suppressed tendency may manifest itself in the disturbance not only of writing, but also of any other conscious motor act, an occurrence Freud terms a "Vergreifen." The intended action is not carried out, or only incorrectly, being entirely or partly replaced by an action corresponding with the suppressed impulse that breaks through. As in the former cases this secondary tendency is associated, either directly or indirectly, with the conscious intention, and the faulty action is customarily explained as being due to "chance," "accident," or "carelessness."

A trite example will perhaps best illustrate the type of occurrence. On starting to open a fresh tin of tobacco I economically reflected that I should first finish the rather dry remains of the previous one. A few minutes later, however, while engrossed in reading, I wanted to refill my pipe, and to my surprise detected myself in the act of opening the new tin, although I had pushed it farther away from me than the other. My checked wish to enjoy the fresh tobacco had taken advantage of my distraction, and so interfered with my conscious intention of filling the pipe from the old tin.

An equally simple example is the following. It is my custom to put scientific journals, as they arrive, on a stool in the corner of my study. On reading them I write on the back the page number of any articles I wish to enter in my refer-

ence books; the journals not so marked are put on top of the files, to be bound at the end of the year, while the others are placed on a pile at one side of my desk. Once a week or so I go through this pile and enter the references, but, whenever I have neglected this for so long that the pile begins to assume formidable dimensions, I find I have a pronounced tendency to put no more there, and to put on the files any fresh journal I read, whether it has articles that should be entered or not. The motive is obvious, to save myself the trouble of having to enter more than I already have to.

A lady went to post some letters which had come for her brother, and which had to be re-addressed and forwarded on account of his absence. When she got home she found the letters still in her hand-bag, but realized that she had posted two letters addressed to herself, which she had opened that morning; they duly arrived on the next day. At the time another younger brother was at home seriously ill with typhoid fever, and she had just written to the elder brother begging him to come home as soon as possible. She knew, however, that on account of urgent business he would not be able to leave immediately, but her posting letters addressed to the home under the impression that she was sending them to her brother, indicated her keen anxiety that he was already there.

A patient came up from the country to get advice about various obsessing ideas that greatly distressed him. He had been recommended to consult two physicians, another one and myself. The other physician told him "not to think about the ideas," and advised him to take a course of physical exercise at a special gymnasium that he kept for the purpose. I of course advised psycho-analytic treatment, which has since cured him. He promised us both that he would think the matter over, and let us know what he decided. That night, on getting home, he wrote to each of us, to the other physician that he could n't yet make up his mind, and to me that he would like to make an appointment to begin the treatment as soon as possible. He put the letters into the wrong envelopes. During the subsequent psycho-analysis it became evident that this "accidental" mistake was unconsciously determined by the spiteful desire to let both the other physician and myself know what his opinion was of the former's advice.

The use of keys is a fertile source of occurrences of this kind, of which two examples may be given. If I am disturbed in the midst of some engrossing work at home by having to go to the hospital to carry out some routine work, I am very apt to find myself trying to open the door of my laboratory there with

the key of my desk at home, although the two keys are quite unlike each other. The mistake unconsciously demonstrates where I would rather be at the moment.

Some years ago I was acting in a subordinate position at a certain institution, the front door of which was kept locked, so that it was necessary to ring for admission. On several occasions I found myself making serious attempts to open the door with my house key. Each one of the permanent visiting staff, of which I aspired to be a member, was provided with a key, to avoid the trouble of having to wait at the door. My mistakes thus expressed my desire to be on a similar footing, and to be quite "at home" there.

Two other everyday sets of occurrences may briefly be mentioned where unconscious disturbances of otherwise intended actions are very frequent. The one is the matter of paying out money, and particularly of giving change. It would be an interesting experiment to establish statistically the percentage of such mistakes that are in favor of the person making them, in comparison with that of the opposite sort.

The second is the sphere of domestic breakages. It can be observed that after a servant has been reprimanded, especially when the reprimand is more than usually unjust in her eyes, is a favorite time for crockery to "come to pieces in her hand." Careless breakage of valuable china, an event that often perplexes the owner as much as it incenses her, may be the product of a number of factors in the mind of the transgressor, class envy of valuable property, ignorant lack of appreciation for objects of art, resentment at having to devote so much labor to the care of what appear to be senseless objects of enthusiasm, personal hostility towards the owner, and so on.

IX. SYMPTOMATIC ACTS

Under the name of "Symptomhandlungen" Freud discusses a series of unconsciously performed actions that differ from the last-mentioned ones in being independent activities, and not grafted on to another conscious one. They are done "without thinking" or "by chance," and no significance is seen in them. Analysis of them, however, shows that they are the symbolic expression of some suppressed tendency, usually a wish. In many instances the action is a complicated one, and performed on only one occasion; in others it is a constant habit that often is characteristic of the person. The mannerisms of dress, of fingering the moustache or clothes-buttons, the playing with coins in the pocket, and so

on, are examples of this kind; they all have their logical meaning, though this needs to be read before becoming evident.

Different ways of occupying the hands often betray thoughts that the person does not wish to express or even does not know of. It is related of Eleonora Duse that in a divorce play, while in a soliloquy following a wrangle with the husband, she kept playing with her wedding-ring, taking it off, replacing it, and finally taking it off again; she is now ready for the seducer. The action illustrates the profundity of the great actress' character studies.

Maeder¹ tells the following story of a Zurich colleague who had a free day and was hesitating between making an agreeable holiday of it and paying a distasteful duty call on some people in Lucerne. He ultimately decided on the latter, and dolefully set out. Half way to Lucerne he had to change trains; he did this mechanically, and settled down in the other train to continue his reading of the morning papers. When the ticket collector came round he discovered that he had taken a train back to Zurich. His wish to spend the day there and not in Lucerne had proved too strong for his good intentions.

In most of the examples previously mentioned in this paper, and of those encountered in real life, it is possible to discover a motive for the given occurrence that logically accounts for this, but which does not lie particularly deep in the person's mind. In other words, it is, in Freud's language, fore-conscious,² and the subject has no particular difficulty in recognizing it as an integral part of his personality. The problem, however, is far from exhausted at this point. It is next necessary to discover the origin of the motive or tendency in question, or to explain why it needs to be expressed at all. In this investigation one reaches the realm of the unconscious proper, and here it often turns out that the error which is being analyzed has a deeper meaning, that it symbolizes more than the fore-conscious motive, and expresses tendencies of much greater personal significance; this may be the case, however trivial the error in itself. In some of the preceding examples the fore-conscious motive disclosed appears trite, and it seems unlikely that such a trifling matter should need a complicated psychological mechanism to manifest itself. In the cases of this kind that I have had the opportunity of submitting to a detailed psycho-analysis, I have found that the unconscious associations often shed an unexpectedly

¹Maeder: *Nouvelles contributions à la psychopathologie de la vie quotidienne*. Arch. de Psychol., 1908. VII, p. 296.

²For the explanation of this and allied terms see Psychol. Bull., April, 1910. p. 111.

instructive light on the full meaning of the occurrence. Unfortunately, however, the motives thus reached are usually of so intimate a nature that discretion forbids the publishing of them.

In still other cases no fore-conscious motive can be discerned, and the error appears to be quite meaningless until the truly unconscious sources are reached. In the following example¹ the fore-conscious motive was not discovered until the resistance to the unconscious sources of it were broken down. It is further peculiarly instructive in illustrating what important and fundamental traits of character may be revealed by the analysis of an absolutely trivial occurrence.

A doctor on re-arranging his furniture in a new house came across an old-fashioned, straight, wooden stethoscope, and after pausing to decide where he should put it, was impelled to place it on the side of his writing-desk in such a position that it stood exactly between his chair and the one reserved for his patients. The act in itself was certainly odd, for in the first place the straight stethoscope served no purpose, as he invariably used a binaural one; and in the second place all his medical apparatus and instruments were kept put away in drawers, with the sole exception of this one. However, he gave no thought at all to the matter until one day it was brought to his notice by a patient, who had never seen a wooden stethoscope, asking him what it was. On being told, she asked why he kept it just there; he answered in an off-hand way that that place was as good as any other. This started him thinking, however, and he wondered whether there had been any unconscious motive in his action. Being interested in the psycho-analytic method he asked me to investigate the matter.

The first memory that occurred to him was the fact that when a medical student he had been struck by the habit his hospital interne had of always carrying in his hand a wooden stethoscope on his ward visits, although he never used it. He greatly admired this interne, and was much attached to him. Later on, when he himself became an interne, he contracted the same habit, and would feel very uncomfortable if by mistake he left his room without having the instrument to swing in his hand. The aimlessness of the habit was shown not only by the fact that the only stethoscope he ever used was a binaural one, which he carried in his pocket, but also in that it was continued when he was a surgical interne and never needed any stethoscope at all.

¹In the *Zentralb. f. Psychoanalyse*, Jahrg. 1, S. 96, I have published a fuller account of this example.

From this it was evident that the idea of the instrument in question had in some way or other become invested with a greater psychical significance than normally belongs to it, in other words, that to the subject it stood for more than it does with other people. The idea must have got unconsciously associated with some other one, which it symbolized, and from which it derived its additional fullness of meaning. I will forestall the rest of the analysis by saying what this secondary idea was, namely a phallic one; the way in which this curious association had been formed will presently be related. The discomfort he experienced in hospital on missing the instrument, and the relief and reassurance the presence of it gave him, was related to what is known as a "castration-complex;" namely, a childhood fear, often continued in a disguised form into adult life, lest a private part of his body should be taken away from him, just as playthings so often were; the fear was due to paternal threats that it would be cut off if he were not a good boy, particularly in a certain direction. This is a very common complex, and accounts for a great deal of general nervousness, and lack of confidence, in later years.

Then came a number of childhood memories relating to his family doctor. He had been strongly attached to this doctor as a child, and during the analysis long buried memories were recovered of a double phantasy¹ he had in his fourth year concerning the birth of a younger sister, namely that she was the child (1) of himself and his mother, the father being relegated to the background, and (2) of the doctor and himself; in this he thus played both a masculine and feminine part. At the time, when his curiosity was being aroused by the event, he could not help noticing the prominent share taken by the doctor in the proceedings, and the subordinate position occupied by the father; the significance of this for later life will presently be pointed out.

The stethoscope association was formed through many connections. In the first place, the physical appearance of the instrument, a straight, rigid, hollow tube, having a small bulbous summit at one extremity, and a broad base at the other, and the fact of its being the essential part of the medical paraphernalia, the instrument with which the doctor performed his magical and interesting feats, were matters that attracted his boyish attention. He had had his chest repeatedly examined by the doctor at the age of six, and distinctly recollected the voluptuous sensation of feeling the latter's head near him pressing the wooden stethoscope into his chest, and of the

¹Psycho-analytic research, with the penetration of infantile amnesia, has shown that this apparent precocity is a less abnormal occurrence than was previously supposed.

rhythmic to-and-fro respiratory movement. He had been struck by the doctor's habit of carrying his stethoscope inside his hat; he found it interesting that the doctor should carry his chief instrument concealed about his person, always handy when he went to see patients, and that he only had to take off his hat (*i. e.* a part of his clothing) and "pull it out." At the age of eight he was impressed by being told by an older boy that it was the doctor's custom to get into bed with his women patients. It is certain that the doctor, who was young and handsome, was extremely popular among the women of the neighborhood, including the subject's own mother. The doctor and his "instrument" were therefore the objects of great interest throughout his boyhood.

It is probable that, as in many other cases, unconscious identification with the family doctor had been a main motive in determining the subject's choice of profession. It was here doubly conditioned, (1) by the superiority of the doctor on certain interesting occasions to the father, of whom the subject was very jealous, and (2) by the doctor's knowledge of forbidden topics¹ and his opportunities for illicit indulgence. The subject admitted that he had on several occasions experienced erotic temptations in regard to his women patients; he had twice fallen in love with one, and finally had married one.

The next memory was of a dream, which I have published elsewhere,² plainly of a homosexual-masochistic nature; in it a man, who proved to be a replacement-figure of the family doctor, attacked the subject with a "sword." The idea of a sword, as is so frequently the case in dreams, represented the same idea that was mentioned above to be associated with that of a wooden stethoscope. The thought of a sword reminded the subject of the passage in the Nibelung Saga where Sigurd sleeps with his naked sword (Gram) between him and Brunhilda, an incident that had always greatly struck his imagination.

The meaning of the symptomatic act now at last became clear. The subject had placed his wooden stethoscope between him and his patients, just as Sigurd had placed his sword (an equivalent symbol) between him and the maiden he was not to touch. The act was a compromise-formation; it served both to gratify in his imagination the repressed wish to enter into nearer relations with an attractive patient (interposition of phallus), and at the same time to remind him that this wish was not to become a reality (interposition of sword). It was, so to speak, a charm against yielding to temptation.

¹The term "medical questions" is a common periphrasis for "sexual questions."

²*Amer. Journal of Psychol.*, April, 1910, p. 301.

X. GENERAL OBSERVATIONS

(1) *Warrant for Interpretations*

The first criticism of the theses here maintained that naturally presents itself is the question as to the reliability of the individual interpretations. It is not likely that any one will reject them all as improbable, but, particularly with the more complex analyses, doubt must arise concerning the trustworthiness of the results. This is especially so in regard to the personal, subjective factor in the interpretations, although as a matter of fact the very constancy of the way in which similar conclusions are reached by different observers indicates that this factor is less potent than might be imagined. Experience shows that, when attention is carefully directed to the objective aspects of the analysis, the importance of the personal factor, which from the unavoidable nature of the circumstances can never be entirely eliminated, can be reduced to a degree where it is practically negligible. In most scientific work the personal factor has to be reckoned with, but appreciation of the way in which it acts, especially when this is based on psychological knowledge, as a rule enables it to be excluded to such an extent as not to interfere with conclusions being formulated that are valid enough to stand the objective test of verifiability. It is contended that this statement applies unrestrictedly to psycho-analytic interpretations. It is, of course, to be conceded that the probable accuracy of these interpretations varies considerably in different instances, as conclusions do elsewhere in science. Thus, in a chemical analysis, the conclusion as to whether a given substance is present or not varies in probability according to the quality and amount of evidence obtainable; in some cases the confirmatory tests are so unequivocal that the final decision is a practically certain one, in others it is very probable, in still others it is only a plausible possibility, and so on.

The view that the psycho-analytic interpretations of the class of occurrences under discussion are reliable is based on, among others, the following considerations:

(a) The psychological correctness of the principles of the free association method. This is too complex a matter to be gone into here, and I will only refer the reader to Jung's well-known works¹ on the subject.

(b) The constancy of the findings by different observers, and the harmony of the conclusions with those reached in the study of other fields, *e. g.*, dreams, psycho-neuroses, mythology, etc. It is extremely unlikely that this is due to coincidence,

¹Jung: Diagnostische Assoziationsstudien. Bd. I, 1906, Bd. II, 1910.

and still more so that it is due to identical prejudices on the part of the different workers, for in the first place this would be postulating a very remarkable uniformity in their individual mental constellations, and in the second place psycho-analytic research brings with it an eradication of personal prejudice, and an appreciation of personal complexes, that is rarely attained elsewhere in the same degree.

(c) The increased intelligibility of the processes in question. An occurrence that previously was obscure and meaningless now becomes throughout comprehensible, and an integral part of the rest of the person's mental operations. It is seen to be merely an irregular manifestation of a logical tendency that is an essential constituent of the personality, the unusual features having certain definite reasons for their occurrence. Moreover, the discovery of the underlying motive, and its connection with the manifestation being analyzed, is a matter that commonly lends itself to external verification. When, in an analysis, one traces a given error in mental functioning, such as a *lapsus linguæ*, to a thought that the person was desirous of keeping back, it is usually easy to confirm the truth of the conclusion. Very significant in this connection is the unmistakable evidence of the resulting affect in the person, which accurately corresponds with that characteristic of the revealed mental process. Often this is so pronounced that it is quite impossible to doubt the truth of the interpretation made; this especially is a matter where personal experience is more convincing than any possible amount of discussion.

(d) The fact that in many fields the principles in question are generally recognized to be valid. Freud's study is only a detailed working-out of laws that were already known to hold true over a limited area. When a man is hurt at finding his name unfortunately forgotten, or at unexpectedly being passed by unrecognized in the street; when a lady is offended by some one who professes regard for her forgetting to carry out her behests or to keep a rendezvous, they are displaying an affect that accords perfectly with the inferences of the psycho-analyst, and with no others. In this correct intuition of mankind lies already the essential nucleus of the conclusions maintained by Freud.

Indeed it is quite impossible to go through life without constantly making interpretations of just this kind, though usually they are simpler and more evident than those needing a special psycho-analysis. Observation of a very few jokes is sufficient to illustrate this, and we "read between the lines" of the people we have to do with, doubting the scientific justification of our right to do so as little as we do in the interpreta-

tions of jokes.¹ This holds in the most manifold fields of mental activity. Three examples may be quoted, of a kind that could be multiplied indefinitely. With Mr. E. R. Bennett's play "The Servant in the House," no one can witness it intelligently and doubt that the Hindoo servant, who is the principal character, is a presentation of Jesus Christ, or that his name "Manson" is a disguised form of the title "Son of Man." Yet we should find it difficult to "prove" this to a carping critic who is bent on avoiding the obvious inference, and still more to "prove" our assumption that the disguise was the product of definite motives in the author's mind. In Mr. Bernard Shaw's play "Press Cuttings" one of the characters, the Prime Minister of England, is called "Balsquith." When one infers that he compounded the word from the names of two Prime Ministers, Balfour and Asquith, the critic may accuse us of reading into Mr. Shaw's mind views of our own that never existed there.² In Shelley's "Œdipus Tyrannus" what right have we to assume that in his ridicule of the Ionian Minotaur³ the author was satirizing the Englishman of his time? Our answer in all these cases is the same, namely, that we feel justified in making the inferences in question because they make something intelligible that otherwise would have no meaning. This answer is perfectly correct, for in the last analysis the justification of every scientific generalization is that it enables us to comprehend something that is otherwise obscure, namely, the relations between apparently dissimilar phenomena.

To this it may be said that in such cases as those just mentioned a logical meaning is given to something that from previous experience we have every reason to expect has one, but that the point in dispute about the "psychopathological" occurrences of everyday life is whether they have such a meaning or not. Here *a priori* argument can take us no farther, and the question can only be referred for solution to actual investigation, a matter usually considered unnecessary, on the pure assumption that the occurrences have no logical meaning. Freud's scepticism made him challenge the necessity of this assumption, and prefer to leave the question open

¹In "Der Witz und seine Beziehung zum Unbewussten" Freud has made a detailed study of this subject. As with the occurrences studied in the present paper, he has shown that the insight consciously obtained is often only a partial one, and that the true significance is often related to unconscious sources.

²The royal censor refused to let the play be acted until the name was replaced by one less open to this personal interpretation, namely, Johnson; the name of the commander in chief, Mitchener (from Milner and Kitchener), had to be altered to Bones.

³=John Bull.

until it was investigated. On doing so he found as a matter of experience two things, namely, that the realm of psychical determinism is more extensive than is generally supposed, and that awareness of a motive at a given moment is not a necessary accompaniment of the external manifestation of this.

Freud further came to the conclusion that there was a definite cause for the popular belief that so many blunders in our mental functioning are meaningless. He holds that this belief is due to the same cause as the blunders themselves, namely, to repression. Various repressed thoughts are in every one of us constantly coming to expression in the shape of "meaningless" blunders, the significance of which necessarily escapes us. Being thus accustomed to the occurrence of such matters in ourselves we naturally attach no significance to them in others; we "explain" these as we do our own, or accept the "explanations" proffered just as we expect others to accept the "explanations" of our own blunders.

As to these explanations little more need be added. Where the factors they have recourse to are operative at all, they act only as predisposing conditions, not as the true cause. Freud¹ gives the following apposite illustration of the actual state of affairs. "Suppose I have been so incautious as to go for a stroll in a lonely part of the town, where I am attacked and robbed of my watch and money. At the next police station I give information with the words: I have been in this and that street, where *loneliness* and *darkness* stole my watch and money. Although in these words I should have said nothing that was not correct, still from the wording of my information I run the danger of being thought not quite right in the head. The state of affairs can correctly be described only thus, that *favoured* by the loneliness of the spot, and *unrecognizable* through the *protection* of the darkness, a thief has robbed me of my valuables. Now, the state of affairs in the forgetting of a name need not be otherwise; favored by fatigue, circulatory disturbances and poisoning, some unknown psychical agent robs me of the proper names that belong to my memory, the same agent that on other occasions can bring about the same failure of memory, during perfect health and capacity." Similarly such a mistake as a slip of the tongue is often attributed by psychologists (*e. g.* Wundt) to a momentary inattentiveness. It is certainly a question of conscious attention, but Freud² has pointed out that the defect is more accurately described as a disturbance of attention than as a diminution, the true cause being the disturbing influence of a

¹Freud: Zur Psychopathologie. S. 22.

²Freud: *Op. cit.* S. 68.

second train of thought. The same remarks apply to all the other explanations urged. Several examples were given above in which names and other words differing by only one letter were confounded or interchanged, and evidence was brought forward to show that this external association was merely a predisposing circumstance, and not the actual cause of the mistake. Many such circumstances favor the occurrence of a blunder, that is, they permit a repressed thought to slip partly through. Alcoholic intoxication is notoriously one. Emotional excitement is another. Many blunders, forgettings, and other oversights, are attributed to the confusion of hurry. Thus, for instance, I have noticed that the using of the wrong key, in the examples quoted above, most often occurred when I was in a great hurry (the same was true of the not recognizing the assistant superintendent in his office), but if haste were the true cause it would be curious that it should bring about a blunder of a kind that defeats its own object; strictly speaking it is the emotional confusion or excitement engendered by hurry that permits a second repressed impulse to manifest itself in what externally appears as a blunder.

As has been remarked above, there are certain occasions in everyday life when the normal person divines the motivation of unintentional errors, though these are rare in comparison with the occasions on which it escapes him. Freud¹ has pointed out that there are two other groups of processes in which an *unconscious*, and therefore distorted, knowledge of this motivation is manifested, namely in paranoia and in superstitions. In both these the subject reads a meaning into external happenings that have no such psychical meaning, and, in a very interesting discussion of the subject, Freud produces reasons to believe that this erroneous functioning is due to a projection to the outside of motives that exist in the subject's mind and are full of meaning there, but which he does not directly perceive.

A little may be said on a feature of some of the analyses quoted that may strike the reader as odd, namely, the remarkable play on words that is so often found. Whoever is surprised at this needs to be reminded of the almost boundless extent to which the same feature occurs in other fields of mental activity, in wit, dreams, insanity, and so on. Even in the serious affairs of everyday life it is far from unusual. Thus, to cite a few business announcements, we see the National Drug Company using as its trade motto "Nadru," the National Liquorice Company (N. L. Co.) that of "Enelco," we find the Levy Jewellery Company reversing its first name into the

¹Freud: *Op. cit.* S. 131 *et seq.*

more pretentious one of Yvel, and advertisements of "Uneeda" biscuits and "Phiteezi" boots are familiar to every one. This tendency to play on words, and to produce a more useful or pleasing result (mirror-writing, ciphers, and rhyming slang¹ also belong here), is evidently dictated by the same *Unlust* motives—to avoid banal or otherwise unattractive words—that so much stress has been laid on above. It is one that has far-reaching roots in early childhood life. In fore-conscious and unconscious mental activities this play on words—clang associations—is much more extensive than in consciousness, and serves for the transference of a given affect from one mode of expression to a more suitable and convenient one.

(2) *Bearing on psycho-analytic method of treatment*

Three brief remarks may be made on this matter. In the first place, investigation of the errors and slips of everyday life is perhaps the best mode of approach to the study of psycho-analysis, and affords a convenient preliminary to the more difficult, and more important, subject of dreams. The greatest value is to be attached to self-analysis, a fact to which attention cannot too often be called. In the second place, analysis of the occurrences in question is of great service in the treatment of neurotic patients. Their behavior in this respect needs to be closely observed, and frequently a quite trivial occurrence will, when investigated, provide clues to the elucidation of the main problems. Thirdly, consideration of the mechanism of these erroneous functionings makes it easy to understand the way in which psycho-analysis brings about its therapeutic effects. Both the errors and neurotic symptoms are the manifestations of dissociated conative trends which are weaker than the rest of the personality opposed to them, are consequently repressed, and can come to expression only in indirect ways and only under certain circumstances. An essential condition for this is non-awareness of the process. Psycho-analysis by directing the dissociated trend into consciousness abolishes this condition, and therefore brings the trend under the control of the conscious inhibiting forces. Conscious control is substituted for automatic expression, the significance of which was not realized. These considerations may be illustrated by the tritest of the examples given above, namely, my opening of a fresh

¹The following are instances from the Cockney type of this. "Aristotle" =bottle. "Cain and Abel" =table. "Harry Nichols" =pickles. Mediate forms are: "Christmas" (card) =guard: "Bull" (and cow) =row: "Malcolm" (Scott) =hot; "Stockton" (on-Tees) =cheese: "Rosie" (Loader) =soda, and so on.

tobacco tin although I wished first to finish the old one. Here it is quite obvious that the rule just stated holds, that an essential condition of the erroneous functioning is non-awareness of the significance of the process; I knew that I was reaching for tobacco, but did n't notice which tin it was. The moment I realized the situation I of course checked the error, and controlled the wish that was taking advantage of my absent-mindedness to come to expression. On a larger scale the same is true of neurotic symptoms; realization of their significance checks the morbid expression of the underlying impulse. *The cardinal proposition is that consciousness of an aberrant impulse means increased control of it.*

(3) *Relation to health and disease*

This matter should be fairly evident from the preceding considerations, so that the two corollaries that follow in this respect need only to be stated. The first is that from a psychological point of view perfect mental normality does not exist. In other words, every one shows numerous defects in mental functioning that are manifestations of dissociated, repressed, psychical material, and which are brought about by the same psychological mechanisms as those operative in the case of the psycho-neuroses. A further matter not brought out in the preceding study is that this material is ultimately of the same nature as that from which neuroses are produced. The second corollary is that the border-line between mental health and disease is much less sharp even than is generally supposed. The distinction between the two is really a social one, rather than a psychopathological one, just as the distinction between sanity and insanity is primarily a legal one. When the erroneous mental functioning happens to carry with it a social incapacity or disability the condition is called a neurosis, and when it does not it is called absent-mindedness, eccentricity, personal mannerism, and so on. Further reflections on the significance of these conclusions will here be omitted, as they are not relevant to the main purpose of the paper.

(4) *Determinism and Free Will*¹

One of the psychological arguments against the belief in a complete mental determinism is the intense feeling of conviction that we have a perfectly free choice in the performance of many acts. This feeling of conviction must be justified by something, but at the same time it is entirely compatible with a complete determinism. It is curious that it is not

¹This section is largely paraphrased from Freud, *Op. cit.* S. 130.

often prominent with important and weighty decisions; on these occasions one has much more the feeling of being irresistibly impelled in a given direction (compare Luther's "Hier stehe ich, ich kann nicht anders"). On the contrary it is with trivial and indifferent resolutions that one is sure that one could just as well have acted otherwise, that one has acted from non-motived free will. From the psycho-analytic point of view the right of this feeling of conviction is not contested. It only means that the person is not aware of any conscious motive. When, however, conscious motivation is distinguished from unconscious motivation, this feeling of conviction teaches us that the former does not extend over all our motor resolutions. What is left free from the one side receives its motive from the other, from the unconscious, and so the psychological determinism is flawlessly carried through. A knowledge of unconscious motivation is indispensable even for philosophical discussion of determinism.

(5) *Social Significance*

It would be interesting to speculate as to the result of a general knowledge of the unconscious motives that underlie the failures of mental functioning in everyday life, but it is perhaps more profitable to review some of the present results of ignorance of them.

One of these is that both intellectual and moral dishonesty is facilitated to an extraordinary extent. There is no doubt that dishonesty of which the subject is not conscious is much commoner than deliberate dishonesty, a fact of considerable importance in, for instance, juristic matters. The hysteric who cannot move her leg because unconsciously she wishes it to be paralyzed, the tourist who overlooks a prohibiting notice because he finds such things annoying, and the impecunious man who forgets to pay a bill because he does n't want to, are all instances of this. At the same time the line between these two types of dishonesty is nowhere a sharp one, and in many cases one can only conclude that the subject could with a very little effort recognize the suppressed motive, which is more than half conscious. In psycho-analytic treatment this is constantly to be observed; the following slight example of it may be quoted. A young woman told me of a certain experience she had had in her childhood in company with a boy. I had every reason to believe that this was far from being an isolated one, and asked her whether it had occurred with any one else. She said, "Not that I can remember." Noticing the wording of her answer and a certain expression in her face, I asked, "What about the times that you can't remember?"

She exclaimed "Oh, shucks," and in such a disconcerted tone that I was sure my surmise had been well-founded. She then made the remark, "Well, I really had forgotten the other times till this minute," the truth of which was probably only partial. The incident made me think of Nietzsche's epigram: "One may indeed lie with the mouth, but with the accompanying grimace one nevertheless tells the truth." Half-amnesias of this kind are extremely common in daily life.

In spite of the constant endeavor to keep back disagreeable or unacceptable thoughts, these very thoughts betray themselves in blunders of the type under discussion. By the world this self-betrayal is passed by unnoticed, but it does not escape any one who has made a study of unconscious functioning. Freud¹ in no way exaggerates when he says: "He who has eyes to see, and ears to hear, becomes convinced that mortals can hide no secret. Whoever is silent with the lips, tattles with the finger-tips; betrayal oozes out of every pore." Moreover, even with a direct lie, careful observation of the undue emphasis here and the distortion there will usually disclose what the person is trying to conceal, for the lie is a creation of the same mind that at the moment is cognizant of the truth. It is very rare, especially on emotional occasions, for self-control to be so complete as to inhibit all unconscious manifestations, which to an attentive observer will indicate the truth. Strictly speaking, one cannot lie to another, only to oneself, and skilled introspection makes even this increasingly difficult.

An important consequence of this is that every one is apt to know more about the inner motives of those near to him than they themselves know, inasmuch as every one is continually performing at all events some simple kind of psychological analysis on those around him. This is a fertile source of misunderstandings and friction,² especially in family and married life where contact is much nearer. One person intuitively recognizes an intention or tendency in the other that the latter refuses to admit even to himself. When the unavoidable inferences are presented to him, he is indignant, rebuts them as being groundless, and complains that he is misunderstood. Strictly speaking, such misunderstanding is really a too fine understanding. The more nervous two people are, the more often do they give rise to schisms, the reasons for which are as categorically denied by the one as they are obvious to the other. This is the punishment for the inner improbity, that, under the pretext of forgetting, absent-mindedness, and so on,

¹Freud: *Sammlung kleiner Schriften*. Zweite Folge. S. 69.

²Freud. *Zur Psychopathologie*, S. 114.

people allow tendencies to come to expression which they would do better to admit to themselves and others, unless they can control them.

Most important, however, is the extension of these principles to the sphere of human judgment, for it is probable that repressed complexes play as prominent a part in distortion here as they do in the minor errors of memory mentioned above. On a large scale this is shown in two ways, in the minimum of evidence often necessary to secure the acceptance of an idea that is in harmony with existing mental constellations, or to reject one that is incompatible with these. In both cases it is often affective influences rather than intellectual operations that decide the question. The same evidence is construed quite differently when viewed in the light of one affective constellation from the way it is when viewed in the light of another. Further, when the general attitude towards a question changes in the course of time, this is often due at least as much to modification of the prevailing affective influences as to the accumulation of external evidence; for instance, the average man of to-day does not hesitate to reject the same evidence of witchcraft that was so convincing to the man of three centuries ago, though he usually knows no more about the true explanation of it than the latter did.

Ignorance of the importance of affective factors in this respect, combined with the ineradicable popular belief in the rationality of the individual mind, has the interesting result that strong differences of opinion are attributed by each side to a defect in reasoning capacity on the part of the other. In an exposition of this matter Trotter¹ writes: "The religious man accuses the atheist of being shallow and irrational, and is met with a similar reply; to the Conservative, the amazing thing about the Liberal is his incapacity to see reason and accept the only possible solution of public problems. Examination reveals the fact that the differences are not due to the commission of the mere mechanical fallacies of logic, since these are easily avoided, even by the politician, and since there is no reason to suppose that one party in such controversies is less logical than the other. The difference is due rather to the fundamental assumptions of the antagonists being hostile, and these assumptions are derived from herd suggestion."

There is a certain amount of truth in this imputation of stupidity to the person on the opposite side, for in his blind refusal to appreciate or even to perceive the evidence adduced by his opponent he may give an unavoidable appearance of

¹Wilfred Trotter: *Herd Instinct and its Bearing on the Psychology of Civilised Man*. *Sociological Review*, July, 1908. (P. 19 of reprint.)

marked stupidity. A further reason for this is that some one under the sway of strongly affective influences thinks not only that any one differing from him must be deficient in reasoning power, but also that the views of the latter are themselves stupid. In attempting to controvert these, therefore, he unconsciously distorts them until they really are foolish, and he then finds it easy to demolish them. Any man of the period who read only the account of Darwin's views that was promulgated by his theological and scientific opponents must have wondered why it was worth while to attack such obvious nonsense, while our wonder, on the other hand, is that reputable and otherwise intelligent men could have managed so to pervert and misunderstand statements that to us are lucidity itself. Similarly at the present time if some of the remarkable accounts of Freud's views that are given by his opponents represented anything like what he really holds, the fact would need much explanation that so many scientific men can accept them and yet remain sane.

Yet this astonishing stupidity in apprehending the arguments of opponents, and in defending preconceived views, is only apparent. The men who so grossly misinterpreted Darwin were often men of the highest intellectual power, and the same is true of many of Freud's opponents; similarly no one can read closely the *Malleus Maleficarum* without admiration for the amazing intellectual ingenuity with which the most fantastic propositions are there defended. The process is one that psychiatrists call "emotional stupidity," a symptom seen in patients who have no real defect of reasoning power, but who through various affective influences are in a condition that at first sight gives rise to a strong suspicion of some organic defect of the brain.

On observing the general attitude towards people whose "emotional stupidity" has in the course of time become apparent, two things are noticeable. In the first place, as was remarked above, the fault is attributed much more to intellectual inferiority than to the more important affective causes. Hence the present day supercilious pity for the scholastics of the "dark ages," an attitude considerably modified by an objective comparison of the reasoning powers characteristic of the two civilizations. In the second place, far greater leniency is shown towards a stupidity that expressed itself in the form of blind adherence to accepted errors, than that which expressed itself in the form of blind rejection of a novel truth; in other words, credulousness is always more harshly judged than incredulousness, though they are both merely different aspects of the same fundamental failing,

namely, lack of true scepticism. Yet the one is hardly more characteristic of human weakness than the other—as Nietzsche put it: “Mankind has a bad ear for new music”—and it would be hard to convince a student of human progress that the first manifestation has a greater retarding influence on this than the second. In any case these considerations go to show the fallacy of the popular belief that the will is the servant of reason, the truth being that reason has always been, and probably always must be, only the handmaid of the will.

XI. SUMMARY

Only a small part of the subject matter dealt with by Freud has been covered in the present paper. Those interested are referred to his book for richer and more numerous examples, and for the lucid and penetrating discussion there given of the theoretical aspects of the subject. It is perhaps desirable, however, to summarize here the main conclusions on the topics discussed above.

The occurrences that form the subject-matter of this study, the general characteristics of which were defined in the introductory section,¹ may be divided into motor and sensory.² The defects of the former class that enter into consideration are two, (1) the erroneous carrying out of an intended purpose (slips of the tongue and pen, erroneously carried out actions), and (2) the carrying out of an unintended purpose (symptomatic acts). The defects of the latter class are also two, (1) simple failure of perception (forgetting, not seeing), and (2) erroneous perception (false recollection, false visual perception). In each class the distinction between the two kinds of defects is not sharp; thus in the latter one, for instance, a failure to remember is always accompanied by an over-prominent remembrance of some associated memory, a false recollection. Further, the distinction between the two classes themselves is not a sharp one, both motor and sensory processes playing a part in many instances; thus in the mislaying of objects, the object is first misplaced, and then the memory of the act is forgotten.

Common to all forms is the fact that the subject, and most observers, either give an obviously inadequate explanation of the particular occurrence, such as that it was due to “inattention,” “absent-mindedness,” “chance,” and so on, or

¹In Germany the erroneousness of the process is conveniently indicated by the preface “ver”; thus, verdrucken, vergessen, vergeifen, verlegen, verlesen, verschreiben, versehen, versprechen, etc.

²This term is here used in its neuro-biological sense, and hence includes perceptive and apperceptive processes.

frankly maintain that it has no explanation at all. On the contrary psycho-analysis shows that there is not only a definite psychical cause for the occurrence, but that this has always a logical meaning, and may strictly be called a motive. This motive is some secondary tendency or train of thought, of which the subject is not aware at the time. Usually it is fore-conscious, or, in popular language, unconscious; in many cases it is unconscious in the strict sense, and is then correspondingly more difficult to reveal. In most cases there are both a fore-conscious and unconscious motive, which are associated with each other. The motive is repressed by the subject, the repression being a defence-mechanism that subserves the function of keeping from consciousness undesirable or painful thoughts. The motive may be one of two kinds: either it is a counter-impulse (*Gegenwillen*) directed immediately against the mental operation that is intended, or it is an impulse directed against some mental tendency that stands in associative connection with this operation; that is to say, the association between the two mental processes may be either intrinsic or extrinsic. As a result of the repression any direct manifestation of the tendency is inhibited, and it can come to expression only as a parasitic process engrafted on another, conscious one. The disturbance thus caused constitutes a temporary failure or error of normal mental functioning.

This error can psychologically be compared with a psychoneurotic symptom, the mechanisms by which the two are brought about are almost the same, and the psychical material that is the source of them is closely similar in the two cases. It is maintained that appreciation of the significance of these everyday errors is important for both the practice and theory of psychology; this is especially so in the contribution it furnishes to the problem of psychical determinism, and in the understanding it gives to the deeper, non-conscious motives of conduct. It further throws a valuable light on certain social problems, notably the question of mutual misunderstandings in everyday life, and on the importance of affective influences in forming decisions and judgments.

A CASE OF COLORED GUSTATION

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Cases of colored gustation have been described in less detail than the more common instances of colored audition. The relative infrequency with which colored tastes and colored odors have been reported has been explained from the fact that taste and odor are so bound up with the perception of a colored body that one's attention is attracted away from the photism even when it is attracted to the color of the object. The photisms are said to be most readily perceived in cases where an odor or a taste from an unknown source suddenly attracts the attention. In the present instance there has been no difficulty in perceiving the taste-color, because the latter is very brilliant, and it frequently persists more than ten minutes. Moreover, the taste-color is quite as intensive and quite as fully saturated as are the colors of objects; it can therefore be maintained without difficulty in the presence of the latter.

The young man (*S.*), who reports the present case, is a senior in college, has had some practice in psychological experimentation, and is an excellent observer. He has been under observation a year. So far as he can remember, he has always experienced tastes as colored. He reports that, as children, he and his sister employed color-names in describing their tastes. His taste-colors are located in the mouth; and they are intensified by closing his eyes. He recalls an illness during which his tastes were especially highly colored. In eating his meals, he ignores the induced colors; and, indeed, one color is frequently cancelled by another, during the act of eating. When the induced taste-color does not correspond with the actual color of the food, a most disagreeable experience results. For instance, brown and yellow mints are extremely distasteful, because of conflict of these colors with the green taste-color which is common to all mints. In general, pink and lavender tastes are agreeable; reds and browns are disagreeable. Blue tastes are never experienced. *S.* reports that colors suggest tactual experiences, and that tactual impressions suggest color.

In our investigation of the case, the following questions were kept in mind throughout: 1. Does *S.* possess a normal

sensitivity to taste? 2. Are the color-tones of his tastes uniformly determined by any particular factor in the gustatory complex? 3. Is the induced color sensational or imaginal? 4. Are his associations of touch and color comparable with his associations of taste and color? 5. Is there a correspondence between the feeling-tone of taste and that of its attendant color?

I. DOES OUR SUBJECT POSSESS A NORMAL GUSTATORY SENSITIVITY?

In our determination of the limens both for the presence and for the recognition of taste sensation, the method employed by Miss Thompson¹ was followed precisely, in order that our results might be comparable with hers. *S.*'s limens of presence were as follows,—all solutions being prepared with distilled water, and each limen being regarded as established only when three out of four judgments were correct: Sweet, .0005 per cent. saccharin; salt, .04 per cent. pure sodium chloride; sour, .003 per cent. sulphuric acid; bitter, .00008 per cent. sulphate of quinine. A reference to Miss Thompson's curves shows that five of her twenty-five male subjects gave a limen for sweet as low as that of the present subject; four of her subjects gave a limen for salt as low as *S.*, and two subjects a lower limen. Seven gave the same limen for sour, and four subjects gave a lower limen. Miss Thompson reports no subject whose limen for the presence of bitter was as low as that obtained for *S.*

In our investigation of the limens of recognition it was found that *S.* showed great facility in describing the tactual accompaniments of the four taste qualities. His judgments of the taste qualities were usually indirect inferences which were based upon the local tactual or color accompaniments. He described salt, sweet, sour and bitter as merely 'feels' upon the tongue. He insisted that a lump of sugar had no taste; and he remarked casually that rock-candy was salt. None the less, his gustatory sensitivity appeared normal, or supra-normal.

The limen for the recognition of sweet was .0005 per cent. saccharin,—a lower limen than Miss Thompson obtained for any of her male subjects. Discrimination both of this taste quality and of its relative intensity was due to the presence of the concomitant black, which was present even with the weakest solution which we employed. The 'feel' of sweet was usually located at the tip of the tongue.

¹H. B. Thompson: *Psychological Norms in Men and Women*. University of Chicago Contributions to Philosophy, IV. 1. 1903, 50 f.

The limen for the recognition of salt was about average,—.11 per cent. sodium chloride. Four of Miss Thompson's subjects gave the same, and ten gave a lower limen. Color rarely appeared with this solution; when it did appear it was slate or dirty white. The discrimination of salt from sweet revealed the following characteristics: It was differently localized; it failed to persist; it had no color. The judgments were given slowly, and with clear consciousness of the foundation upon which they were based.

The limen for recognition of sour was also average, or somewhat high,—.007 per cent. sulphuric acid, a limen which Miss Thompson found for twelve of her subjects, while seven of her subjects gave a lower limen. Color entered the experience, but without much uniformity,—brown, red and green being reported. The distinctive tactual component was its puckering character.

The limen for recognition of bitter was low. At the point where discrimination began,—.0003 per cent. sulphate of quinine,—a dull orange-red entered the experience, and became more pronounced as the solutions increased in intensity. The tactual component was a roughness. *S.* remarked that the solution tasted exactly like a mixture of red pepper and water. Only one of Miss Thompson's male subjects gave the same limen as *S.*, and only two gave a lower limen. It seems to be significant that the two solutions which gave the lowest limens for recognition,—the sweet and the bitter,—are just those taste qualities which are most unambiguously colored.

Individual papillæ were stimulated by means of strong solutions of the four sapid substances, and it was found that the color component did not occur with minutely circumscribed stimulation. Here as before, discrimination proved to be a product of tactual differentia, excepting in the case of bitter. The rough effect which was noticeable in the case of bitter with extensive stimulation was lacking in the papillary test. *S.* identified bitter by successively eliminating the other three taste qualities. He also reported an intermittence or 'beating' of sensation, which facilitated his identification; the identification was made very slowly but with considerable accuracy. When, on withdrawing the tongue, the solution spread over the lingual surface, the familiar orange-red appeared. In not a single instance did *S.* detect the purely bitter quality of the solution,—a two per cent. solution of hydrochlorate of quinine.

Although our tests suggest the inference that *S.*'s sense of taste may be defective, they do no more than suggest it. A crucial test would involve a separation of tactual and gustatory

qualities,—a separation which, indeed, we attempted but without success.

The most convincing evidence of a defective gustatory sensitivity is furnished by *S.*'s insistence that solutions of cayenne pepper and of quinine taste exactly alike. He was, indeed, unable to distinguish between strong solutions of red pepper and of quinine, because both produced the same 'feel' upon the tongue, and both were accompanied by the same color. After rubbing his forehead with capsicum vaseline, *S.* reported a 'smart' or 'burn,' which, although not accompanied by color, gave 'the same tactual feeling as the taste of bitter.' It would seem that, for *S.*, bitter is simply a rough, burning sensation. He does not find the taste of quinine unduly disagreeable; and he shows none of the ordinary reactions to an intensive bitter. He is, moreover, unable to understand how bitter, as he employs the term, is, in the slightest degree, characteristic of the taste of unsweetened chocolate or of ground coffee, both of which gave the color brown. All spices, on the other hand, induced red or reddish brown colors, similar to the color of pepper. His recognition of spices was slow. Cinnamon and mustard were named, but without any high degree of confidence; and ginger was stated to be either cinnamon or pepper. It is a significant fact that the taste of bitter, which furnished the best evidence of a gustatory defect, was also the taste which was most uniformly and most unambiguously accompanied by color.

As already stated, *S.* describes sugar as a 'feel.' He sugars his food in order to make the taste milder; and he is accustomed to put a pinch of salt into chocolate in order to change its 'feel'. When a drop of peppermint, or of lemon juice was added to salt, *S.* identified the salt very slowly, and even confused it with sugar.

His recognition of tastes was often retarded to a remarkable degree. In one instance, strong essence of peppermint was not identified until after three minutes. In repeated experiments with anise, which is attended by a brilliant black color, it was found that this color appeared in time to serve as a mark of identification. Sarsaparilla syrup, which likewise induced black, was also identified as anise. Listerine was at first called camphor and alum; and, after four tests in which its name was furnished to *S.*, he still failed to recognize it.

The question naturally arises as to whether *S.*'s olfactory sense is normal. A test with the olfactometer showed that it is. It is possible that the recognition of strongly odoriferous substances was, at times, retarded by the brilliant taste-colors,

which may have served to distract attention. It was found, however, that odors are not colored for *S. save* in rare instances. In the few positive cases which we discovered during the course of the investigation, a distinct taste was found to be induced by the olfactory stimulation.

2. IS THE COLOR-TONE OF TASTES UNIFORMLY DETERMINED BY ANY PARTICULAR COMPONENT IN THE GUSTATORY COMPLEX?

Here the writer recognized the possibility that odor was the factor which determined the color-tone of the taste; and this possibility was carefully tested. Solutions including syrups of orange, lemon, cherry, pineapple, the essences of wintergreen, anise, and bitter almonds together with lime-juice and alum were employed. *S.* plugged his nostrils and the central region of the tongue was painted lightly with the solution, *S.* immediately recording his experience without withdrawing his tongue. The sides and tip of the lingual surface were then painted, and records were made as before. Then a few drops of the solution were placed upon the tongue and allowed to spread, and to be swallowed. And finally the nostrils were unplugged, and a few drops of the solution were taken and immediately swallowed. The results of these experiments were clear-cut and definite. Excepting in the case of the intensely sweet solutions, color entered the complex only when the solution spread over the surface of the tongue. This color became more intensive and more persistent when, on the nostrils being unplugged, the olfactory component was added to the gustatory complex.

The author was tempted to conclude that the presence of color in the complex was largely due to the olfactory component. But this conclusion is not in accord with the fact that odorless tastes,—those from our sweet, sour and bitter solutions aroused colors; and with the additional fact that odors themselves were uncolored, save in rare instances. On the other hand, the fact that the taste-colors were frequently intensified by unplugging the nostrils, and the fact that intensive colors were more frequently present when the substances were more strongly flavored, suggest the influence of odor as an inducer of color. The presence of odor and of extensive stimulation certainly increases the vividness and the persistence of the taste-colors.

The writer is convinced that a thoroughgoing analysis would reveal the existence of a constant and uniform principle which determines the color tones of various tastes. It must be confessed, however, that certain facts still remain unaccounted for, even after extensive experimentation. A cata-

logue of the colors which are induced by various stimulations shows that it is impossible to classify them upon the basis of the olfactory component; it is possible, however, to a certain extent, to classify them upon the basis of the four taste qualities. It must be borne in mind that, for *S.*, the chief characteristic of a taste is frequently its tactual, and particularly its pungent or cooling effect. And this peculiarity must be reckoned with in any attempted classification of tastes. But so distinct are the complexes experienced that any endeavor to classify the very individual gustatory fusions under four heads must appear to be forced. With these reservations in mind, however, we shall attempt the classification.

Strong solutions of sugar and weak solutions of saccharin were found to give black, although, strangely enough, neither granulated nor lump sugar gave a colored taste. The latter, in fact, were not found to be intensively sweet, although they produced a distinct tactual sensation. Anise, cherry syrup and sarsaparilla syrup were described as sweet, and induced black,—that of anise being very brilliant. Tar-water also gave a black taste; but *S.* was uncertain whether, in this case, he actually experienced a true synæsthesia. The induced black seemed to him to be imaginal rather than sensational.

Quinine solutions, both strong and weak, gave a dull orange-red taste,—an orange-red which re-appeared in the taste of red-pepper, essence of bitter almonds, and alum. A modified red was induced by lime juice, peach flavor, pineapple syrup, and various spices. As previously mentioned, the burn or sting of the sensation was a prominent part of such tastes.

Saturated salt solution was found to give a crystal-clear experience. Moreover, it was found that a salt solution would remove mouth-colors which were already present,—a discovery which proved to be most valuable in experimentation, since the long persistence of the taste-colors made experimentation an exceedingly slow and laborious process. Listerine was the only other solution found to have this effect.

The results obtained from sour were less clear-cut than those from other taste-qualities. Green is, perhaps, the color of purely sour solutions. An occasional flash of green appeared in the test with minimal solutions. Eight per cent. tartaric acid was used as a strong solution, and this also gave, at times, a flash of green. It was noticeable that even this strong sour solution was sometimes confused with bitter. An attempt was made to provide tastes that *S.* would find very sour. This proved to be difficult. A lemon juice that the experi-

menter found excessively sour seemed to *S.* to be only moderately so. This lemon juice was found to give, however, a flash of green, a green which was more certainly induced when the lemon juice was *cooled*. A sour, lemon-pineapple sherbet was reported as very green, a green which persisted. In the laboratory tests, peppermint was the only solution that gave uniformly a persistent and vivid green. It would seem from these facts that a cooling-effect is essential to green tastes. It is, moreover, not without interest that *S.* classifies peppermint as a sour taste.

Usually, in the tests in which green appeared, it was very unstable, alternating with the color pink or red. Such alternation was observed for orange syrup, peach syrup and, above all, for wintergreen. Wintergreen, in fact, gave the most interesting results. The taste-color of wintergreen was a brilliant pink, which, however, was preceded by green or alternated with green. Usually the green persisted only a few seconds, while the pink lasted many minutes. When, however, the wintergreen solution was *cooled*, it gave a green that persisted nearly two minutes before changing to pink, while the same solution when warmed, gave a deeper pink than usual, and no green.

The alternation of green with pink raises an interesting question, as to the possibility of obtaining after-image effects from colored tastes. In support of the affirmative answer to this question, are two other casual observations. Once a black taste became white; and *S.* reported a grape-juice punch that in course of eating changed from a purple to a yellow taste. On the other hand, on this assumption, it is difficult to understand why the vivid green of peppermint, which at times persisted many minutes, should fail to give an after-image. In any case, we are left with pink as an unexplained color.

Tests of the effect of mixing the standard solutions, and the effect of successive applications of such solutions, were next planned. The mixed solutions gave the following results: Salt and sweet produced a sweet taste without color; salt and tartaric acid tasted salt and bitter and induced dark orange; salt and quinine gave a bitter taste and a faint suggestion of red; tartaric acid and sweet gave, upon one occasion, a sour-sweet taste, and a pink color; a second time, a sour taste with a flash of green; tartaric acid and quinine produced a bitter taste and a reddish color, which was less pronounced than usual; quinine and sugar solution tasted sweet, although the combination was very bitter for the experimenter, and produced a "queer hollow black" which vanished as soon as the solution was swallowed.

Application of the standard solutions in pairs was next attempted, each pair being utilized twice, and the order of application varied. The results, on the application of the second solution, were as follows: Salt-sweet, no taste, no color; sweet-salt, no taste, no color; salt-tartaric acid, biting effect of salt intensified, no color; tartaric acid-salt, salty taste, no color; salt-quinine, neutralized taste, red color as soon as salt effect wears off; quinine-salt, second taste clears up the first, then orange-red returns; tartaric acid-sweet, neutral taste, no color; sweet-tartaric acid, sweet then sour taste, green with sour taste; tartaric acid—quinine, taste not recorded, color red, a different red from that produced by pure taste of bitter; quinine-tartaric acid, taste not recorded, orange-red of bitter taste brightened by sour stimulation; quinine-sugar, taste unrecorded, orange-red unchanged; sugar-quinine, bitter taste prevailed, red darker than bitter-red.

One seems justified in concluding that salt "clears up" the colors induced by the other solutions, but that this "clearing-up" is least stable in the case of bitter. Sour is found to modify the orange-red of bitter, and to neutralize sweet, once giving pink, in this combination; bitter and sweet appear, at times, to give a color intermediate between the colors of the pure solutions.

It seemed possible that pink represented a sour-sweet taste. Lemon juice was accordingly sweetened in the hope of producing a pink taste. One attempt was successful; but a second attempt gave yellow instead. It is very difficult to make the sweet in a mixture perceptible for *S.* Peppermint essence was dropped on sugar; but the sugar was "not even tasted" and the green taste remained unchanged. If, in fact, pink be a sour-sweet taste, the color is certainly not a mixture of the colors obtained from pure sour and a saturated sugar solution.

The pink of wintergreen unites with the golden color induced by a lime wafer, to produce a rose-color, unlike the usual pink of wintergreen. If anise be taken while the wintergreen pink is still bright, there is a change to brilliant black. This black persists for a short time only; and a dirty pink results, which, in time, clears up, and gives a light pink which lasts several minutes.

The only other color that remains unaccounted for is yellow, with its variants,—tan and brown. This color was obtained from the following solutions: sweetened lemon juice, yellow (once); peppermint on salt, yellow (tried once); vanilla, tan or brown (constant); lime juice, yellow (once, alternating with red); lime candy wafer, golden (constant); lemon candy

wafer, yellow and brown (tried once); lemon essence diluted, yellowish brown mixed with green; saffras candy wafer, pink shot with yellow light (tried once); hot oil of cloves diluted, tannish brown (tried once); same solution on salt, flash of brown (tried once); chocolate and coffee, dark brown (constant); nuts of various kinds,—brown, minced English walnuts giving the lightest color. Once, while the green taste of peppermint was still vivid, wintergreen was given. This stimulation resulted in a bright and pronounced pink, which changed finally to tan.

It is perhaps unnecessary to state that all tests were tried without *S.*'s knowledge of the stimulus to be given and that his eyes were usually closed, during the test.

3. IS THE INDUCED COLOR SENSATIONAL OR IMAGINAL?

That the color element in the tastes under consideration was sensational in value seems to us to be proved by its constancy in tone for a given taste, as shown by tests at widely separated intervals; by its persistence; by its localization in the mouth; and by the fact that results were novel and unanticipated by *S.*, who was curious as to what might come, and reported results as in a sensation test. Several of the solutions were new to him, as, for instance, anise; but these new gustatory experiences yielded as constant colors as did the familiar tastes. It was noticeable that the color was usually named before a taste was recognized; in fact, *S.* frequently relies upon color as an aid in recognition. These taste-colors were not influenced by suggestion, as was shown by tests. Moreover, when colored candy wafers were used, and the eyes kept open, the color experienced was not affected by the objective color.

S. found difficulty in describing the taste-colors. For instance, he reported that the beautiful glazed black of anise, had never been experienced in any other connection. *S.* insisted that color is an integral part of the taste-fusion, and reported that wintergreen changed perceptibly in taste when it shifted from green to pink.

That the colors were not called up voluntarily is shown by the fact that, when asked to give from memory the color of a particular taste, *S.* frequently made mistakes, even in the case of solutions that gave perfectly uniform results during experimentation. Furthermore, *S.* showed very little capacity in voluntary visualization of colors, and was unable to project these colors.

S. also showed little capacity in the voluntary projection of a taste-color to an external surface. Twice, however, the surface upon which he was gazing became, to his surprise, a

brilliant pink, the color of the wintergreen which he was tasting. Both of these occurrences were spontaneous and unexpected. Attempts to throw the mouth-color upon a colored surface, in order to test the effect of such superposition, met with little success. In general, when instructed to gaze steadfastly at a colored surface while experiencing a taste-color, *S.* reported most disagreeable tension with dizziness. The mouth-color was fully as vivid as the objective color, but was differently localized.

The following were among the tests which we attempted. Strong essence of wintergreen was given, and pink color obtained in mouth; disc of spectral green placed before *S.*; no fusion. Attempt made to throw green peppermint taste-color upon rose paper; unsuccessful. *S.* unable to keep attention off mouth-color even when so instructed; rose finally fixated for 45 sec.; eyes then closed; *S.* got gray in front of eyes, and green in mouth; then a rose-red image came, and seemed to fuse with green; green returned. Attempt made to throw the brown obtained from cinnamon candy wafer upon dark blue paper; no fusion; intermittent attention; blue caused confusion and dizziness; blue did not banish brown which became darker. Orange-red pepper taste obtained and dark green paper used to stimulate eyes; attention fluctuates; no fusion. After-images were also obtained before giving a taste-stimulation and an attempt made to fuse these colors with those induced by taste, but without success. The eyes were fatigued for a certain color without any perceptible effect upon a mouth-color of the same general tone. Thus, fatiguing for black had apparently no effect upon the black taste of anise; and fatiguing for green had no effect upon the green peppermint taste. While maintaining green in the mouth, *S.* could get an after-image from spectral green without interference of colors. It has already been mentioned that peppermint green can be maintained as long as seven minutes, without failure of the color through fatigue.

4. ARE THE ASSOCIATIONS OF TOUCH AND COLOR COMPARABLE WITH THE ASSOCIATIONS OF TASTE AND COLOR?

Our experimental results lead us to answer this question in the negative. Color calls up tactual experiences much more consistently and much more frequently than touch evokes color. In an investigation of the latter situation, it was found that certain tactual experiences frequently suggested color, but that these colors were only rarely sensational in value, and were not uniform in tone. Occasionally there were instances of true synæsthesia; but there was no evidence of

a systematic case. On the other hand, colors do, apparently, call up true tactual sensations. *S.* named the "tactual feel" of every color in the Bradley chart of spectrum scales,—a test which left his hand itching and in a disagreeable condition. Nevertheless, it is very difficult to determine in how far these tactual experiences were anything more than the usual secondary accompaniment to visual perception. With his eyes closed, *S.* was frequently unable to confirm, by stroking the material, the tactual impression which he received from the visual stimulation. On the whole, however, it was evident that *S.* obtained unusually acute suggestions of tactual texture from visual texture. It is obvious that a satisfactory explanation of synæsthesia must await a more complete understanding of the secondary element in perception. In the meanwhile, border-line cases deserve more careful examination than they have received.

It is not without interest that *S.*'s preferred form of attention is auditory. He is very musical, and has an excellent command of auditory imagery. Colored audition seems to him to be forced and extraordinary. Yet, during a recent test, on the imagery aroused by poetic fragments, he has twice reported changing an auditory suggestion into a play of imaginal colors.

5. IS THERE A CORRESPONDENCE BETWEEN THE FEELING TONE OF EACH TASTE AND THE FEELING TONE OF ITS INDUCED COLOR?

It has been suggested that synæsthetic experiences involve associations through emotional similarity. The suggestion has, perhaps, been couched in too general terms to deserve detailed consideration. Our conclusion, after a careful observation of *S.* for a year, is that his experiences of taste-color are, on the whole, indifferent to him, and that there has been no æsthetic organization of tastes on a color basis, as has been suggested to be a possibility in such cases. Violets and blues, which were found by the method of paired comparison to be *S.*'s preferred colors, play no part in these experiences. The judgment 'agreeable' or 'disagreeable' is, apparently, given on the basis of the whole gustatory experience of which color constitutes an essential part. In at least one case, however, the taste of lime candy, *S.* spoke of the color, golden, as being very "pretty," while the taste was not "particularly agreeable." There was frequent disagreement between the affective tone of the color and of its tactual accompaniment. Thus, green has an agreeable "feel," but is not an agreeable color. Violet-blues are agreeable in color, but not particularly so in

"feel." Blue-greens give a "perfectly awful feeling, like running the hand over sand-paper; disagreeable to both sight and touch." The double arousal of sense-qualities, in the manner under discussion, is not without interest in the investigation of feeling-tone. It would seem to afford an especially good opportunity for the investigation of mixed feelings. Our observations on this latter point were too meagre to lead to any definite conclusion, except the unlikelihood of the synæsthetic experiences, in the present case, being explicable upon an affective basis.

In conclusion, the following facts, as deduced from the present study, are important in a theoretical consideration of synæsthesia:

1. The synæsthetic factor is sensational in value, as has been demonstrated in many other cases.

2. The color hallucination may be induced by the minimal sensory intensity of the primary component of the gustatory fusion. Other reported cases have also shown sensory defects of the primary sense-organ. Thus Pierce reports defective hearing in connection with gustatory audition. On the other hand, the literature of the subject frequently states that, in particular instances, no sensory defects were found. The reliance, in the majority of the cases reported, upon a descriptive rather than upon an analytical method may induce hesitation in accepting the evidence upon this point.

3. In the case of colored tastes or odors, color may enter the perceptual fusion from experience of the source of taste as colored. The color of the object is an important component of the usual gustatory or olfactory perception. It is easily comprehensible that the odor of violets should be blue in tone, in a given instance; and that, too, without rejecting the synæsthetic element as a sensational part of the perceptual fusion, and interpreting it, instead, as an artificial association. Reduce the intensity of a primary element in a perceptual fusion, and its place may be taken by a normally secondary factor. Thus, we can understand why for *S. red* pepper should taste dull red, and why possibly sweet tastes are black, if, as *S.* is inclined to believe, burnt sugar figured in a vivid experience of childhood. It is, however, difficult to understand how, in colored audition, tonal vision, or gustatory audition, the synæsthetic factor is involved in perceptual experience. In my opinion, we shall not understand synæsthesia until we have made a more thoroughgoing analysis of perception.

A NOTE OF THE CONSCIOUSNESS OF SELF

By E. B. TITCHENER

It happens that a number of graduates, in the department of psychology at Cornell University, have received, during the past few years, an unusually thorough training in 'systematic experimental introspection.' Many thousands of observations have been taken, under controlled conditions, of such consciousnesses as understanding, recognition, relation, expectation, belief; the classical experiments on the thought-processes have been repeated and, in some cases, varied; different kinds of imagery have been studied and their temporal courses traced. Here, then, is a group of observers who seem to be especially well qualified to report upon the nature and appearance of the self-consciousness,—about which, as it is hardly necessary to say, psychologists are very far from agreement.

The reports were obtained as answers to questions which were laid, one at a time, before the individual observers. The method is crude, and I should be the last to claim anything like finality for the results. For one thing, the reports are necessarily partial and imperfect; a complete account of the psychological self, and of the conditions of its appearance, would need to be pieced out from observations taken over an extended period of time. For another thing, the bare statement, even of a highly trained observer, that this or that mode of experience is habitual with him, or that this or the other form of experience is unknown, cannot be accepted as of equal value with the—often unconscious—self-revelation of an experimental record.¹ As regards the first point, however, I am satisfied if the reports are correct so far as they go; and, as regards the second, I rely upon the nature of the questions themselves and upon the way in which the enquiry was conducted. The questions were of a large and simple kind, and, after the first sets of answers had been received, were again laid before the observers, who were instructed to note at their leisure the facts appearing in daily life and in the course of laboratory work, and to hand in another set, of corrected answers, if they found correction to be necessary.

¹Cf. G. E. Müller, *Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes*, 1911, 143 ff.

The first question raises the point of the continuity or intermittence of the self-experience. In my own case, "the conscious self, while it can always be constructed by a voluntary effort, is of comparatively rare occurrence."¹ Wundt writes to the same effect: "Psychologically regarded, it is in normal circumstances the ordinary state of affairs that objects are given simply as objects, without there being any thought whatever of the ideating and sensing subject. . . [The expression] 'forgetfulness of self' . . . is misleading, in so far as it is prompted by the tendency to consider reference to the subject as the normal . . . state of affairs."² And Mach, in his polemic against the ego, reminds us that not only in sleep, but also "when we are absorbed in contemplation or thought, in the very happiest moments of our lives, the self may be partly or wholly lost (*fehlen kann*)."³ On the other side, we read in Calkins that "I am always, inattentively or attentively, conscious of the private, personal object, myself, whatever the other objects of my consciousness;"⁴ and James, speaking of the 'material' self, remarks that "we feel the whole cubic mass of our body all the while, it gives us an unceasing sense of personal existence."⁵

The second and third questions deal with the mode of appearance of the self in consciousness, and of the conditions under which it appears. It seems, if we consult the current works upon psychology, that there are three principal ways in which the self may become conscious. (1) There may be a certain class of mental processes which, apart from any determination of present consciousness, carries the self-meaning. For Lipps, *e. g.*, all conscious experiences fall into the one or the other of two great groups, conscious contents and self-experiences; and the self-experiences are 'feelings in the wider sense of the term.'⁶ These 'subjective' experiences always appear together with the 'objective'; "I always feel myself somehow."⁷ (2) The self-experience may proceed from a

¹Text-book of Psychology, 1910, 544 f.

²W. Wundt, *Ueber naiven und kritischen Realismus*, Phil. Stud., xii., 1896, 342 f.; *Kleine Schriften*, i., 1910, 291 f.

³E. Mach, *Beiträge zur Analyse der Empfindungen*, 1886, 18 n; *Die Analyse der Empfindungen und das Verhältniss des Physischen zum Psychischen*, 1900, 17.

⁴M. W. Calkins, *First Book in Psychology*, 1910, 4.

⁵W. James, *Principles of Psychology*, i., 1890, 333. I may here remark that the quotations made in this Note are illustrative only; I do not attempt either to furnish a complete list of authorities or adequately to characterise the positions of the authors cited.

⁶T. Lipps, *Leitfaden der Psychologie*, 1906, 3 f., 281; cf. G. Kafka, *Versuch einer kritischen Darstellung der neueren Anschauungen über das Ichproblem*, in *Arch. f. d. ges. Psych.*, xix., 1910, 116 ff.

⁷T. Lipps, *Das Selbstbewusstsein; Empfindung und Gefühl*, 1901, 13.

determination; and may then be either explicit or implicit.¹ When it is explicit, the self-meaning is carried by a characteristic group of conscious processes which is, so to say, set apart for this special office; for Wundt, *e. g.*, the self-experience consists "in essentials of a total feeling, whose predominating elements are the apperceptive feelings, and whose secondary and more variable constituents are other feelings and sensations connected with" the vital functions, the movements of the limbs, the condition of the internal organs.² Where it is implicit, we have—under the determination—a certain arrangement and temporal course of processes which, otherwise determined, would lack the special self-reference. Here we may, perhaps (for I am not sure of the instance), mention James' reduction of the spiritual, central or active self to kinæsthetic sensations in head, throat, and respiratory mechanism.³ (3) Finally, conscious selfhood may inhere in the whole of conscious experience, *e. g.* as the character of 'warmth and intimacy' which, according to James, distinguishes all of 'my' ideas from the ideas that I ascribe to any 'you'.⁴

So much may suffice by way of introduction; I turn now to the reports. The letters A, B, etc., denote the observers; their present status, as student or teacher of psychology, is indicated by s or t; and sex is shown by the letter m or f. Further reports, from present members of my graduate seminary,—students trained in introspection, but not trained so widely or for so long a period as the members of the other group,—are distinguished by the use of italicised capitals, *A, B, etc.* Corrected reports are placed within square brackets.

Question I. "I am always, inattentively or attentively, *conscious of myself*, whatever the other objects of my consciousness.' Is this statement true, as a matter of experience, (a) in everyday life, (b) in the introspective exercises of the laboratory?"

Asf. (a) No. (b) No. The statement is true as a rule at the beginning of an experiment (when I am *O*), before I have become used to the demands of *E*. It holds only occasionally after I have become practised and have forgotten that I am under *E*'s observation.

Btm. (a) No. In seeing a play, I am often another person, portrayed by the actor, and do not realise that I am a spectator until my neighbor speaks. So also when I am absorbed in a book. (b) No. I do not realise often that there is any I which perceives the stimuli. I should say that the

¹Cf. the analysis of Belief offered by T. Okabe, this *Journal*, xxi., 594.

²W. Wundt, *Grundzüge der physiologischen Psychologie*, iii., 1911, 353 ff.

³W. James, *Principles of Psychology*, i., 1890, 301; Does 'Consciousness' exist? in *Journ. Phil. Psych. Sci. Meth.*, i., 1904, 491.

⁴*Principles of Psychology*, i., 1890, 330 ff. The precise nature of the 'warmth' (cf. p. 333) does not here concern us; nor does the question of its recognition or realisation (apparently answered by James in the doctrine that the experience of 'mine' is genetically prior to the experience of 'me').

consciousness of self is no more frequent than the 'feeling of familiarity.' I take myself for granted, very much as I take familiarity for granted in 'immediate apprehension.'

(a) No. Only occasionally do I realise that there is any I which is standing over against objects or situations. I do become self-conscious, most strongly, in just those situations which seem to demand that I appear not to be self-conscious; when I know myself to be watched, when there is demanded the making or keeping of some motor adjustment (bodily movement, verbal reply). In such cases there are usually the situation (external perception of place and people and so on) and beside it the I (kinæsthetic sensations in upper chest and arms, and organic sensations) and my emotive reaction (pleasantness or unpleasantness, with abdominal organic sensations). This analysis (which I believe to be typical) is mainly from an actual experience yesterday. (b) No; even less than in everyday life. The very instruction from *E* leaves no chance for one to get self-conscious. The things to be watched are sensations and images and so on, and one watches them just exactly as one watches a thermometer rise. The more careful and strenuous the observation, the less chance does there seem to be for the realisation of anything else than the thing observed.]

Ctm. (a) Not in my case; I am only very rarely 'conscious of myself.' By 'myself' I mean not only the sum total of organic and kinæsthetic sensations representing my body and its movements, but also 'unified experience' vaguely and verbally referred to as 'my' experience. Very often my experiences, simultaneous or immediately successive, are not 'unified,' not referred to a single and identical agent, but they run side by side. (b) Not in my case, if the introspection is what I call successful, that is, if I did not catch myself introspecting. It is true that in introspective exercises I notice much more easily kinæsthetic and organic processes, also more verbal imagery, than in everyday life, because of their greater clearness and reproducibility. I become 'self-conscious,' however, only if their intensity rises above the normal degree, and that is very rarely. The matter of 'unified experience' has never come up in my introspections, as far as I can remember.

Dsf. (a) No. I am usually inattentively, and at times very attentively, conscious of myself, but there occur fairly numerous instances when I am not self-conscious at all. These periods when I am not conscious of self are of comparatively short duration (usually when I am deeply interested in a book, in listening to music, studying a picture, etc.), and often—but not always—returning self-consciousness leaves me with a feeling of surprise. (b) The mere fact of being an observer in a laboratory exercise seems to imply self-consciousness. I think that I am always somewhat conscious of myself when busied in this way. Yet even in the laboratory there are times when self-consciousness is decidedly marginal. These times are usually during the experiment itself, when perhaps attention is fixed upon some external stimulus; but the vague feeling that I am soon to answer the question 'what was *my* experience' is always faintly present. [This observer had no opportunity to correct her first answers.]

Etf. (a) I should say almost always. Occasionally I become so absorbed in a book or task or train of thought that, when interruption comes, I feel almost as I do when I have waked from sleep, and the immediate past seems almost blank, and cut off from the rest of my experience. I believe that consciousness of self is at a minimum if not wholly lacking at such times. (b) Yes, I believe so. I tend to visualise anything that I am thinking about, and so in writing introspections: the mental facts or processes that I try to describe are placed in a large dark vagueness which represents my own mind. This is situated at about the level of my head, but is much larger. Again, the effort of introspection is usually accompanied by wrinkling of forehead, drawing of eyes, and vague feeling of tension in head. These things constitute in part what I mean by conscious-

ness of self in the present case. I never forget that I am looking in, and this realisation is so strong sometimes as to amount to sensations from eyes as if turning inward. [This observer had no opportunity to correct her first answers.]

Ftm. (a) No. (b) I have never worked under the *Aufgabe* of this question; but my impression is that 'consciousness of myself' is occasional, except in the early stages of practice.

Am. (a) Yes. I am seldom *attentively* conscious of myself. There seems, however, to be always in my consciousness the obscure groundwork of pressures, strains and organic sensations which, become clearer and supplemented by visual images, make up my attentive consciousness of self. (b) Yes. The laboratory experience does not seem to differ from the everyday experience. (a) No. Observation since my first report leads me to reverse my opinion. I am now confident that I am not always conscious of myself. Self-consciousness carried kinæsthetically with possible visual images occurs comparatively seldom. Only when there is some special experience calling attention to myself, either directly as my physical self or indirectly as in a difficult or baffling action or problem (which nearly always calls up the kinæsthetic self), do I have this self-consciousness. The experience may be clear, as when accompanied by visual images of self or in the strong kinæsthetic self of an unusually difficult situation; or it may be unclear, as in vague kinæsthetic feelings of effort. Consciousness is, however, made up most of the time by mixed perceptual and ideational contents without reference to the experiencing self. (b) I have not observed any difference in the consciousness of self under laboratory conditions from that of everyday experience. Even introspection in the laboratory does not involve a constant reference to the introspecting self, although this reference is very frequently present, and often very clear.]

Bm. (a), (b) No.

Cf. (a), (b) No.

Dm. (a), (b) There are certain sensations which characterise my alimentary canal; others my arms, face, legs, etc.; varying, to be sure, with my activity, but nevertheless for any given activity possessing a fair constancy. There are thus a large number of groups of sensations which are frequently present in the various parts of my body; and when some such groups are present (which is practically always) there is consciousness of self; that is, these sensations, by virtue of their habitual attendance upon my activities, possess logical reference to self.

In addition, my ways of thinking and acting—in so far as they are conscious—bear my stamp; the feeling of them is characteristic. Hence when under an *Aufgabe* or in a particular situation I speak of them, I naturally use the term *I* think or *I* act, just as I say *I* feel, when speaking of the somatic sensations.

But when I do thus make specific use of the idea of self; when I do thus have some psychic term (*e. g.*, the kinæsthetic and verbal image *I*) which specifically designates the I-ness; then I have no longer mere sensations, images, etc., with their vague reference to self, but now an actual *perception*. This perception may have various degrees of clearness. It waxes and wanes and revives again. It comes most close to being lost altogether when I become completely engrossed—'lose myself'—in a task. Then, if only for a very brief time, I become objective. It is especially frequent in occurrence with images of future activity.

[Yes. By 'myself' I mean: This centre of material, psychical, social, etc. relations. By being conscious of myself I mean that states or processes of consciousness are present such that they carry a reference to self. Empirically one finds that the self which one means is seldom totally the same in any two cases; it is myself in this or that particular situation that is referred to. Empirically one finds also that the psychical phenomena which carry these references are varied, and seldom totally the same. One does

not find, that is, that any particular set of constant psychical factors (such as sensations of respiration) is the exclusive or predominant vehicle.

On the other hand, generally speaking, continually recurring psychical phenomena (at least, those of somatic character) do seem to me at this writing to be the vehicle of self-reference. There seems to be something of self-reference in the very feeling of moving my eyes, uttering a word, or moving or resting a limb; and this quite apart from any specially set *Aufgabe*. However, I am quite doubtful as to whether or not familiar sights, sounds and touches do *ipso facto* refer to self.

I should be inclined to distinguish between sensations of self and perceptions of self. Of the former sort were the tonic sensations in the right leg which were present five minutes ago. These bore a reference (even though vague and almost formless) to myself; they referred not at all to the red house across the valley (which I was not thinking of).

There are degrees of clearness or intensity of perceptions of self, and there are different kinds of perception, as above indicated.]

Em. (a) No. When I am alone, when I am engrossed in work, when a problem has presented itself, I am not conscious of myself at all. It is only when the environment is new and unfamiliar, or when I am in personal conversation with someone else, or when other individuals suggest the visualisation of myself among them, that I am conscious of myself. Strong kinæsthesia, and especially organic movements, emphasise self-consciousness. (b) I am strongly conscious of myself, especially at the beginning of an experiment, because I feel that the situation is new, *E* converses with me, and I visualise myself. I feel that the reports are of processes peculiarly my own. Kinæsthesia comes out strongly, and this emphasises consciousness of self.

[(a) No. My consciousness of self depends decidedly upon 'the other objects of my consciousness.' I should lay less stress on the situation (mentioned in my first answer) in which I am thrown into a new environment as the occasion of self-consciousness, since I find that even here I am very little self-conscious. As a rule, however, when kinæsthesia and particularly organic sensations are strong, or when I see or hear a reference to myself, then I am conscious of self. (b) No, not often. Here again when reference is made to *my* experiences, *my* introspections, *my* sensations, etc., I may sometimes get a *momentary* consciousness of self, mainly in visual terms (as one sitting before an experimenter). Seldom, and only in the most trying and unusual circumstances, am I continuously aware of myself for any length of time, here as elsewhere.]

Ff. (a) No. I often lose all consciousness of myself; not, however, for a long period. An uncomfortable position, some distraction of attention occurs, and I am suddenly aware of myself in all sorts of organic sensations. On some days this awareness is more apparent, more frequent than on other days. With certain persons consciousness of myself is invariably present. [Further observation shows that the frequency of self-awareness is dependent upon my physical condition.] (b) Yes. The very fact that introspections are to be given makes me aware of myself. I am then conscious of organic sensations of which, ordinarily, I am totally unaware. [The self is usually in the background; yet I am conscious of organic sensations, of changes of mental attitude, of effort, which generally persist during the entire introspective period.]

Gf. (a), (b) No.

Question II. The second question called for a description of the self-consciousness, which should be made "as definite as possible. Is the consciousness of self explicit (*e. g.*, visual image, organic sensations) or implicit (intrinsic to the nature of consciousness, inherent in the course of consciousness)?

Can you bring out the character of the self-consciousness by comparing or contrasting it with other phases of a total consciousness?"

The replies should fall into three natural groups: (1) the reports of those who answer I. (a) in the affirmative, and thus assert that they are always self-conscious; (2) the reports of those who answer I. (a) in the negative, but I. (b) in the affirmative, and thus assert that the introspective attitude always implies self-consciousness; and (3) the reports of those who answer both members of Question I. in the negative. In fact, the replies are not all as clean-cut as this grouping demands, and we must therefore be content, under the first two headings, with a classification *a potiori*.

(1) Etf. ("I should say almost always.") I have already described my self-consciousness during introspection. It is hard to describe that which is present in everyday life because, when I attempt to do so, it is this introspective self-consciousness which is present. I believe that the more natural kind is somewhat different on different occasions. It often involves organic sensations, and feelings of bodily position and of comfort and discomfort. In the presence of other people it is often connected in some way with their approval or disapproval; and almost always, whether I am alone or not, there is a strong sense of my own approval or disapproval. In other words, it is affectively toned.

Dm. (Occasionally 'loses himself.') I have virtually already answered this question. I find my self-consciousness in groups of psychic entities which habitually reoccur. When my sensations, feelings, images and activities are changed, and somewhat new ones take their place, I have less feeling of self. If they were in continual change and did not characteristically reoccur, I deduce that I should be without the self-consciousness I now have.

I have not said that the consciousness of self is a 'phase of a total consciousness'. Rather the self is a thing meant, a complex logical entity, which in the past has lived in X, is now studying psychology, etc. But that logical entity is represented in the total consciousness of almost any moment in that way which I have already mentioned, viz., the habitual attendance of certain psychic groups. Other designatory terms, so to call them, are visual images of myself in a particular situation, also auditory images of my voice and of voices speaking to me, and again various combinations of these with kinæsthetic images of activity.

(2) Dsf. (Self-consciousness sometimes decidedly marginal.) The consciousness of self is not comparable with the consciousness of external objects. It is not explicit in the sense of coming as visual imagery or organic sensations. It is rather an inherent feeling or knowledge or attitude that tells me that I am that which has images and sensations. Not a consciousness of my physical self as the object of experience, but an underlying unique knowledge of myself as the experiencing subject. I cannot seem to be able to get at it or to analyse it further in introspection. Often it is intense, but often it is merely the background of experience.

Ff. (No qualification.) Sometimes the self appears as a visual image, as if it were a thing apart and separate. The self to which I refer in my answer is, however, an intangible something, forming a sort of background, in which (as I have said) I can distinguish organic sensations.

(3) Asf. Occasionally in the form of an indefinite visual image (this is often *implied* by a vague kinæsthetic complex); sometimes I have also

vague visual images (or kinæsthetic substitutes for them) of other people. Usually *via* organic sensations, nausea, tightening of diaphragm, changes in respiratory sensations; frequently accompanied by a slight watering of the eyes (this almost invariably occurs when I am 'touched').

Btm. Self-consciousness appears usually in the form of kinæsthetic sensations from the lower trunk or from parts of the body in strained positions. Sometimes in visual images.

Ctm. My self-consciousness is usually intensely organic, a 'sinking of the stomach,' a blushing and flushing of the face, hot and uncomfortable; I am conscious of the position of my body, and especially of the movements of my limbs, through intensely unpleasant kinæsthetic and cutaneous sensations, of great variety and disconnectedness. If I am standing, the weight of the body is awkwardly shifted from one foot to the other, and one or both hands are put under the coat at the hips, thumbs pointing backwards. All these processes have a fair degree of clearness, with one or another now and then shooting to the extreme focus of attention, ousting momentarily some intellectual process which happens to be running its course in the meanwhile.

Ftm. Chiefly organic sensation. At times, a vague visual image (as if I stood before myself and saw my own face). At times, the *Bewusstseinslage* of responsibility. [I find that my self-consciousness is usually emotional.]

Am. My self-consciousness is definitely explicit. In its clearest form it consists of organic sensations (of a kind of 'nervous strain' quality) in the body, especially in the chest; and, when connected with my 'willing self' or my 'thinking self,' of deep strain sensations in the head. There are sometimes also vague strain sensations in the limbs; these are stronger when self-consciousness means my 'willing self.' Besides the clear sensations in self-consciousness, there are always poorly defined visual images, such as translucent rays being projected from the region of my chest where the organic sensations are strongest, and meaning 'I am the centre of this experience.'

The self-conscious experience seems more often to be a part of other experiences than a thing of itself. It colors the meaning of the others. In itself it resembles the experience of effort, but differs slightly in meaning and in its persistence.

Bm. It seems to me that all sorts of sensations and feelings may refer to that which experiences, to that which owns and appropriates the experience. I cannot now be more explicit. [Further observation shows me that the self-meaning is most commonly carried by organic sensations, or by visual memory-images of my body doing something; but it is also carried by other sensations and images. The verbal ideas *I* and *my* may or may not appear. The complex is affectively toned; there is a feeling of warmth or familiarity. I have noticed that the consciousness of self is clearer during inhalation. The experience with me is rare.]

Cf. Chiefly organic sensations; a visual image may be present too. The consciousness is explicit.

Em. Self-consciousness is partly explicit, manifest in a visual image of myself, organic sensations and kinæsthesia, and in part implicit, as when I recognise my introspections as material peculiarly my own, which *E* could not directly know. When I am conducting a piece of work, I am never conscious of myself as master of my hands and muscles, brain, etc. The visual image of the work as it is completed is almost continually before me as an end,—of course, with many interruptions; I mean the visual image is the majority factor. It is only when the work, problem or experiment, has been completed that I say 'I have done that.' If, however, a hitch comes (new situation), I may again be enormously aware of myself, as before the problem was begun. The consciousness of self is partly a

visual image of myself at present, *plus* a vague memory image or images (whether visual or not I do not know) of big experiences in the past.

Gf. For the most part, verbal imagery and organic sensations.

Question III. The third question, addressed only to those who had answered I. (a) in the negative, points out that this answer "implies that self-consciousness is intermittent. Under what circumstances, then, is it likely to appear?"

Asf. Whenever I know that other people are observing my physical or psychophysical self, *i. e.*, when I see their eyes fixed on me, or when I think of other people's opinion of me; when I am emotionally stirred up; in comparing my physical or mental characteristics with those of others, or with those of myself at some other time; always when something occurs which, as I say, 'touches' me, rather unexpectedly, *e. g.*, a word of commendation or reproof from another person; when another person refers to a characteristic which he designates as mine; when I am very elated, fatigued, sick; when I am wearing new or ill-fitting clothes; when sitting for a photograph. In general, when I am in an unusual situation.

Btm. Particularly when a *new* situation is to be met, when there is necessity for making or keeping a new or not entirely familiar bodily adjustment; or when the adjustment was unsuitable. Often also as a bodily reaction to a situation involving other persons.

Ctm. Most strongly immediately before appearing in public or before some personage of importance; when I hear somebody speaking about me, or read my name in print as mentioned by somebody else; when I open a telegram; in the course of talking, when a familiar word has slipped my memory, or when I get tangled up in an argument. [You have called my attention to the fact that my answers seem to make self-consciousness mainly an unpleasant experience. It is, however, true (I have verified the point by recent observations) that my states of self-consciousness are almost invariably unpleasant. There is sometimes a 'glow' of self-consciousness, which is pleasurable, after praise, recognition, etc., but this is not marked, and in any case is soon replaced by indifference or (if the self-consciousness continues) by an unpleasant, often a strongly unpleasant mood of self-criticism. I am myself a little surprised at the constancy of the unpleasantness, now that I have definitely realised it, since I am by no means of a pessimistic temperament.]

Dsf. Self-consciousness appears usually under some of the following circumstances: in cases of physical pain (organic discomfort), nervous condition, tiredness, when one has made a blunder and feels foolish or has done something one regrets, in a feeling of uselessness or inability to do what is expected of one (by self or others), in vanity or jealousy, in fear, whenever as a rule one thinks of oneself in relation to other *persons* or during purposeful introspection. There are other circumstances in which self-consciousness appears, and often it is present all the time.

Ftm. I think in the early stages of laboratory practice. Also, I suspect, in observations that involve perception of body (*e. g.*, compass points on skin) and in those that involve extreme capacity of a mental function (*e. g.*, memorial learning).

Am. [I am apt to experience consciousness of self under the following situations. (1) In many situations of *shame*. Often, though not always, when I am undressed in the presence of strangers; also when I am in an embarrassing position, especially when I have done something physically awkward or have discovered something wrong with my clothing. In the last case one of the most prominent factors in the self-consciousness is a strong tactual perception [or image?] directly under the part of my dress affected. I am also generally conscious of self when I am ashamed or belittled morally or intellectually, as when I am surprised in doing some-

thing of which I am ashamed, when I realise that I am acting hypocritically, and especially when I am violently accused, whether rightly or wrongly; also sometimes when I am badly defeated in an argument. I experience self-consciousness in this last situation chiefly as a feeling of mental isolation. I get exactly the same consciousness when I realise that I can know directly no one but myself and that I am separated from all others by mediating sensations. (2) In many situations of *elation*. I am conscious of self when I have a strong feeling of exaltation, as after an intellectual victory, after being paid a compliment, or some other success. This is a definite complex of strong organic sensations in the chest,—which does not always occur even in these situations. It used to be most definite in religious fervor. It now occurs most often in the enthusiasm of a new idea; in enthusiasm there is a set of intoxicating muscular and organic sensations, a large part of which I should interpret as consciousness of self. (3) In many situations of mental *effort*. The strains that go to make up the feeling of mental effort are in many cases identical with self-consciousness. They are most prominent in this way in the feeling of resolve or determination, especially in repeated resolve that cannot have any immediate motor result. Another striking instance is in stage-fright, where in place of the imaged speech consciousness is largely filled with strains and organic sensations meaning self.]

Bm. When thinking of nearest and dearest relations and friends; but not always. When thinking about what I ought to do in a given case; sometimes, but not always. Sometimes when praised or blamed. Especially when alone after having left home with people sorry to see me leave, etc.

Cf. It is likely to appear in one's social relations, in some emotional states and religious experiences, rather than when one is absorbed in a given task.

Em. I have already answered this question, under II.

Ff. I have already answered this question. When attention is deliberately turned upon the self, as in observations in laboratory. Under the other circumstances mentioned.

Gf. The consciousness of self appears under unusual circumstances. When I set myself the task of introspecting, or when I am conscious of being alone, or when I feel myself under strict observation: these are some of the circumstances under which self-consciousness appears.

The replies do not, by any means, stand upon the same psychological level. They show clear differences of introspective ability. They show, also, differences of attitude, of training, of point of view. They show, I am afraid, different degrees of interest in the subject; there are answers, of a partial and tentative sort, which have not been supplemented or corrected. Can any conclusions be drawn from such material?

Question I. asks whether self-consciousness is persistent or intermittent. I group the replies under the rubric of sex, and also under that of introspective experience (*m*, *f*, for the senior and *m*, *f*, for the junior group of observers); I add a *q*, in cases in which the reply was qualified.

	<i>m</i>	<i>f</i>	<i>m</i>	<i>f</i>	Total
(1) Persistent throughout the waking life		1q	1q		2
(2) Persistent during introspection		1, 1q	1q	1	4
(3) Intermittent	3	1	3	2	9

Here 11 out of 13 observers (7 men and 6 women) deny the persistence of self-consciousness throughout the waking life, and 9 out of 13 deny its persistence during introspective as well as during everyday experience. The 11 include 5 seniors and 6 juniors; the 9 include 4 seniors and 5 juniors. The 11 include 6 men and 5 women; the 9 include 6 men and 3 women.

Even the two observers who affirm the continuity of self-consciousness qualify their answers: the testimony to intermittence is therefore stronger than I have made it. And I do not hesitate to draw the conclusion that *self-consciousness is, in many cases, an intermittent and even a rare experience.* It may, of course, be maintained, in spite of what I have said above, that the method is altogether worthless; or it may be objected that the results are due to 'laboratory atmosphere.' But, at the worst, it is not likely that the text-book statements of the persistence and continuity of self-consciousness rest upon any better method; and the argument from suggestion, in a matter like this, becomes a little ridiculous. Graduate students are not simple sheep.

If this conclusion is accepted, it remains to account for the positive replies of the four observers in the first and second horizontal lines of the Table. It is possible that the four are mistaken. The two senior women of column f had no opportunity to revise their answers; and another observer, as we have seen (p. 544), changes on revision from Yes to No. Contrariwise, the *m* of these two lines does not change. It is possible, again, that the two groups of observers, the four and the nine, may have understood the question differently, and are therefore talking of different things. But it is not easy to make this possibility concrete, to use it as a ground of explanation; it is at the best a possibility, and by no means a probability; and the fact of change from Yes to No again tells against it. So I incline to the hypothesis of *individual difference*. The tendency to conscious selfhood is, I believe, one of those "tendencies which represent total directive pressures laid upon the organism, more strongly upon some individuals and more weakly upon others, but in some measure upon all; and which are realised or expressed on very various occasions, and with very varying accompaniment of consciousness" (Text-book of Psychology, 1910, 464, 544).

The persistence of self-consciousness need not—if our results are to be trusted—betray itself in the intercourse of everyday life. Four observers, two men and two women, were asked to name the man and the woman to whom they would most confidently attribute such persistence. All four mentioned the *m*, no one mentioned the *f*, of the first horizontal line of the Table. Reference to the replies of *Etf* and *Dm* will show that the self-consciousness takes on very different forms in the two cases.

I had thought that the women might prove to be more persistently self-conscious than the men. The question must, however, be left open, not only because our observers are few, but, in particular, because the women of column f had no opportunity to revise their answers.

Question II. asks for a description of the self-consciousness. Under this heading, the following general results may be noted. (1) There is no evidence of a special class of 'subjective' processes (Lipps). (2) With one possible exception, all

the reports fall under the rubric of determination. We find reference to an implicit self-consciousness in C's 'unified experience,' in D's mention of 'my ways of thinking and acting,' and in statements (*B*, *D*, for example) of the variable contents of the self-conscious experience. We find self-consciousness explicit in E's 'large dark vagueness which represents my own mind,' in A's 'translucent rays projected from the chest,'—probably, indeed, in all the cases of visual imagery, as well as in many organic complexes (*A*, *C*). For the most part, however, no hard and fast line of distinction can be drawn between the explicit and the implicit consciousnesses. (3) A possible instance of continuous and all-pervasive conscious selfhood is furnished by the observer *D*.

I do not think it wise to press the data further. I add only a rough list of the constituents of the self-consciousness, in the order of frequency of mention:

Organic complexes	12
Visual imagery	10
Affective processes	8 (implied in 4 other cases)
Kinæsthetic complexes	8 (probably, in other cases, merged in organic)
Conscious attitudes	4
Verbal-auditory images	4
Cutaneous sensations	2

The attitudes are those of responsibility (*F*), recognition of ownership of introspections (*E*), ownership of experience (*D*), and activity in background of consciousness (*F*).

Question III. asks for the circumstances under which self-consciousness is likely to appear. Here the one outstanding result is that the experience of self is preponderantly a social matter. Of the 11 observers who replied to the question, 10 (*A*, *B*, *C*, *D*, *A*, *B*, *C*, *E*, *F*, *G*) refer to some situation which involves the ascription of selfhood by others, or implies personal relations to others.

It seems, then, that the 'material self' and the 'spiritual self' are, for observers of our sort, subordinate to the 'social self'; that the realisation of the self occurs, usually, under a consciously social determination. Here of course, is nothing new. But it is reassuring, in view of the testimony to intermittence, to find that the fact appears thus plainly.

Next in order comes the unusual or novel situation (*A*, *B*, *C*, *E* [with qualification], *G*).

I conclude, therefore, that it is not permissible to define psychology as "the science of the self as conscious."¹ This definition was, in fact, rejected by one and all of our thirteen observers.² Self-consciousness appears, in many cases, as an

¹M. W. Calkins, *A First Book in Psychology*, 1910, 1.

²By twelve, for empirical reasons; by the thirteenth, for reasons that are mainly theoretical. "No. For suppose that there were periods in the consciousness of any individual which were without reference to self:

intermittent mode of conscious experience. Like other conscious attitudes, it takes shape, explicitly or implicitly, under determination. And so far as our results go, the determination is usually social in character.

then such periods of consciousness would not be subject-matter for psychology. Whether such periods exist is a matter for psychology to investigate; it may not assume their absence beforehand. Or suppose that there were phases of consciousness which in no measure had reference to self,—as indeed there seem to be: then such phases would be barred from the study of psychology."

ON MEANING AND UNDERSTANDING¹

By EDMUND JACOBSON

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§ I. INTRODUCTION.

We give, in the following pages, the results of experiments on the perception of single letters, the understanding of words, and the understanding of sentences. The experiments were performed by what is known as Binet's method, or the Würzburg method, or the method of examination: the stimulus, written or printed, was laid before the observer, who upon signal opened his eyes, fixated the paper before him, and after performance of the assigned task gave a report of his experience. The observers were Miss L. M. Day (assistant in psychology), Mr. W. S. Foster (assistant) and Dr. L. R. Geissler (instructor in psychology); all three had had thorough introspective training. In the experiments of §§ 2, 3, the writer also served as observer.

The method of examination is, without question, merely the first beginnings of an experimental method. Okabe and Clarke, in work published from this laboratory,² have proposed to supplement it by the method of confrontation. We ourselves, at one point or another in the course of the present experiments, introduced three novel features. (1) In the experiments of §§ 2 and 4 the observer was instructed to give his account of conscious events in their strict temporal order. Spontaneous reference to this order is customarily made, in most extended reports by the method of examina-

¹From the psychological laboratory of Cornell University.

²This *Journal*, xxi., Oct. 1910 and xxii., April 1911. Ogden curiously regards the addition as worthless, because alterations were not suggested (*Psych. Bulletin*, viii., 1911, 194); it seems to us that confirmation is as valuable as correction. Ogden's suggestion that the confrontation was "quite a perfunctory affair" is both gratuitous and incorrect.

tion, through such terms as *then, after that*, etc. We thought it worth while specifically to give the temporal instruction: partly in order to increase the fullness of the reports, partly in order to attain as great an accuracy in the reproduction of the experience as the circumstances of the experiments permitted. As regards the latter point, we found that considerable training was required before the observers indicated the temporal position of every reported event; and we are thus forced to the conclusion that the sequences and coincidences noted by previous writers have a certain inadequacy. As regards the former point, we found that the temporal arrangement was of material aid in the correlation of 'process' and 'meaning,'—phases of the reports which will be explained under (2) below. The actual instruction was to *give as precise and minute an account as possible of everything that occurred in consciousness, in temporal order, making liberal use of such terms as next, then, simultaneous with, and overlapping*. When the report failed to specify the temporal position of an event, a question was usually asked; but the necessity of such questions diminishes with practice. The experimenter, who took down the observer's dictation, began a new paragraph whenever *next, then, after that*, or any term definitely denoting succession was employed: so that the events of a quoted paragraph are to be considered either as simultaneous or as belonging to a single (though extended) conscious present. But it should be noted that the break of consciousness between paragraphs is relative only; neither 'process' nor 'meaning' terminates abruptly, in order directly to give place to a successor; and the observers were asked expressly to declare when an event of one paragraph lasted over into the next.

(2) The observer in these §§ 2 and 4 was also instructed to place everything, except the direct description of conscious processes, in parenthesis. Previous students of the thought-processes have distinguished between description proper (*Beschreibung*) and a mode of report that is variously named *Kundgabe* or *sprachlicher Ausdruck* or the objective reference involved in the stimulus-error. We do not here enter upon the question whether these three terms cover precisely the same material and designate precisely the same attitude on the part of the observer; nor do we now identify our own reports of 'meaning' with any one of them; it is enough for the present if intimation, linguistic expression, objective reference and report of meaning be regarded as four species of the same genus. What we desired was that attributive description of conscious processes should be marked off, by the observers themselves, from whatever else might enter into the reports;

and we accordingly required them to *put direct description of conscious processes outside of parentheses, and statements concerning meanings, objects, stimuli and physiological occurrences inside.* The procedure was justified by the results: for though failure to specify now a meaning and now a process was at first not infrequent, it grew less and less common with practice, until the twofold report became characteristic of the experiments. It is, of course, out of the question that the observer should on every occasion specify the attributes of every process and the details of every meaning: where analysis is not imperative, *e. g.*, it is sufficient to report 'perception of signal', 'sensations from eye-movement', or 'sensations from the stimulus'. But it is necessary that the observer be trained to distinguish such references to meaning or to the physiological source of his experiences or to the stimulus from description of the concomitant conscious processes; since the perception of a given object or of the same physiological occurrence or of an identical stimulus may, under different conditions, be accompanied by different conscious processes, and what the latter are often needs to be investigated. Moreover, the object of perception is not to be confused with the presented stimulus; if the experimenter desires to know what the observer is perceiving, at a particular moment, he must obtain special reports on the matter from the observer; he cannot assume that the stimulus is perceived as he himself perceives it.—By the use of parentheses we secured in any given experiment a fuller description of processes, where the observer had a tendency to report in meanings, and a fuller statement of meanings, where the observer tended to report solely in terms of processes.¹

(3) In the experiments of § 3, and to a slight degree in those of § 2, we availed ourselves of a special mode of repetition. If the observer had failed adequately to analyse some complex experience, or if we wished him to verify an analysis already given, or to answer some question after the event, we restored the original conditions of the observation and instructed the observer to 'get back the original complex'. We found that it was often possible, in this way, to reinstate the former experience,—so far, at any rate, as that the observer

¹It should be said that, while F and J tended, after practice, to make their reports of meanings as full and detailed as their reports of processes, D and G (owing, as was later discovered, to a partial misunderstanding of the instructions given) sometimes reported meanings with less completeness. In these cases the experimenter usually had recourse to questions.

The marks of parenthesis were, as a rule, either entered by the observers themselves upon the dictated report, or inserted by the experimenter with the approval of the observer. The reports quoted in the present paper have been submitted to their authors and approved.

recognised the present complex as a revival of the old. Sometimes the experiment failed; and it speaks for the reliability of the observers that they did not hesitate to report failures. Sometimes specific differences were realised between the second and the first experiences. Complete success, under the limitations of the method, was usual with D, G and J; less frequent with F.¹

(1) The method of examination furnishes two kinds of report: the 'selective', in which the observer gives special attention to certain features of his experience, and the 'complete', in which he seeks to reproduce the experience as a whole. Since we were unable to say beforehand what was relevant and what irrelevant to our problems, we asked only for the complete account.

By putting questions to the observer, it is often possible to gain information as to matters omitted from the report; and, what is more important, the bringing of the observer's attention to these omissions leads to their avoidance in future. Owing to the danger of undesirable suggestion, very great care is needed in framing the questions; and a careful record of question and answer must always be kept. Our object in the work of §§ 2 and 4 was to drop them entirely, as soon as the reports became spontaneously complete. During the stage of training, however, the observers were frequently requested by the experimenter to supplement a process-report by naming the meaning, or conversely to supplement a meaning-report by an analysis of processes. After some practice, the recourse to parentheses became familiar: though it should be added that no observer was wholly consistent in their use, or entirely regular in paralleling process and meaning.

(2) We cannot insist too strongly upon the necessity of repeated instruction; the task set is so difficult that even the most reliable and most willing observers tend to omissions. A meaning may be stated, while the corresponding process is in whole or part omitted: thus, an observer reports "general notion of a discussion in that book about the psychology of genetics," and a question is needed to bring out the fact that the 'general notion' was carried in kinæsthetic and verbal terms. Or a process may be described, while the corresponding meaning is in whole or part omitted: thus, an observer reports "sensations of slight strain in chest, as breath was held for a moment; sinking in abdomen; other sensations of touch from clothes; other organic sensations not so clear in consciousness," without giving any indication of the meaning of the attitude. There is often failure, even after practice, to report the time of an occurrence, to state fully the object of perception, to give the stages in the development of a meaning, to rehearse the conscious processes present. The observer must therefore be keyed up to his task by insistent repetition of the instruction.

(3) We do not here enter upon the question—which indeed is a question rather for epistemology than for psychology—how it is possible to give two parallel accounts, in terms of process and in terms of meaning, of

¹This method of repetition was introduced in order that we might determine whether the method of examination satisfied Wundt's requirement of "Wiederholung bei gleichem beobachteten Inhalt" (*Psych. Studien*, iii., 1907, 332 f.). The results are encouraging; though we offer them only as a first contribution to the settlement of the question.

It is perhaps needless to add that recognition is not conditioned upon possibility of description; we often recognise, quite definitely, something that we are entirely unable to describe.

one and the same total experience. The possibility has been taken for granted by previous investigators (Bühler, Dürr, von Aster), and we simply follow in their footsteps. It should, however, be said expressly that the shift of attitude, from process to meaning or conversely, presented—after preliminary training—no insuperable difficulty to the observer. If a process or a meaning stood alone in the report, the failure was due to inadvertence. All the observers found that duplicate accounts were possible, that processes could be summed up in a meaning, and that meanings could be paralleled by processes. We may add that the word 'process' was chosen, not as the equivalent by definition of sensations, images and feelings, but in order to leave room for any other conscious form (*e. g.*, an imageless thought) that might be discovered.

(4) We append a full report on the experience of understanding a sentence. The observer was instructed to open his eyes upon signal, to look at the paper, to get the meaning of the sentence written upon it, and then to close his eyes and dictate his report. The notes which follow the quotation call attention to the details of the method.

Observer F. Stimulus sentence: *She came in secretly.* Time: 1.25 sec. "Purple (from written words)¹ clear. White (from paper)¹ and black (from cardboard)¹ in background, and these were [comparatively]² unclear. Simultaneous with the visual clearness, kinæsthetic-auditory images (corresponding to the words); weak intensity, more as if whispered than as if said in ordinary voice; *i. e.*, lacked deeper tones; and slightly faster than I should ordinarily say them. (The words did not come singly, but the sentence as a whole made a single impression on me; *e. g.*, the period at the end was a part of the total impression. [All this was]³ Perception of sentence as visual and kinæsthetic-auditory impression.)³

"Then vague visual and kinæsthetic image (of Miss X. coming in a stealthy position, on tip-toe with legs bent, through the door into the Audition Room from the Haptics Room)⁴, *i. e.*, blue visual image (upper left part of skirt)⁴ and very vague, featureless image, flesh-colored (of left side of face).⁴ The image (was projected straight ahead of me, to the position in which the door actually is).⁵ Kinæsthetic images in own right upper leg⁶ (which was directly opposite in position to the image, as if my own leg was bent); also kinæsthetic images or sensations in muscles, probably intercostals, of right side (such as I get when standing and bending right leg). (The sentence meant: Miss X. came in over there, through the door, secretly.)⁷

"In the fore-period (I told myself: Get the meaning, and set myself muscularly to work hard)."⁸

Notes.—

¹Reference to stimulus.

²Insertions by the experimenter, for the sake of clearness.

³Statement of object of perception: a sentence which, as yet, was meaningless.

⁴Statement of object of image.

⁵*O* fails to say what processes carried this projection.

⁶By agreement, the reference of a process to the body was not included in parentheses.

⁷Completion of understanding; meaning of sentence has been specialised.

⁸The contents of the fore-period were here not under analysis.

§ 2. THE PERCEPTION OF LETTERS

Our problem, in this group of experiments, was to determine what precisely occurs in consciousness when there is 'perception' of a single letter. The method has been described.

The stimulus was a letter written in long-hand; the time of exposure was left to the decision of the observer, the instruction being that he should close his eyes as soon as he had experienced as much as he could report with accuracy and completeness. Usually, the time of observation was 1 to 3 sec.

The processes involved in perception.—For the most part, the visual sensations aroused by the stimulus are not sufficient, of themselves, to constitute a perception of the particular letter; some additional process or processes must supervene. Since the office of these additional processes is to designate the object of perception, we shall call them, in brief, 'designatory processes'. They generally consist of kinæsthetic or auditory sensations or images as of pronouncing or hearing the letter, or of a combination of the two. The following Table summarises the results.

Observer	Total number of perceptions of letter	D. P. reported	Associative processes reported without perception
D	14	13	1
F	15	10	0
G	10	9	2
J ¹	21	21	2

(1) If the D. P. are absent, there is usually no perception of the letter. Two instances are appended.

Observer D. Stimulus Y.— . . . Then sensations in throat (of repeated contraction and relaxation) accompanied by faint auditory images (of the sound). In the course of this, the perception (of Y) faded away, and attention during this time was on the kinæsthetic sensations and on the idea (that I must not utter the word).

Then (was aware that eyes, which had not been carefully fixating, were now doing it). Strain sensations in muscles around sides of eyes.

Then sensations (of eye-movement) and awareness (that I was following the Y around; and while doing this *it was not Y for me but just a line gure*).

Observer F. Stimulus Z.—When first opened eyes, the black white and grayish sensations became clear (the stimuli being the paper, ink, letter and black background). The extent of the visual field was [O indicates a rough circular outline on the table]. This state was of short duration. (All this was perception of [some] letter on white paper on black ground.)

Later came the auditory image Z and with it the perception (of Z). [All that was at first perceived, as the observer specified on question, was *letter in general*.]

(2) In order to test the above result, the observer was sometimes instructed (oftenest in the case of J) to wait till a time arrived when there were present in consciousness no kinæsthetic or auditory images or sensations as of uttering or hearing the letter, and to begin the report from that time. Two instances may be given.

¹In two cases from J, in which the kinæsthetic-auditory image as of utterance of the letter was probably or certainly lacking, the D. P. were given in the form of images of incipient right-hand movements, as for the writing of the letter. These cases are not included in the Table. On the other hand, the Table contains 7 'repetition' experiments, 4 from F and 3 from J: cf. (5) below.

Observer G. Letter *G*.—Strong strain sensations (from fixation of thickest part of the letter), with great clearness of blue localised to the upper right half, and with special clearness of its extent and form. Much less clear were the other blues of certain extent and form interrupted here and there by white (as if the letter was incomplete, or as if there were breaks in the line.) These other blues were blurred in outline and indefinite in shape and direction. They were simply there, (without seeming to belong together,—which is now carried into this previous experience); [The observer means that the incoherence was present in the original experience, but that he was not aware of it as such; he now notices it, as he frames his report.] (this I call blank empty staring at the stimulus); accompanied intermittently by temperature and pressure and auditory sensations (of expiration) alternately with warm and pressure sensations (of tip of tongue against upper teeth) and by vague white somewhere surrounding the blues. These blues were constant. This whole experience is *not a perception* (of the letter *G*), but merely a conglomeration or concurrence of certain sensations. No conscious tendency was present to articulate.

Observer J. Letter *W*.—There were the black and white sensations, but I can scarcely say that at any time there was perception (of any figure or indeed of anything at all), despite the fact that the visual sensations were clear and intense. The best I can say is, that these visual sensations, along with a vague complex of background processes—(those resulting from position of body, eyes, and possibly also from gastro-intestinal organs)—made up a general attitude of staring, which, however, involved no perception at the time, (though it would be correct afterwards to say I perceive, from memory, that there was a figure of such and such a type).—

From these and similar reports it would follow that sensations may appear in consciousness as such, without necessarily forming part of a particular perception.

(3) It may not be superfluous to state that the D. P. appear in other perceptions than those of letters. Here are three instances:

Observer G. Letter *Y*.—Visual perception, clear (of first part of letter), with slight *kinæsthetic sensations* (of *fixating that part*) and other slight visual sensations (of rest of white field).

Observer J. Letter *B*.—[Next] a period (when the eye changingly rested on certain parts of the upper strokes of the letter) and there was simultaneous *kinæsthetic-auditory verbal imagery* 'thin' (meaning the lines were thin, and thus constituting a perception that they were so). This perception may have had other components, but certainly those mentioned were the only prominent ones.

Observer F. Letter *Z*.—Then attention (caught by pendulum swinging). That is, sensations (from pendulum bob seen in indirect vision) were clear, and there were *kinæsthetic images in neck* (as if to swing head with the bob).

A rivalry of perceptions from the same stimulus may show itself in alternation of the D. P. An illustration follows:

Observer J. Letter *C*.—There was a fluctuation, a struggle of perceptions in successive order. Predominant was a perception (of an apple. Visual fixation was on left side of base of stem of apple). There is no visual image (of an apple), but verbal-motor incipient utterance 'apple' occurred. (When the letter *C* was perceived, the visual fixation was not as just described), and there was no verbal image 'apple'. At times during the perception (of *C* there was incipient motor innervation of the index finger of the right hand to follow the curve of *C*; at times also to continue the movement in the form of an *A*); and simultaneous with this was a visual image with very faint, hazy and shadowy outlines. I do not remember whether verbal images were or were not present simultaneously with the perception (of the letter).

(4) Sometimes the observer fails to report the presence of D. P., and a question is needed to bring them to light. Thus in one case the observer

reports 'attention attracted to the horizontal line,' and only in reply to question by the experimenter is it added that there were simultaneous kinæsthetic sensations from eye-movement,—though these obviously played the part of D. P. in the perception.¹

We have said that, if the D. P. are absent, there is 'usually' no perception of the letter. The rule has possible exceptions. Especially during the earlier observations, J was often in doubt whether there was a perception of the letter at times when the contents of consciousness were predominantly visual. Thus, with stimulus G, he reports "a period during which the visual sensations alone were prominent, with simultaneous pain and pressure sensations about eyelids and probably in other muscles of eyes. During this period there was no well-defined well-developed perception of G; at most there was a hazy and ill-defined perception; but I cannot say with surety whether there was this or none at all."

(5) The Table mentions five cases in which no perception occurs, notwithstanding the presence of associated processes. In three of these, the first, third, and fifth of those quoted below, this failure seems to be due to the absence of clear visual sensations from the stimulus; the fourth may have a like cause, since G, in mentioning vague visual sensations in the fifth, says that perhaps the fourth case was similar; but for the second case we have no explanation farther than that suggested in the report itself.

Observer D. Letter B.—Then sensations in larynx, repeatedly in rhythm. At the same time there was no visual perception (of letter B),—*only vague indistinct sensations (of blackness and whiteness)*, of long duration.

Observer J. Letter M. Time, 20 sec. Instruction: Repeat to get back the visual sensations as they occur when perception of M is absent. [A previous regular report, as well as repetitions of this occurrence with the letter, had been made.]—I am unable to report according to temporal order this time. (The eyes kept running over the stimulus and there was continual tendency periodically to utter M.) At most of these times there was perception (of M), but there were other times when this imaginal utterance was present simultaneously with visual sensations from the stimulus, yet no visual perception (of M). This was succeeded by a period in which, with the same kind of imaginal utterance, there was again visual perception (of M). It was apparent that there was some difference in the visual sensations or in the concomitant kinæsthesia, *i. e.* images (of eye-movement or head-movement). But the difference was delicate and hard qualitatively to describe.

Observer J. Same letter. Instruction: Repeat and imaginally utter M periodically. Time, 3 sec.—(The periodic utterance occurred.) I am not sure but that the visual sensations were attentionally clearer between utterances than at the points of utterance. But it was apparent that the strong perception (of M) that usually attends such utterance was absent.

Observer G. Letter Y.— . . . Next clear verbal kinæsthetic-auditory complex (whispering Y) with faint kinæsthetic sensations (from eye-movement over the whole letter successively in the order of writing it). (All of this is perception of the letter.)

(Next repeated whispering) with kinæsthetic sensations (from eyes moving backwards over whole letter); same kinæsthetic-tactual-temperature [complex] (from exhaled breath) vaguely present. (This repetition is not a perception,—but merely a concurrence of these mental processes. I am unable to say what exactly is the difference in consciousness [between

¹The stimulus in this case was a geometrical figure. With the letters, and indeed with any form of frequently recurring stimulus, such cases become, in our experience, rare. It should, however, be added that the D. P. are by no means always obvious; sometimes both skill and practice are required for their detection.

the perception and non-perception as they occurred above]. Am doubtful as to whether there was any conscious difference, unless the first repetition was accompanied by a vague feeling of familiarity—slightly pleasant, while the later repetitions were indifferent, and, so to say, automatically continued.)

Observer G. Same letter. Instruction: Repeat the mental situation [as above].— . . . Next (whispering) complex becomes still less clear. Vague visual sensations, black and white, without any connection between them in consciousness,—no consciousness of their form or extent. Drowsy sensations practically indifferent. (All this is not perception of Y.) This description of the non-perception of Y differs from the original non-perception [*i. e.* that described in G's report above] in point of the drowsy sensations and the vague visual sensations,—both of which may have been previously present, but which were not reported.

Perception as meaning.—We turn now to the 'meanings' that appear parenthesized in the reports of our observers. The main point to note is that *the precise statement of meanings is by no means easy*. Just as processes flit by on the passing instant, so do meanings change and elude the observer; and the skill in expression of meaning acquired in daily life is comparatively rough and superficial. This fact may be illustrated in two ways.

First, it is often not enough to record simply that 'the perception of the letter' occurred; what is perceived is frequently—perhaps always—something more complex. We gave to F the special instruction that he should state, precisely, what he perceived; and the result justified the specialisation of method. For example:

Letter Z.—(As soon as I opened eyes) perception (*of Z placed on white paper in a particular direction from left upper corner of paper*). This was clear visual sensations (from black Z and white paper), also sensations (from upper left-hand corner of paper). The attribute of extent [form and position] of this corner and the visual sensations (from Z) were clearer than the sensations (from the white paper), which in turn were perhaps clearer than those (from the black background). Simultaneous or immediately after and forming a part of this perception, abbreviated auditory image Z. ((I notice now, in reporting, that this image was purely sibilant.)) There were also vague kinæsthetic images or sensations in throat and lips, those in lips being the more noticeable.

Letter D.—(Soon as opened eyes) gray and white clear (from paper and ink). Simultaneous auditory image D. These visual sensations were clear only for a brief time, about one-tenth of the whole period. The auditory image was of higher pitch and less intensity than it would be from spoken D. Its other attributes, clearness and duration, were the same as if I had uttered D. (There was *perception of D on white paper*.)

Secondly, the stimulus frequently arouses other perceptions than those of the particular letter, and the object of these perceptions needs careful statement. Examples may be found in reports already quoted; we add one further instance:

Observer D. Letter A.—Kinæsthetic sensations retreat to margin of consciousness; become non-focal, non-clear; simultaneous visual perception (*of a dark line of the shape A on a white ground; it was not perception of*

A). Sensations of eye-movement, plus an awareness (of the same along the figure, thus) [observer indicates the direction, which is that taken by the pen in writing the letter], plus kinæsthetic sensations especially in the neck, but not definitely localised and not distinct.

Summary.—The perception of a particular letter usually depends upon the arousal of contextual associates, which we have termed 'designatory processes'. The direct visual apprehension of the stimulus, *i. e.* the presence merely of ordered visual sensations, does not suffice as a rule, under the conditions of our experiments, for the perception of the letter.

These designatory processes may characterise other perceptions, as well as the perception of a letter.

From knowledge of the stimulus, the experimenter cannot determine the nature of the perception at a given instant; a report of the precise object of perception must be obtained from the observer.

Variation of the object of perception, with a given stimulus, is accompanied—again, under the conditions of our experiments—by variation of the concomitant or underlying 'processes'; this variation may usually be traced both in the designatory processes and in the processes which subserve accommodation of attention.

§ 3. THE MEANING OF WORDS

The experiments now to be reported were the first made in the present investigation; the method was tentative, and the observers were comparatively unpractised for the problem in hand. The usual method of procedure was as follows: A written word was laid before the observer for a period of 1 min. He was instructed to fixate the word, to utter it with quick repetition, and to get its meaning. The concluding 10 sec. were marked off by signals; and the observer's task was to report what occurred in consciousness during this particular interval.

Our aim in adopting this method was to secure frequent appearances and disappearances of the verbal meaning, and so to provide repeated opportunities for its analysis. The method was fairly successful, though the period of 10 sec. proved to be too long for a complete report; the experimenter was therefore obliged in many cases to have recourse to questions—made as little suggestive as possible—in order to secure omitted information and, less frequently, in order to verify the absence of an unreported item.

The special form of the method which involved *repetition* has been described above, p. 555. Another variation was sometimes introduced, by which a feature of the original report was *eliminated*, and the consequence of this elimination noted. Thus, with the word *silently* G reports the presence of kinæsthetic-verbal images 'still' and 'silently means *ruhig*'; these images carry the meaning of the stimulus-word. He is thereupon in-

structed to fixate the word and to articulate, as before, but not to permit the rise of such verbal associations. The report of the changed situation reads: 'No meaning to the word. Just sounds and just sensations from articulation.'

The repetition and prolonged fixation of the stimulus-word had the effect, as we expected, of intermittently destroying associations. But they led also, in some cases, to the disintegration of the perception itself. Special parts of the word might stand out and be perceived in place of the whole. Thus, a kinæsthetic or auditory or combined image of one of the letters arises, accompanied by visual fixation of that letter, and perhaps leaving the rest of the word visually (peripherally) obscure: then there is perception of the single letter rather than of the whole word, despite the fact that the word is being uttered. Our records suggest, though they do not prove, that so long as there are visual sensations from the whole of the word, with simultaneous enunciation of it, the perception remains.—Cf. E. Severance and M. F. Washburn, this *Journal*, xviii., 1907, 182 ff.

No definition of 'meaning' was furnished by the experimenter. F at first showed occasional uncertainty as to what constituted meaning; and D for some time showed occasional doubt and inconsistency. Eventually, however, the reports of all four observers became practically uniform. It is needless to say that no observer was informed of the results obtained from the others, and that all were cautioned not to discuss the experiments outside of the laboratory. Illustrations of what were called 'meanings' follow.

Observer D. Stimulus *bloody*. [The word has been articulated and fixated for the previous 50 sec., and these activities are continued during the final 10 sec.] After the signal I said to myself, Must get meaning again; and then said, Must the blood be running?—accompanied by a visual image of an animal of indefinite shape with a flowing wound: Or may it be dry?—now with a visual image of same animal, but I was looking at the edges of the wound where there was coagulation. Visual image of some animal on table, and of Mr. X saying: So-and-so is foud of seeing blood run. Then lost meaning.

Observer D. Stimulus *secretly*. [Conditions as above.] Just after the signal I tried voluntarily to get back to what I had before, when I had the bodily attitude of hiding or concealing. [Later] a visual image of a girl whispering to me disappeared suddenly, and I was left just saying the word.

[In order to give opportunity for the analysis of this imaginal bodily attitude two repetitions (p. 555) were made. Both were successful; in the first repetition the attitude was declared more distinct than in the original experience. The reports, supplemented by questions, brought out the fact that the attitude was wholly kinæsthetic; the observer was crouching, and concealing an object in front of her with body and hands; she was aware of people behind her, who, however, were not given in visual images, but were implied by the nature of the attitude.]

Observer F. Stimulus *face*. [Conditions as above.] When signal came was saying to myself: Wonder whether he wants me to get a noun or a verb. Then pulled myself together [observer indicates retractive movement of arms and inward movement of chest, with forward tension of shoulders and head leaning forward]. Now with attention to sound of voice it was as if I were telling myself to face something. All strains seemed to drag me to the front, and I said: Verb,—with accompanying auditory image. Then vague visual image of experimenter's face, and

then of my own. . . [The attitude here carried the verbal meaning, the visual images the substantive meaning.]

Observer F. Stimulus *to*. [Conditions as above.] Visual image of a clothed right arm reaching out to the storm-door at the front of this building. While this image lasted, attention was on sound of voice; and then the arm reached *to* the door, but did not open it. This recurred once or twice, except that attention was no longer on the voice. Then I thought I ought to get some other meaning. Then verbal-auditory image *to him*, with kinæsthetic image of moving left hand, which was held forward, from left to right. [During the entire period the observer had nodded his head vigorously with each enunciation; and questions bring out the fact that this gesture means for him the instruction: Get that meaning!]

Observer G. Stimulus *to*. [Conditions as above. Two meanings are given below; the rest of the report, containing two other meanings, is omitted.] Strong kinæsthetic tendency to move to right in the direction of the end-stroke of the letter *o*. The *to* meant a direction, a going somewhere, similar to that given by a guide-post, and there was a sense of being at a loss. . . . Then the numerical meaning, in the form of putting two fingers on the table.

[Instruction: Repeat, and get back the first meaning.—I do not know whether it came as completely as before. There was a tendency to move eyes and body to the right, and to pronounce the word briefly as if saying: To—some place. There was no more of the *Bewusstheit* of direction than this. There was strong fixation of the last part of the word.]

Observer G. Stimulus *cutting*. [Conditions as above.] Meaning present as a faint visual image of a knife-blade and a kinæsthetic tendency to press it down. [Where was that tendency?] In the first three fingers of right hand; it was accompanied by movement of eyes to the place on the right.

Observer J. Stimulus *botany*. [Conditions as above.] . . . Remembrance that must concentrate on meaning. [Not analysed.] Then visual image of green plants and a recently seen hot-house. This disappeared, leaving only the sounds from enunciation. Later an attempt again to follow the instructions [not analysed] was followed by the motor expression 'study of plants' and still later by 'study of plants and flowers,' and these phrases were frequently repeated, notwithstanding the simultaneous enunciation of 'botany'.

The meaning of the stimulus-words were thus carried by visual, auditory and kinæsthetic processes; or, to speak more precisely, the meanings which these processes bore were the meanings of the stimulus-words, in so far as the latter were consciously realised. If we may use the term 'association' in the widest sense, to denote peripheral-kinæsthetic as well as imaginal processes, we may say that the meanings were given in the shape of associations to the words. But the associations to a given word do not remain constant: thus, the visual image of plants and a hot-house, associated to the word *botany*, gives way a moment later to the verbal-motor 'study of plants'. It seems to follow that *the meanings of the words, so far as they are conscious, vary as the associations vary*. The logical meaning of a word, as expressed in a formal definition, does not change; but what we are studying is not this perfect logical meaning, but rather the phases of meaning or the part-

meanings carried by certain transient processes; and as thus understood the meaning must be said to vary.

If the associations are absent, meaning is also reported as lacking. Here are some examples:

Observer G. Stimulus *loud*.—The first impression was of an *Aufgabe* given me by the word, and I started to speak loudly. After several repetitions this *Aufgabe* came again, but then gradually became unconscious, and there was mere mechanical pronunciation. Then verbal image *laut*, leading to stronger accent on the *d* during enunciation. With a new inhalation the same *Aufgabe* returned, and there was greater muscular effort in articulation for the next few pronunciations.

[Instruction: Repeat, without getting this *Aufgabe* association or other similar ones; but try to get what you can of the meaning, and then report.—Practically nothing under these conditions besides the visual and kinæsthetic perceptions, the latter being especially clear. The word has an empty look; I don't know how to describe it.]

For another like case with G., see pp. 562 f.

Observer D. Stimulus *kill*.—The first signal suppressed a coming visual image of an object floating on the water, a clipping from a newspaper, etc. [Then] visual image of the physiological laboratory and of a pithed frog, with appropriate tactual and organic sensations. Visual image of the operating room and of an animal I had killed through over-etherization. Then [other similar images].

[Instruction: Repeat, and get none of these associations, and then describe.—I got a few motor-auditory verbal images,—'to murder', and 'to destroy life'. There were vague sensations from bodily position, and a strain to get something else besides these images. [What?] Tension in my head, and a slight tendency to scowl.]

Instruction: Repeat, and do not get these verbal images.—The word is quite lifeless and meaningless. [The observer adds incidentally that this meaningfulness had its organic side—weak breathing, a let-go feeling, a depression.]

Observer J. Stimulus *piano*.— . . . Then the writing was no longer in consciousness as a word, but rather as a collection of curved lines.

[Instruction: Repeat, to see what is in consciousness when only these lines are present.—I fixated one letter after another, each time pronouncing the whole word. The other letters were all in consciousness, but not so clear; nevertheless the word was present as a whole. But at times, when fixation was on the *a* or the *n*, there occurred slight optical divergence, and the whole word became slightly [peripherally] unclear. This was continued until there was no consciousness of any of the individual letters seen as such, but only a consciousness of wavy blue lines with a tendency to follow them with the eyes and with the right hand from left to right and back again. But in this the lower parts of the *o* and *a* were omitted. [Apparently here also there are no associations to the word as such, and it is meaningless.]]

We did not find a characteristic variation of associations with the different parts of speech. Those which stood for the meanings of prepositions, *e. g.*, were not invariably motor tensions or impulses.¹ On the whole, kinæsthesia was more prominent with prepositions than with nouns like 'piano' and 'dog'; but visual and auditory processes were also involved in the meanings. Here are instances of various kinds:

¹Cf. E. H. Rowland: The Psychological Experiences connected with the Different Parts of Speech, *Psych. Rev. Mon. Suppl.* 32, 1907, 25.

With F, stimulus *to*, the report cited on p. 564 shows that the prepositional meaning is at first carried purely in visual and auditory terms. Again, with stimulus *for*, a report runs: Auditory 'for me', with visual image of *me* written on the paper. Slight tendency to lean forward; rather pleasant. Auditory image: What for? with accent on the *for*. The *for* became very clear.

Again, vision may be mixed with kinæsthesia. Observer D. Stimulus *upon*.—In the fore-period I had visual-kinæsthetic images of myself standing on a pile of wood. And I had various objects given more kinæsthetically than visually,—usually adjusting body for looking from one to another.

Similarly, the meaning of adverbs may be given visually, auditorily or kinæsthetically. Observer F. Stimulus *heavily*.—Visual image of gray cube of iron several times falling on floor of the Audition Room. An unclear auditory image of the noise. Strains in ear-drum. Organic sensations in abdomen such as are involved in hearing a weight dropped, and such images as one would get from a jar of the building. Whole experience repeated a number of times, not quite as fast as I uttered the word. Tendency to nod head synchronously with utterance: meant 'heavily'.

It would be tedious to illustrate this point with reference to adjectives, substantives and verbs; let it suffice to say that with these as with the other parts of speech, as classes, there appeared no characteristic *differentiæ* of associations.¹—

There were associations reported which were not called 'meanings'. Thus, G reports with stimulus *cunning*:

Certain verbal processes which I should call meanings, and certain others which I should not. Belonging to the last class was 'Cunningham,' formed by adding 'ham' to what was being said aloud. Then visual image of a ham. Then verbal question: What is cunning? followed by verbal image *wise*. Verbal question: What else? then vague complex of the difficulty I should have in writing a definition of cunning. I cannot analyse this, but it included frowning and strains in neck.

What, now, is the difference between these two kinds of associates,—those that carry the meaning of the stimulus-word and those that do not? The question may be answered from two points of view. If we regard the associates as 'processes', in the sense of § 2, then we must reply that the meaning-associates proceed from the instruction given, while the not-meaning-associates are external to the instruction; the former indicate the activity of a particular determining tendency, the latter indicate the activity of reproductive tendencies not connected with this determination. If, on the other hand, we regard the associates as themselves 'meanings', again in the sense of § 2, then we must reply that the associates which carry the meaning of the stimulus-word are, as independent part-meanings, logically relevant to the total word-meaning, while the associates which do not carry the meaning of the word are as independent part-meanings

¹Thus, we found nothing that could warrant such a generalisation as Rowland makes in the case of adverbs: *op. cit.*, 27 ff.

logically irrelevant to the total word-meaning. Both of these replies, however, require qualifying comment. First, the observer is not (at least, in our experiments was not) aware of any introspective difference between the processes associated under the instruction, and the external associates,—between the processes which carried the word-meaning and the processes which were outside of that meaning. There is no modal or qualitative difference; there is no special 'feel' of 'belonging' to the instruction, or to the situation induced by it¹; simply, the observer is able, on question, to point to certain associated processes as carrying the meaning of the word and to certain other processes as not involved in the word-meaning. Secondly, the independent part-meanings borne by the associates are not necessarily their obvious or face-meanings; the test of logical relevancy or irrelevancy cannot, any more than the test of procession from the instruction, be applied by the experimenter on behalf of the observer; some ingrained habit of the observer in regard to reproductive tendency, or the disposition into which he is brought by the present situation, may give all manner of warps and twists to the part-meanings carried by the associates as such; constituent processes, which appeal to the experimenter as vehicles of a definite part-meaning, may prove to be extrinsic to meaning, may (in popular phrase) be 'ignored' by the observer; and constituent processes which appeal to the experimenter as casual may turn out to be, for the part-meaning, essential. In every case, then, we are forced back upon the distinctions drawn by the observer; there is no criterion, whether psychological or logical, which can be applied by the experimenter in default of the observer's specific statement.

If we seek to analyse the instance given above (Observer G, Stimulus *cunning*), we reach the following general result. First, to take the associates as processes: we have the utterance of *cunning* arousing, by mechanical sound-association outside of the instruction, the familiar name *Cunningham* (the name of a friend and colleague); and we have then the added member *-ham* (the observer himself notes the 'addition' of this member) arousing, still outside of the instruction, the image of a ham. Thereupon the observer harks back to his instruction: and his return is effected, typically, in verbal imagery. 'What *is* cunning?' he asks, in internal speech, and the verbal image *wise* appears, issuing from the instruction 'Get the meaning.' The processes *Cunningham* and *ham* do not aid in carrying the meaning of the stimulus-word; the process *wise* does so aid.

Secondly, to take the associates as meanings: *Cunningham* and *ham* have their own independent meanings, irrelevant to the meaning of the stimulus-word *cunning*; they form separate constellations, outside of the

¹It should be said that the observers were not specially questioned upon this point. As the reports stand, however, there is no indication of any 'feeling' of direction or of guidance or of any regional consciousness. The instruction itself was carried in the usual and typical ways; we do not think it necessary to give illustrations.

instruction. *Wise*, on the contrary, has a fringe of meaning of its own, which is logically relevant to the meaning of *cunning*.

We have chosen this instance for analysis, because it is unusually simple; because in it the experimenter can, to some extent, put himself in the observer's place, and see the 'reason' for the admission of some associates to the rank of vehicles of word-meaning, and for the rejection of others. But the simplicity of the instance is quite unusual; and, for that matter, we have no doubt that our analysis, undertaken after the event and on general psychological principles only, is far from complete.

Although the observer was able, without hesitation, to make the distinction between meaning-associates and associates that had no share in the meaning of the stimulus-word, the relation of the meaning-associates to the word-perception was never reported as a specific and characteristic conscious reference. Special questions were therefore asked, in order to determine whether such a specific reference came to consciousness.

Observer G. Stimulus *cutting*. [Question, following report on p. 564: What was the connection in consciousness?] Simply simultaneity. There was no apperception of their belonging together; in fact they did not occur at the same place, as the kinæsthetic motor tendency was in the right hand and the faint visual image was here [indicating a certain place on the table toward which the eye moved and where the imaged hand had not been]. [Was there any conscious connection between the visual image and the word, *i. e.*, the sound and sight of it?] No.

Observer G. Stimulus *Roosevelt*. . . . Vague visual image, a circle with three lines in it. [What connection had the circle with Roosevelt?] That is the visual image I have from caricatures of Roosevelt, the circle meaning his head, the lines his teeth. [What connection was there consciously between the circle and Roosevelt?] I don't know what you mean by connection; the only connection I see is that they came simultaneously or successively.

Observer D. Stimulus *face*. The observer reports visual image of a mask and slight eye and head movements as if to look at it. [What was the conscious relation of that mask to the visual-auditory-kinæsthetic impressions from the word?] It did not have any; I did not consciously refer it to what I was seeing at all.

Observer J. Stimulus was a proper name, and verbal imagery 'the experimental psychologist' had been reported. Observer adds: I cannot answer the question whether there was any conscious connection between the sensations from enunciation and this verbal image. The question seems strange.

[Instruction was given to repeat.] The images came as before, but more vaguely. . . . I found a certain conscious spatial relationship, namely, the visual image appeared close to the word seen; but I was not able to ascertain whether there were other conscious relationships.

We are thus led to the conclusion, indicated in a previous paragraph, that the conscious 'meanings' brought out in these experiments are not the perfect and static logical meanings of definition, but rather partial meanings, particular exemplifications, or what not, touched off under the given instruction by the habit or the momentary disposition of the observer. Logically, the representation of meaning is inadequate; psy-

chologically, it is adequate to the demands of the occasion. We may add that, especially at the beginning of the work, the observers often showed a tendency to verbalise a definition of the stimulus-word, and thus to meet the situation with logical as well as with psychological adequacy.¹

§ 4. THE UNDERSTANDING OF SENTENCES

In this part of our study, the stimuli were simple sentences, type-written. These were laid before the observer, who was instructed to open his eyes upon a signal; to read and understand the sentence before him; and then to close his eyes and recount his experience.

We shall outline the results from each one of the observers.

Observer D. Stimulus *Her dress was white.* Time 2.5 sec.—(After the ready signal) sensations of kinæsthesia and strain in head and neck region. Simultaneous awareness (of the *Aufgabe*, and determination to get full meaning); a special set of strain and other organic sensations belongs to this.

(Then signal Now, and opened eyes.) For a moment dazed feeling and blurry sensations (from incomplete fixation) of light on dark.

Then a kinæsthetic dart or snap in head and (sentence) was visually clear. [Later question: Describe this dart or snap. 'In top of head and around eyes'. In scalp? 'No; inside head'.]

Then vague kinæsthetic sensations in throat and indefinite auditory images (accompanied by automatic reading of sentence).

Then mixed-up feeling, unpleasant; sensations of nausea and (of inhibited breathing), (all this meaning: I don't know what I am to do). The whole field of vision was obscure.

Then (rapid eye-movement); quite definite kinæsthetic sensations, but hard to describe. Mixed-up feeling continues.

Then visual image (of myself in a particular white dress). Image was very small and very indistinct, and the kinæsthetic accompaniments were more prominent than the visual. (Definitely localised to the left.)

Then feeling of doubt; (again rapid eye-movement); muddle of organic sensations and unpleasantness. (Signifying: Is this the meaning?)

Then feeling of relief; (general relaxation); totally different set of organic sensations from above. Pleasant. Kinæsthetic sensations in throat (meant assurance that I had the meaning).

This report is typical, in so far that D always records the automatic reading before she gets the meaning of the sentence. It is typical of about one-half of her reports, in that it shows her doubt whether she shall identify the associated ideas, aroused by the stimulus, with its meaning. It is apparent

¹Since the experiments here reported were concluded, the writer has found that, if he reads any particular word upon a printed sheet (looks at the word, and gets a kinæsthetic-auditory repetition of it), there is usually attached to it a thin coat of meaning which distinguishes it from other words similarly read, though there is a total absence of recognisable associations. Save for two or three possible instances, whose interpretation is not clear, such direct or incorporated meaning did not appear in the experiments of this Section. On the general question, see Titchener, *Thought-processes*, 1909, 177.

that she finally does thus identify, after finding that nothing else occurs which can be termed meaning. The following excerpts from other reports illustrate this point:

D. *It is very warm in this room.* Kinæsthetic sensations in throat plus auditory images (of words). (Read the line.)

Then a curious feeling, largely organic sensations of general laziness, pleasant warmth, drowsiness, and kinæsthetic sensations (chiefly of eye-movement and strains in head that meant my Office, where I had ten minutes ago been very warm). [The observer reports that here was the meaning of the stimulus sentence.]

D. *Let him bring a glass of water.* . . . Then feeling (of relief), that is, mild pleasantness and less strain in head and different organic sensations in region of diaphragm. Verbal kinæsthetic idea (meaning I don't have to do or say anything to get the meaning; I just know I understand it). At same time there was some kinæsthesia (from eye-movement?) (that constituted meaning of sentence).

Then and slightly overlapping the above, very vague schematic visual image (of some man in the laboratory, I don't know who, standing at sink and holding a glass before the running water). All this was just in grays.

Then verbal kinæsthetic idea (Perhaps this has something to do with the meaning).

D. *She came in secretly.* . . . Then slight kinæsthesia (from automatic reading of the sentence). At last word kinæsthesia (accompanied by sudden eye-movement or blinking). (After this did not fixate paper.) [Later question: Was the meaning of the sentence present here? 'No.' Did you perceive the words or sentence? 'Yes; but *secretly* is the only word I perceived very clearly.']

Then a visual image, vague and schematic (of a girl who was sometimes myself and sometimes Miss X walking on tiptoe into my Office). At the same time organic and kinæsthetic sensations (as if I were going through that performance), namely, respiratory sensations (from repressed breathing), general kinæsthesia (from slight tremor of whole body), articulatory sensations, kinæsthetic sensations (from walking on tiptoe), and contact sensations in arms and hand (from touching sides of doorway as I entered). All this organic and kinæsthetic complex was the clearest thing in consciousness. There were quick alternations of pleasant and unpleasant feelings accompanied by kinæsthesia which I can't analyse now, though it was definite at the time.

Then feeling (of assurance) in terms of respiratory sensations (from rather deep and free breathing), and a certain kinæsthetic complex (from eye-movement, meaning that this attitude of walking in secretly conveyed the meaning of the sentence.)

Next we give a sample of G's reports. It will be seen that there is a very full description of processes. The reader is requested to attempt, as he goes through the report, definitely to decide at what point, if at all, the meaning of the sentence was realised.

Observer G. Stimulus *Did you see him kill the man?* Time 3 sec.—Auditory perception (of word Ready). Simultaneous unpleasant strain and tactual sensations (from hands on face and general position).

Then a pinkish grayish limitless visual perception (of the field of the closed eyelids) accompanied by vague kinaesthetic strain in region of eyes and in eyes.

Then auditory perception (of Now). Faint verbal idea (meaning What a difference in intensity between the Now and the Ready)! The opening

of the eyes is accompanied by a succession of blurs, partly gradual, partly sudden, with vague strains from the front part of the eyes themselves. At the same time recognition (of the white strip) with indefinite indistinct outlines, and with similarly indefinite blue sensations strung along a horizontal line in center of the white area.

Then faint strains (from fixation of blue complex) accompanied by verbal ideas of articulation.

Then (new fixation) and repetition of these ideas. Background of consciousness almost zero.

After the perception (of the last word) a sudden rise of all sorts of organic, kinæsthetic and tactual sensations localised in abdominal region, mouth region, elbow, and facial areas touched by hand, together with a new visual perception. [Later question: Of what? 'Of objects on table, eyes being open'.]

Followed by verbal idea (What is it?)

Then a general, organic and respiratory, conscious attitude (of relief). (No meaning all the way through.)

It is certain, if we may trust our own experience, that the reader who tried to discover the point at which a meaning might have been realised failed in the attempt; and the failure emphasizes the difference that we have drawn between report of 'meanings' and report of 'processes'. Or, to put the matter differently: If the observer had omitted the information 'No meaning', and had challenged the experimenter or any one else to state when (if at all) the meaning of the sentence was realised, and what this particular meaning was, the person thus challenged would have found it impossible to infer the meaning from the description given of the corresponding processes. Information about meanings as well as description of processes—we have made the point before, and we shall recur to it again—must come always from the observer himself.—

There are five other cases in which no meaning is reached. Sometimes meaningless reading is followed by the meaning. Thus:

Observer G. Stimulus *The iron cube fell heavily on the floor.* Time 4.5 sec.—Visual perception (of words) accompanied by imaginal and articulatory processes (of reading). The first perception (of the third word) was vague; (in fact, it was not a word but a blot). It became a word as soon as certain parts (of the blot) stood out more clearly and were verbalised. (The rest of the sentence was first perceived as meaningless words, then re-read) with strong motor tendencies around the eyes (meaning attempt to see an iron cube fall down from the table.) The perception (of the word Floor) was accompanied by a faint auditory image (meaning a very loud sound). (Then closed eyes.)

In yet other cases the meaning comes simultaneously with the perception of the words, and is carried by non-verbal images or sensations. We may therefore say that (save for one instance, which resembles the three peculiar reports of F to be discussed below) the reports of G are like those of D; the perception of the words, that is, visual sensations accom-

panied by designatory processes, does not necessarily involve awareness of the meaning of the sentence, which either (1) comes in terms of non-verbal images or sensations, appearing simultaneously or later, or (2) does not come at all.

The reports of F show two types: in the one, perception of the words or of the sentence precedes the meaning, which finally appears in terms of non-verbal images or sensations; in the other, these meaning-associates occur simultaneously with the perception. There are, however, three reports which stand by themselves. We give two of them, in part:

F. *It is very warm in this room.* 2 sec.—Purple sensations (from words) clear. White sensation (from paper) and black (from background) in background of consciousness. Also very weak strain sensations in chest, in background, which remained comparatively constant in intensity while I was reading. Simultaneously with the reading, auditory images (of the words). (Strain sensations mean: I am under *Aufgabe* to read and interpret and not to waste too much time. Visual sensations plus auditory images carried in themselves the meaning of the sentence.)

F. *The affair was bewildering.* 1 sec.—White and black sensations (from paper and background) in background of consciousness. Simultaneous with the visual clearing of each word, auditory images. (The meaning of the sentence was in the auditory images and visual sensations themselves. *No other context to carry the meaning that I can find.*)

If we may assume that F has not overlooked something, we have the result (confirmed by a single case from G) that the visual and auditory images and sensations from reading might be the sole processes present in consciousness, while yet the sentence had meaning.¹

We turn now from 'processes' to 'meanings'. And we note that it is not enough for the observer to make the bare statement that he did, or did not, understand the sentence. For oftentimes, at the moment of understanding, the sentence has a special or peculiar meaning.

An illustration has been given in the report on p. 557. Another follows. F. *His face was very serious.* . . . (Read the sentence over again), that is, visual sensations and auditory images as before, except at a slower speed. Accompanied by kinæsthetic sensations in face (from frowning) and, I think, sensations or images from (slight nods of head towards the words, for emphasis). (Determined effort to see what the sentence meant. Meaning clear this time.) [Question: What was that meaning? The answer came with conviction and immediately.] (*My face is very serious.*)

¹Cf. p. 569. The writer finds that he can converse or think in words or in incipient verbal articulations, with the meaning present, while for considerable periods of time he can discern no vestige of sensations or images other than those from the words themselves. There are, in the background, sensations due to bodily position and to general set; but while it is introspectively clear that these play an important part in the whole experience, they do not seem to vary correspondingly with the verbal meanings, as the conversation proceeds or the thought goes on.

Our results do not tell us what is the difference, if any, between the processes occurring in these cases and in those of meaningless reading of the sentence.

So in the case of G: two reports obtained from the same sentence *Her dress was white* show that on the one occasion *her* referred to a particular person, on the other to nobody in particular.

The Single Word and the Word in Context.—Every sentence employed as stimulus in these experiments contained, in a prominent place, one of the words that had been employed singly in the experiments of § 3. Several months intervened between the two sets of experiments; and we have no reason to suppose that the repetition was remarked by any of our observers. Our object was to compare the meaning of a word presented singly with the meaning of the same word given in a verbal context. The conditions of the two sets of experiments were by no means parallel; still, certain results appear to be trustworthy.

There are a few cases in which the associates of the single word recur (usually with some alteration of form) in the cluster of associates aroused by the sentence. Thus, in both experiments a proper name calls up, for one observer, a visual image of the same person; 'process' and 'meaning' are identical in the two reports. In another instance, the word *face* has the same reported meaning under both conditions, though the 'process' appears in the one experiment as a visual image, in the other as a kinæsthetic complex.

In the great majority of cases, however, the associations traceable to the word in context are not those previously aroused by it in isolation. This result harmonises with the statement made on p. 564 regarding the variable character of meaningful associations. The word-in-sentence is not a separate stimulus, but merely a constituent of a total stimulus, which is the sentence; as constituent of the total stimulus it may, of course, set up determining tendencies in the sense of its own meaning; but this meaning is now only a phase of the total meaning of the sentence, a meaning of incorporation or of implication; and it is therefore impossible to predict, from the report on the single word, how the meaning of the word-in-sentence shall appear in consciousness. G reports, with the stimulus *heavily*, 'Meaning was mostly kinæsthetic, and secondarily organic.' With the stimulus-sentence *The iron cube fell heavily on the floor*, this mode of meaning has lapsed; the effect of the word *heavily* shows only in the 'faint auditory image (meaning a very loud sound)'. We may refer also to the reports of D on *secretly* and *She came in secretly* (pp. 563, 570), which illustrate the same point. The difference was especially marked in the case of prepositions: taken alone, these words tended to form a context of their own, verbal or attitudinal (by gesture); occurring in a sentence, they simply colored the meaning of the total stimulus.—Cf. the remarks of H. M. Clarke, this *Journal*, xxii., 1911, 236 ff.

Summary. 1. The meaning of a sentence is often entirely lacking at the first reading, *i. e.* the initial perception of it, and appears later, borne by processes representative of its content or of some response to that content made by the observer.

2. Sometimes these representative processes come with the initial perception, and the sentence at once has meaning; sometimes they seem to be absent, while the meaning nevertheless arises.

3. The same stimulus-sentence may give rise to different meanings for the same observer, so that it is not enough for him to say that he understood it; he must be asked to specify precisely what he understood.

§ 5. IN REPLY TO CRITICISM.

The discussion of Imageless Thought has led, time and again, to personal exchanges of regrettable warmth. Yet the issue is, after all, an issue of fact; it is the observations that count, and not the thrusts of controversy. When, for instance, Dr. Watt suggests that an observation made in the Würzburg laboratory is *eo ipso* more dependable than an observation taken in the Cornell laboratory;¹ when, forgetting the genesis of his own Theory of Thinking, he belittles the work of graduate students;² when Professor Ogden charges that Okabe's analyses of Belief "would apply equally well to a description of the æsthetic attitude, the ethical attitude, the consciousness of understanding, or indeed any other of the higher apperceptive states of mind;"³ when he remarks that Clarke's conscious attitudes are "unblushingly" analysed into sensory and imaginal components;⁴ when he declares that the method of confrontation is "quite a perfunctory affair" and leads to an "equivocal result;"⁵ when he cleverly dubs the sensation-alistic school 'the opposition', and thus puts the champions of imageless contents into the secure position of governmental orthodoxy:⁶—in all these, and in many similar instances, the polemics simply mean "I refuse to accept your results." Or perhaps, since the phrases are polemical, they may carry the further meaning, "although I can't explain them away;" for emotion is likely to appear when argument has broken down.

Let these things pass, then, and let us come to close quarters with Professor Ogden's criticism. This is, in a nutshell, that Cornell observers have been predisposed against "the discovery of meanings in experience", and have therefore confined their introspections to the "known mental categories of sensation, image and feeling in which [they] have been schooled." The best reply to the first of these statements is the fact of the present paper. Professor Ogden's critique appeared on June 15, 1911; and, by that date, the experiments by our 'method of parentheses' had been concluded. It is true that previous Cornell experimenters have intentionally neglected meanings, *in the sense of this term used in the present paper*. But, so far from having a predisposition against meanings, we have in the present work made a systematic attempt to cultivate reports about them. And we reach a result which does not

¹Mind, xx., 1911, 403.

²*Ibid.*, 403 f.

³Psychol. Bulletin, viii., 1911, 194.

⁴*Ibid.*

⁵*Ibid.*

⁶*Ibid.*, 186 f.

accord with Professor Ogden's views: we find that *wherever there is meaning there are also processes*, and we find that the correlated meanings and processes are *two renderings, from different points of view, of one and the same experience*.

We have already stated that it is frequently no easy matter to give a detailed account both of attributes of process and of shades of meaning. the beginner who is set for the report of meanings will be likely to overlook the corresponding processes, and conversely, just as, if he is set for the report of the quality of a sensation, he will be likely to overlook the correlated sensory intensity, and conversely. To be sure, after considerable practice it becomes tolerably easy to report the principal features of the double task, but even then omissions sometimes occur.

But it must be remembered that the danger of defect is two-sided; it inheres in either mode of predisposition. When Professor Ogden writes: "It is precisely in the brief moments of active thinking that the thought-factor is most apparent" (*op. cit.*, 187), he lays himself open to the very objection that he is urging against his opponents. If by the thought-factor is meant the meaning, the topic or object of thought, that must, in the very nature of things, be most apparent under the conditions of quick active thinking; and, again in the nature of things, the corresponding processes must, under such circumstances, be least apparent; the observer is set for meaning,—and even if the instruction is changed, and he is later set for the reporting of processes, the brevity of the experience will work against him. But there is absolutely nothing in the case to compel our belief that meaning without process exhausts the experience, that process is altogether absent.

A like reply might be made to the complaint of Professor Ogden's colleague (*op. cit.*, 193). If the relatively untrained observers gave plentiful meanings in their original reports, and failed to specify processes, that is because they had not been taught to distinguish between process and meaning and to report on the former as well as on the latter. If the trained observers of the later work gave nothing but sensations and images and feelings, that is because they had been taught to observe processes, and the experimenter did not demand of them the statement of meanings. Our experience shows conclusively that observers who have had a long training in process-report are able, after training, to parallel the processes by meanings.

And the same reply, once more, invalidates Professor Ogden's discovery of imageless thought in the quoted report of our observer F (*op. cit.*, 195). "Red : blue :: green : yellow. I started to say this automatically. Then I repeated the stimulus and said 'intermediate' verbally. Some kind of consciousness that meant 'principal colors.' I did not say 'principal'." Rewritten in terms of our method of parentheses, the last sentences would be: "Some kind of consciousness (that meant principal colors.) I did not say principal". F found a meaning present, the meaning of principal colors; and he found also a corresponding process, about which, however, he could say nothing more than that it was not a kinæsthetic-auditory verbal image.

As to the second member of Professor Ogden's criticism, that Cornell observers have confined their reports to the description of sensations, images, feelings, and like familiar modes, and have failed to find a new process (if we use this term again in our present sense),—we must admit the fact. But Professor Ogden has, nevertheless, confused the deed of this non-discovery with the will. The observers did not, it is true,

report on 'meanings' as well as on 'processes,' in the sense of the present study; for this is, we believe, the first instance of the intentional and systematic assignment of the double task in any laboratory. They did, however, have the *Aufgabe* to report all the processes that were present in their experiences. Thus, Okabe writes: "*No hint was given that certain processes were wanted or expected by the experimenter, and no limit was set to the observer's vocabulary.*" "It seems especially important to note that G finds no trace of imageless contents, since he is precisely of what has been described as the imageless type."¹ Can the critic have overlooked these and similar passages? The aim of Clarke's study of Conscious Attitudes was to "bring these experiences to the test of introspective observation, and thus to discover whether or not they are analysable." "The introspections of any one observer show different stages of clearness and intensity of imagery, which allow us to connect, by a graded series of intermediate steps, a complex of vivid and explicit imagery with a vague and condensed consciousness which we suppose to represent what is called 'imageless thought'.² Has the critic again read a little hastily?—Let us make the rejoinder concrete. Suppose that you are told: "Here is a pile of coins, of various denominations, some of which are American, some English, some French. Sort the coins out, both by country and by denomination. We are informed that there are also German coins in the pile. Keep an eye especially keenly on this possibility." You sort, and you find nothing but American, English and French pieces. And your conclusion is summarily rejected, *on the ground that you have had special training in the identification and discrimination of American, English and French money!*

A final word on Bühler and his thought-elements. "I was fortunate enough," says Bühler, "to find two experienced psychologists who put themselves at my disposal for the experiments. . . In the present paper. . . I shall refer always and only to the observations of Külpe and Dürr. . . The experimenter must feel himself into the position of his observers, must experience with them, if he is properly to understand them; he must be able to go into their peculiarities, and to speak with them in their own language."³ Bühler, then, sought to feel himself into the position of his two observers; and, as regards the one of them, Dürr, the attempt—as Dürr has himself written—was unsuccessful. Bühler's thought-element rests, therefore, upon his interpretation of Külpe's reports. And Professor Ogden now tells us that it occurred

¹This *Journal*, xxi., 1910, 563, 567, 593.

²This *Journal*, xxii., 1911, 215, 248.

³Arch. f. d. ges. Psychol., ix., 1907, 306, 309.

to Külpe, while lecturing on Leibniz, "that the monads were not 'concepts' but *thoughts*;" here, still according to Professor Ogden, is Külpe's first idea "regarding the character of thought as a distinct mental element."¹ But was not Külpe, then,—to borrow a word of Professor Ogden's—*predisposed* to the discovery of the thought-element?

We greatly regret that we have been unable to compare our results, in detail, with those of former workers in the same field. Limits of space forbid; as they forbid, also, a further exploitation, at this time, of our observers' reports.

¹*Op. cit.*, 185.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XVI. THE EFFECT OF AREA ON THE PLEASANTNESS OF COLORS

By DOROTHY CLARK, MARY S. GOODELL, and M. F. WASHBURN

The arrangement of apparatus in the experiments to be described was as follows. Two sets of colored paper squares were provided, one set being 5 cm. a side, the other 25 cm. a side. The small squares were pasted, the large ones fastened with small wire clips, to cardboard squares of the same size as the paper, in order to give them stiffness. The colors used were from the Bradley series and comprised the following: saturated violet, blue, green, yellow, orange, and red, with the lighter tint and the darker shade of each. For every color there were two squares, a larger and a smaller one. In performing an experiment one of the colored squares was suspended by means of wire so that it was seen against a background of the gray laboratory wall about a meter and a half away, and the observer sat at a distance of one and a half meters from the square, which was hung at about the level of her eyes. This arrangement was suggested by Dr. E. Murray as being likely, by rendering the background indefinite, to lessen its influence. The observer at a signal looked at the colored square for ten seconds and recorded her judgment of its pleasantness or unpleasantness in numerical terms, using the numbers 1 to 7 to indicate the following affective grades: very unpleasant, moderately unpleasant, slightly unpleasant, indifferent, slightly pleasant, moderately pleasant, very pleasant. The colors were shown in irregular order, which was, however, kept constant for all observers. A large and a small square of the same color were never shown in immediate succession, as our object was to obtain independent judgments of the affective value of each square, not comparisons of one with another. Nearly all of the observations were taken upon bright days. The observers were twenty-three in number, all women and all but three college students.

The results were treated in two ways. First, the number of observers who assigned a higher affective value to the larger area of each color was counted and compared with the number of those who assigned a lower value to the larger area. This method took no account of the degree of the preference, that is, of how much greater, numerically expressed, the observer's estimate was of the pleasantness of one area as compared with that of the other. The following conclusions were drawn from this method of considering the results. Among *saturated colors*, red was the only one which the majority of the observers preferred in large rather than in small area. All the others were preferred by more observers in the smaller area, though the majority in the cases of yellow and violet was slight. In the case of all of the *tints*, a slight majority preferred the larger area. In the case of all the *shades* the larger area was preferred, though the majority was small for green and violet.

Secondly, the numerical values assigned by all the observers to the large area of a color were added, and divided by the sum of the numerical values assigned by all the observers to the small area of the same color. This proceeding gave the ratio of the total affective values of the two areas of a given color. The following facts resulted from a study of the figures thus

obtained. In *saturated colors*, the smaller area is pleasanter except in the case of saturated red, where the larger area is pleasanter. All the *tints* showed slightly higher affective values for the larger areas. In the case of the *shades* there was a more marked preference for the larger areas except in the case of green.

We may conclude, then, that under the experimental conditions described (1) *saturated colors are preferred in smaller area, with the exception of saturated red, which is preferred in larger area*; (2) *the larger area of tints is slightly preferred*; and (3) *the larger area of shades is preferred, the preference being least in the cases of green and violet*.

There was no correspondence between the absolute affective value of a color and the preference for it in larger or smaller area. It may be noted that in this study as in the preceding ones, the highest absolute affective value was that of the blue tint and the next highest that of saturated red, also that yellow and orange had the lowest affective values among saturated colors, tints, and shades alike. Twelve of the twenty-three observers in this study were also observers in the study on An Effect of Fatigue on Judgments of the Affective Value of Colors.

XVII. FLUCTUATIONS IN THE AFFECTIVE VALUE OF COLORS DURING FIXATION FOR ONE MINUTE.

By DOROTHY CRAWFORD and M. F. WASHBURN

The materials used in this experiment consisted of pieces of the Bradley colored papers, 2.9 cm. square. This size was used in the present study, as in some of our other studies on the affective value of colors, for the reason that it can be conveniently cut from the sample books issued by the Bradley Company. Eighteen colors were used: saturated violet, blue, green, yellow, orange, and red, and the lightest tint and darkest shade of each. Each piece of paper was laid on a white ground before the observer, who was asked to express her judgment as to its pleasantness or unpleasantness by using one of the numbers from one to seven, in the ordinary way. The observer was further asked to look steadily at the color for an interval of one minute, measured by the experimenter, and to report by means of the appropriate numbers any changes in the affective value of the color. At the end of the period of fixation she was asked to give the reasons for the changes which had occurred. The same proceeding was repeated for each of the eighteen colors, in random order. Fourteen observers worked on the problem; all, as usual, women, and nearly all students. Eight of the observers had had practice in introspection. Several of them made the experiment more than once, at considerable intervals, so that the total number of experiments performed was twenty-seven.

For most of the observers some fluctuation did occur during the one minute period: the number of colors with which no fluctuation took place varied from fourteen, out of the eighteen, to none, and averaged between four and five. Our principal interest was in the causes which produced the changes in affective value. These changes may be roughly divided into two groups: *alterations due to changes in the color itself, and alterations due to purely mental causes*.

Under the first head, two obvious factors suggest themselves: adaptation and the presence of a negative after-image, due to shifting of fixation, in the neighborhood of the color. The effects of adaptation were variously described as 'fading,' 'dulling,' 'getting dirty,' 'getting darker.' The most important purely mental cause for change in the pleasantness or unpleasantness of a color lay in the occurrence of associated ideas. These were most frequently of definite things, such as violets or wall-paper; sometimes of

touch experiences, indicated by the words 'velvety,' 'soft,' Other mental causes of change concerned the affective process itself: they were expressed by 'getting used to it,' or 'getting tired of it.' Both of these last comments were surprisingly rare; getting used to the color was mentioned only six times as a cause of increased pleasantness, and getting tired of a color was twenty-two times given as a cause of increased unpleasantness. This is in comparison with one hundred and twenty-seven cases where change was due to the occurrence of an association.

When the number of cases of change due to each of these two principal classes of causes was counted up for each of the eighteen colors, it was found that *changes caused by alteration in the actual appearance of the color were decidedly more numerous in the case of the saturated colors (133) than in that of the shades (93) or tints (70)*. The principal reason for this difference seemed to be the *greater frequency with which an after-image was noticed in the case of the saturated colors (thirty-three times, as compared with three times for the tints and six times for the shades)*. The process of adaptation was about equally influential upon the three classes of colors. Besides adaptation and the negative after-image, our observers occasionally reported other changes in the appearance of the colors, such as alteration in the color-tone, orange getting pinker, green shade growing less yellow, and so on, which could not with certainty be ascribed to adaptation. Once in a while an observer would report that a color grew brighter as it was looked at: it is possible that this was due to renewed fixation after having shifted the eyes.

On the other hand, *changes due to purely mental causes were most frequent in the case of tints (70); shades came next (59), and saturated colors last (55)*. This difference was in large measure due to the fact that *associations occasioned a greater number of changes in affective value in the case of tints (47) than in the case of shades (39) or saturated colors (31)*. Saturated colors occur with less frequency in nature than unsaturated colors, and this fact would naturally make them poorer in associations: why tints should be superior to shades in associative power is not clear. The occurrence of an associated idea, when it produces a change in the pleasantness or unpleasantness of a color, is equivalent, of course, to a change in the source of the affection, just as truly as when the color itself changes under the influence of adaptation. But when an observer reports that she has grown 'used to,' or 'tired of' a color, these terms probably refer to a dulling of the affective process itself, apart from an alteration in its source. We have already noted that, in the comparatively short interval which we used, such changes were rare. This result was in part due, no doubt, to the fact that the conditions of the experiment set the mind of the observer towards finding some affective value, pleasant or unpleasant, in the colors, and growing used to and tired of a color are processes leading to the disappearance of affective value. The six cases of 'getting used' to a color were equally divided among shades, tints, and saturated colors. *The shades gave the most instances of lowered affective value through getting tired of the color (11); the saturated colors came next (8), and the tints last (3)*. Very likely the tendency to get tired of shades is due to their being somewhat depressing. Getting used to and tired of colors may be called processes of affective adaptation. In addition to them, certain changes were reported which seemed to refer to the affective process itself rather than to its cause, but which were too indefinite to be classified; such, for instance, as 'growing depressing,' 'growing insipid.'

A further question which suggests itself is whether the above-mentioned causes of change in the affective value of colors were causes of increased or diminished pleasantness. This question is not a simple one. If we find, for instance, that a given cause such as adaptation produces in a given color more changes in the direction of increased affective value than in that of diminished affective value, we must bear in mind that a color

which started at the beginning of the one-minute interval with the maximum affective value, 7, would have no chance of increasing, while one which started at 1 would have no chance of decreasing. On the other hand, by far the greatest number of changes in affective value that occurred under the conditions of this experiment were changes of one place only in the scale. Therefore if the initial value of a color were anything but 7 or 1, the chances were about equal for a rise or a fall. It ought to be sufficient, then, to correct our comparison of the number of rises in affective value produced by a given cause for a given color with the number of falls, by taking account merely of the number of maximum and minimum judgments of initial affective value made for that color.

Adaptation and association were the only causes of change that occurred with sufficient frequency to make this calculation worth while. Its results were as follows:

Saturated violet had 7 for its initial value once, and 1 not at all. It had therefore a very slightly greater chance for decrease than for increase. There were six cases where associations produced an increase and two cases where they produced a decrease. Associations then, on the whole, exerted a favorable influence. There were seven cases where adaptation produced increase and three where it produced decrease: the influence of adaptation, then, is also favorable to this color.

Saturated blue had 7 once for its initial value and 1 three times. It had therefore more chance to increase than to diminish. Associations produced three increases and no decreases; hence they had no demonstrable influence. Adaptation on the other hand produced sixteen increases to one decrease, and undoubtedly had a favorable influence on the pleasantness of this color.

Saturated green as regards initial values was exactly like saturated blue. Associations had almost no influence upon it, occurring only twice as a cause of increase and not at all as a cause of decrease. Adaptation was favorable, with nine cases of increase to two of decrease, but its effect was not very marked.

Saturated yellow never had 7 for its initial value, while it had 1 three times; its chance for increase was therefore greater than that of blue and green. The effect of association was on the whole favorable; there were six increases and no decreases. The effect of adaptation was rather unfavorable: four increases to an equal number of decreases.

Saturated orange never had 7 for initial value, and had 1 five times. It had therefore decidedly more chance for increase than for diminution. Associations had little effect, the proportion of increase to decrease being five to two. Adaptation was also of small influence, the proportion being seven to four.

Saturated red never had 1 for an initial value, and had 7 seven times. Thus it had much more chance to diminish than to rise in affective value. On the whole the influence of association on this color must be considered favorable, for there were two increases and only three decreases, despite the greater likelihood of the latter. Adaptation on the other hand did saturated red no good; there were ten cases where it brought about a fall, and four where it brought about a rise.

Violet shade, having 7 and 1 for initial values once each, had balanced chances. The influence of association was then wholly favorable, the ratio of rises to falls being five to nothing. Adaptation was nearly as often unfavorable as favorable (six to eight).

Blue shade had decidedly more chance for fall than rise, 7 occurring ten times as initial value and 1 once. The influence of associations was negligible, as they produced change in two instances only. The effect of adaptation must be considered favorable, as there was an equal number, five each, of rises and falls assigned to it.

Green shade had equal chances for rise and fall, as neither of the extreme numbers was ever assigned to it at the outset. Associations were dis-

tinctly favorable to it, the ratio of rise to fall being six to one. Adaptation on the other hand had an unfavorable effect nearly as often as a favorable one (five to six).

Yellow shade had greatly more chance for increase than for decrease, 1 occurring twelve times as initial value and 7 not at all. The influence of associations was then but slightly favorable, the ratio of rise to fall being five to one. Adaptation seemed to have a somewhat unfavorable effect, causing four decreases to seven increases.

Orange shade had equal chances for increase and decrease, neither extreme occurring as its initial value. Associations were distinctly favorable to it, in the proportion of ten to three. Adaptation was a little more favorable than unfavorable (seven to five).

Red shade had more chance of decrease than of increase, as 7 was assigned to it four times at the outset and 1 never. The influence of association must then be reckoned as decidedly favorable, since it produced six rises and no falls. Adaptation on the other hand was perhaps a little more unfavorable than favorable in its influence, producing eight falls to four rises.

Violet tint had 7 for its initial value nine times and 1 not at all; it was therefore much more likely to fall than to rise. Association thus must have had a distinctly favorable influence to produce six increases and no decrease. Adaptation on the other hand had little if any effect, the proportion of falls to rises being two to ten.

Blue tint had somewhat more chance of decrease than increase, 7 occurring twice and 1 not at all as its initial value. The effect of associations was favorable, though not so markedly as with violet tint, the ratio of rise to fall being seven to two. Adaptation was unfavorable, causing nine decreases to four increases. *Green tint* had 7 six times and 1 once for initial value, and so was more likely to fall than to rise. Associations were favorable, producing five rises to one fall. Adaptation was distinctly unfavorable, causing thirteen falls to two rises.

Yellow tint had equal chances, 7 occurring twice and 1 twice. Association produced less effect upon this tint than upon any of the other tints, but such effect as existed was mostly favorable (five to one). Adaptation was equally unfavorable, producing one rise and five falls.

Orange tint had more chance of fall than of rise, 7 occurring five times and 1 once. The effect of association was on the whole favorable, producing eight rises and five falls. Adaptation had little effect, but that little was probably favorable, there being three increases to four decreases.

Red tint had nearly equal chances, leaning slightly towards fall, with two cases of 7 and one of 1. Associations were favorable to it, in the proportion of seven to one, but the effects of adaptation were balanced, eight to seven.

These results may be summarized in the following statements. *For saturated colors, associations have little influence, but what they have is predominantly favorable. Adaptation is favorable to violet, blue, and green, rather unfavorable to yellow and red, and without definite effect on the affective value of orange.* The colors of the warm end of the spectrum our observers seem to have liked quite as well in their original saturation as in the duller tones produced by adaptation. *In the case of shades, association produced a favorable effect upon violet, green, orange, and red; little effect of any kind upon blue, and nearly as much unfavorable as favorable effect upon yellow. The effects of adaptation were on the whole as often unfavorable as favorable to the shades. Associations are favorable to tints without exception, and adaptation was on the whole unfavorable. Broadly speaking, the tendency of associated ideas is to raise the pleasantness of a color, and the tendency of adaptation is to lower it rather than raise it.*

With two exceptions, in every case where an after-image was noticed, it diminished the pleasantness of the color.

IMITATION IN RACCOONS

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This paper is, in certain respects, supplementary to work which has already been published by Cole,¹ and by the present writer.² The same four raccoons which served as subjects in those earlier investigations were employed in the present study. At the time when these later experiments were undertaken, the animals were seven months old. They had all been trained, for a period of nearly three months, in 'puzzle-box' and other tests; but, with the exception of one individual, they had had no experience with experiments similar to those which were here undertaken.

For the purposes of comparative psychology, three sorts of imitation may be distinguished. *Instinctive* imitation is illustrated in the reaction of the chick which pecks at an object on seeing another chick do so. *Gregarious* imitation is exemplified by the stampede of the herd when one of its number becomes alarmed and flees. When a monkey sees one of its fellows obtain food by pressing a lever and releasing a door, and himself proceeds to an intelligent performance of the same act, we have a case of *inferential* imitation. The present study is concerned with an investigation of this higher, or inferential type of imitation.

Our apparatus consisted of an inclined plane of poultry netting, 1.5 m. long by 25 cm. wide; it was supported at one end upon a box, in such fashion that it extended in a slightly upward and diagonal direction across a corner of the room to a platform, which was 30 cm. wide and 50 cm. long. The platform was 90 cm. high; and the other end of the inclined plane was 65 cm. above the floor. At the lower end of the plane, stood a box-step, 32 cm. high, by means of which the raccoons could easily climb upon the plane.

At a given signal, the raccoon went up the step and along the plane to the platform, where he was fed. The experimenter stood at a distance of about a meter from the plane. In earlier experiments, one of the raccoons (Jim) had learned to go up to the platform to be fed. He now served as imitatee, while the other three animals were employed as possible imitators of his acts. The procedure consisted in releasing the imitator in the room where he was able to make several observations of Jim's performance of the act of climbing and obtaining food. Immediately afterwards, the imitator was given an opportunity to perform the act alone. We kept a record of the number of times that he clearly saw the act performed, of the number of times he probably saw it, of any apparent tendency to imitate, and of all other significant facts.

TOM. First day. Tom was present in the room while Jim went through twenty-one repetitions of the act of going up the step, and along the plane to the platform and receiving food. Tom apparently³ saw seven of the twenty-one repetitions of the act; and he probably saw the act in five other repetitions. Then Tom was placed in the room alone, in order that

¹L. W. Cole: Concerning the Intelligence of Raccoons, *Jour. Comp. Neur. and Psychol.*, XVII, 1907, 211-261.

²W. T. Shepherd: The Discrimination of Articulate Sounds by Raccoons, *Amer. Jour. Psychol.*, XXII, 1911, 116-119.

³This qualification is necessary because it is difficult to be entirely sure that one animal has really seen the action of another.

he might imitate Jim's reaction under similar conditions. He failed to accomplish it during a period which lasted one minute and twenty seconds *Second day*. (Six days after the first trial.) Tom saw Jim perform the act three times; and he probably saw the reaction five times in all. When tested alone for a period of ten minutes, he failed to repeat the act,—indeed he showed no indication of any tendency to imitate Jim's reaction. *Third day*. (One day after the preceding trial.) Jim's reaction was seen four times, and probably was seen nine times, in all. When tested alone, Tom gave no indication of any tendency to imitate, and had not accomplished the act at the end of ten minutes. In three of Jim's repetitions, however, after the imitator had performed the act and was eating his food, or had just eaten it on the platform, Tom went up also and sniffed about on the platform.

DOLLY. First day. Dolly was in the room while Jim went through the act fifteen times. She saw four of his reactions, and probably saw eight more. When tested alone, she showed no tendency to imitate, and had not accomplished the act at the end of ten minutes. *Second day.* (Seven days later.) She saw Jim's reaction eleven times, and probably saw it three times more. During his tenth reaction, she went up the plane and to the platform, where she sniffed about. During the subsequent test of her imitation, she climbed upon the experimenter for food, and rambled casually about the room. She then went up the step to the plane, and down again, and again wandered about the room.

JACK. First day. Jack saw six of Jim's twenty-one reactions, and probably saw seven more. During Jim's eighth reaction, Jack went up the step, and crossed the plane to the platform, where, if one may judge from his actions, he seemed to expect to be fed. But when tested alone immediately afterwards he wholly failed. In no way did he indicate any tendency to imitate Jim's reaction. *Second day.* (Five days later.) Jack saw seven of Jim's reactions, and probably saw four others. During the progress of Jim's fifteenth reaction, Jack went up the step and partially up the plane to a coat which hung upon the wall near-by. Jim had already gone to the platform. Jack did not appear, however, to expect food. In the first trial where Jack was tested alone, after the usual signal had been given, he played about the room, went to the window and to various boxes in the room; he finally ascended the step and the plane to the platform and was fed there. His time for this trial was two minutes and fifteen seconds. When the signal was given for the second trial, he went up on a box at the other side of the room, and looked at the experimenter for food,—as it appeared. Finally, he went to the platform and was fed. In the third trial, his behavior was similar to that in the second trial. He went up to the platform in twelve and a half seconds. In the fourth trial, he did the act in twenty seconds, first going part way up the plane and looking toward the experimenter. In the fifth to the eighth trials, his behavior was similar to that during the fourth trial; his times for the accomplishment of the act during these trials were thirty-seven, twenty-five, thirty-two, and thirteen seconds respectively. In the ninth trial he reacted correctly in eleven seconds; and in the tenth, in thirteen seconds. In both of these latter trials he made but a brief stop during the act of ascending the plane.

The results of these experiments may be regarded as wholly negative. When tested alone, after seeing Jim's reactions, neither Tom nor Dolly made any attempt to imitate the act which they had just seen. It is true that they went up the step or up the plane during the process of Jim's reaction; and Tom's behaviour on the third day,—when on three occasions he went up on the platform where Jim was eating,—seems to be imitative. But if the raccoons perceived the results of Jim's reactions, it is difficult to understand why they did not themselves react to that intelligent perception of the results of Jim's actions, when tested for a period of ten minutes immediately afterwards. It seems probable that some mental process

of no higher order than 'instinctive' imitation is sufficient to account for these reactions.

Jack's reactions appear to be somewhat more doubtful. But when we consider all of the evidence, and especially when we note his hesitating behavior on the second day—where he apparently had formed, or almost formed, the appropriate associations,—it would appear that we may attribute his learning to the humbler procedure of 'trial and error,' and not to an 'inferential' imitation of Jim's reactions.

We conclude, therefore, that these brief experiments have failed to show that 'inferential' imitation (involving ideation) is a part of the mental equipment of the raccoon. And it may be recalled that another investigation of imitation, in which we employed the same animal,¹ yielded wholly negative results. Davis's² interesting observations of the raccoon likewise failed to reveal the presence of the higher form of imitation.

¹L. W. Cole: *Op. cit.* pp. 232-235.

²H. B. Davis: 'The Raccoon: A Study in Animal Intelligence.' *Amer. Jour. Psychol.*, XVIII, 1908, 447-489.

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BOOK REVIEWS

Introduction to philosophy, by W. JERUSALEM, translated by C. F. Sanders. New York, The Macmillan Company, 1910. pp. VIII, 319.

An excellent compendium of the philosophical schools and theories, objective and impartial, yet with a clear platform of its own, is here made more accessible to American students. For completeness in historical orientation of the theories, and in exposition of their recent developments, we hardly know a better book. Its standpoint is empirical, practical, social, and devoted to common-sense. Judged as a text for beginners in this country, it seems to presuppose a more thorough general preparation and more mature habits of thought than one ordinarily finds in the American student; its greatest utility here would perhaps lie in its compendious character. For graduate students wishing a summary view it should be invaluable. Every problem is historically grounded, and the bibliographies are carefully prepared and well-balanced.

Jerusalem's philosophy "is characterized by the empirical view-point, the genetic method, and the biological and social method of interpreting the human mind" (p. vi). It is "rather close to pragmatism in epistemology," but gives "a further development of the pragmatic concept of truth" (p. vi). The following are typical phrases: "the airy realm of the transcendental," "come down to the level of reality," "understand life itself," "define its ideal and destiny" (p. vii). Philosophy is defined as "world theory," which is "obliged to keep in close touch with science," and "to construct the fragments, beyond which scientific investigation can never attain, into consistent, articulated system" (p. 2). Emotional and practical motives also play a part. Philosophy "should teach us to regard the world and life from nobler view-points" (p. 3). In its unity and in its study of the methods by which unity is gained, lies the distinction between philosophy and science (p. 14). Thus "the investigation of the foundations of knowledge" is philosophy's "most important task" (p. 15). But its field is very broad. It includes psychology and logic as *Propaedeutik*, epistemology, metaphysics, æsthetics, and ethics (including sociology),—all of which are to be studied historically as well as systematically.

The second division of the book, which deals with psychology and logic, limits itself mainly to the defining of the subject-matter of these two sciences. Psychology studies processes, not states; knows nothing of substance (soul); and is independent of metaphysics (p. 26), although contributory to the problem of knowledge, and to other problems (p. 40). The sections on logic, comprising the theory of judgment, are rather advanced reading, but they constitute an admirable summary.

The third division, "Criticism of Knowledge and Epistemology," traces the theories historically from naïve realism through Kantianism and idealism to the author's view, critical realism. Idealism, he finds, fails to account for social agreement; and since a universal consciousness is "psychologically practically inconceivable" (p. 82), critical realism alone remains. The discussion of epistemology includes sensualism, intellectualism (rationalism), mysticism and pragmatism. The author identifies modern mysticism (wrongly, in the opinion of the reviewer) with spiritism; and in his criticism of pragmatism, he seems to overlook its chief difficulty, *i. e.*, its failure to account for the need of knowledge for its own sake, apart from further consequences. The governing category of knowledge he

finds genetically to be "fundamental apperception," which seems to be Kant's "transcendental unity" with a psychological body (p. 108). This, as well as the more particular categories, is evolved by natural selection, in accordance with the pragmatic principle of useful adaptation to environment (p. 111). Abstract reasoning is the best substitute we can find, in the absence of concrete verifiability; this furnishes the origin of the apparent independence of logic (pp. 118-120). There is really no *a priori* knowledge (p. 123). On the whole, the discussion, although difficult for a beginner, is a masterpiece of logical arrangement and clearness.

The fourth division, "Metaphysics or Ontology," first discusses the ontological problem. Monism is either materialism, spiritualism (panpsychism), monism of being (Haeckel) or of becoming (Mach, Avenarius). The author doubts the conservation of energy in psychical process, and inclines to accept Wundt's "creative synthesis" (p. 147). Panpsychism is condemned (unfairly, we believe) as not accounting for the physical. The author is a dualist and an interactionist; he regards will as the type of causation (p. 181). Pluralism is less completely discussed than other topics, inasmuch as radical empiricism does not seem to be understood. Its attempt to defend plurality from the point of view of immediacy is not mentioned (p. 184). In his discussion of the cosmological problem the author follows Paulsen, in the main.

The fifth division treats of *Æsthetics*; and the sixth, of *Ethics and Sociology*. In the opinion of the author, indeed, ethics is sociology. Its subject-matter is not "deportment," but "volition" (p. 241), "the evaluation of an act in its social significance" (p. 265). Strangely enough, he brings the problem of freedom under ethics, rather than under metaphysics. He upholds *psychological* freedom, or the "absence of the feeling of external or internal constraint;" but denies metaphysical freedom, or the view that acts are "outside the law of causality" (p. 256). As to sociology, so much does he value it that he says "the sociology of the future . . . might well become the foundation of all philosophy" (p. 285).

From the "Concluding Reflections" many quotations might be cited to confirm our general estimate of the author's position. "Philosophy must return to the theory . . . of sound common-sense" (p. 293). "The ultimate object of knowledge is, after all, the preservation and improvement of life" (p. 300). Intellectualistic idealism is "an hypertrophy of the cognitive impulse" (p. 300). The universe is a vast will (pp. 306-307); and "the investigation of the laws of this divine will furnishes the sublime problem of all science" (p. 307). Is not this panpsychism? Or at least, is it not just a little above common-sense?

But, all criticisms apart, the book is a remarkable, and on the whole, a very just summary of philosophy. One finds it impossible, in a short review, to do justice to its historical perspective, and its logical arrangement of the problems. May it meet with the hearty welcome which it deserves.

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The Process of Abstraction: An Experimental Study, by THOMAS VERNER MOORE. University of California Publications in Psychology, 1, 2, 1910, pp. 73-197.

Following in the wake of the Würzburg school which has tried, during the past ten years, to study experimentally the higher thought processes, Moore attempts to determine experimentally the mental processes involved in the process of abstraction, or the formation of our general ideas. He seeks to discover how general ideas form and develop, and what mental processes are involved in their formation.

His method consisted in presenting to his subjects, a series of groups of geometrical figures so drawn and arranged that a common element constantly recurred in each group, while the other figures in the group were con-

stantly varied. As soon as the subject felt sure that the same figure had occurred more than once, he stopped the exposure-apparatus; he then described his state of mind during the experiment stating, especially, what it was that he first noticed in isolating and perceiving the common element.

It was found that the groups of figures thus exposed constituted something of a unit which underwent rather definite changes as the common element became isolated and perceived. That is to say, the elements of these groups had a different mental value after the common element had been perceived from what they had before. This made it possible to study the process of abstraction genetically. Four stages or steps in the process of abstraction were ascertained: (1) The breaking up of the groups of figures and the selection of the common element. (2) The process of perceiving or apprehending this common element. (3) Holding this common element in immediate memory, until (4) it was recognized as having occurred before. Each of these stages was made the subject of special observation, with a view to determining the mental processes involved.

On the first point, the breaking up of the group, and the isolation of the common element, nothing new was determined. Moore simply says that the selection of the common element depended upon the degree to which the repeated figure attracted the observer's attention. This step seems not to have been worked out in detail. No psychological history of the mental process actually employed in selecting the common element was obtained; but it was determined that, when the group was finally broken up, the common element always became accentuated at the expense of the surrounding elements, which seemed to be positively cast aside and swept more or less completely from the field of consciousness.

The second step, the perception the common element, was initiated by this breaking up of the group. The sensations aroused by the recurrent figure were attended to. This, at once, instituted a process of apperception or mental assimilation, by means of which the sensations were related or joined to one or more appropriate categories. A general idea that some kind of a figure (roundish, open, pointed, etc.) had been repeated, was the result; but no definite information about the shape or nature of this figure could be given. The figure was clearly apprehended but not in representative terms. "Mental images formed no essential part in this first apprehension of the figure." There was not even a more or less specialized general concept of the form of the figure perceived. After the knowledge that some kind of figure had been repeated, the memory of this fact usually lingered in consciousness until a clearer idea of the figure was formed. But this second idea of the common element might still be expressed in perfectly general and non-representative terms. It was only rarely represented in consciousness in imaginal terms, or accompanied by feelings of pleasantness or unpleasantness, strain, and the like. The third step in the perception of the common element was the acquisition of a correct idea of the figure and a clear knowledge of its shape; this stage was attended by doubt or error as to the orientation of the figure in the group. The fourth and last step in the perception of the common element involved forming a *correct* idea of the figure and its shape, with a true knowledge of its orientation in the group. It is, therefore, clear that the perception of the common element in abstraction proceeded from that which was general and vague and imageless to that which was particular and definite and clear. Mental images belonged only to the later and more unessential stages of the perception of the common figure.

After the common element had been isolated and perceived, it had to be held in immediate memory until it could be certainly recognized as having been seen before. This memorial process was investigated, and three factors were found to affect the memorial permanency of the common element: (1) The method of memorial visualization, motorization, association, analysis, etc. (2) The appearance and noting of impressions

between the time when the common element was first noticed, and its final perception and recognition. (3) The focality of the common element, when perceived. The most economic method of memorizing the common element, and the method most often used, was that of association and analysis. Analyzing the vague idea of the figure "to see what it was made up of, what it resembled, its possible use," etc., was found to be the most effective method of fixing it. This mental analysis, while never put into actual words or representative terms, was found to have a greater effect for memorial permanency than the combined effects of any visual and motor imagery employed by the subjects. "Subjects often remarked that figures were remembered in this way when they were attempting to memorize them by visualization." But the method of memorizing is not the only element that influenced the memorial permanency of the common element. Succeeding impressions had a positive tendency to impair the subject's memory for the common element. In every case, the perception of new figures tended to obliterate from memory the figure already perceived. Then, too, the farther the figure was from the focal point of vision when perceived, the less accurately could it be held and reproduced.

The last stage in the process of abstraction,—the *recognition* of the common element, or the knowledge that the figure had been seen before,—was an entirely different mental process from the selection, perception or retention of this common element. It often occurred that the figure which the subjects had in mind for some time was later recognized as the common element. "Certainty that the figure apprehended had been seen before was what was dawning upon the subject during the interval when his mind was thus being fully made up." In the development of this recognitive certainty there was (1) an intimation or feeling of weak probability that a figure had been seen before; (2) a stage of actual probability that a common element was present; (3) a final stage of certainty. While the process of recognizing was thus distinct from the process of perceiving the common element, it must not be inferred that the process of perception was regularly completed *before* the process of recognition began. "What actually happened was that almost any degree or certainty of recognition might co-exist with any degree of the perfection of perception." There might be (1) an intimation of a common element, without any knowledge of its form; (2) probability that a common element was present, but an imperfect idea of its form; (3) probability that a common element was present, and a true idea of its form; (4) certainty that a common element was present, but an imperfect idea of its form; (5) certainty that a figure was repeated without any knowledge of its form. These results were obtained by stopping the apparatus, during some of the experiments before absolute certainty had developed. In this way, cross-section analyses of the recognitive consciousness were obtained which showed the process at all stages of its development. The subjects were also shown discs without a common element, and discs containing more than one common element, to insure accurate observation. Certain recognition, is, therefore, not dependent upon perfect perception; neither is it dependent upon a comparison of mental images. It often took place without the formation of any mental image of the thing that was recognized. "A person might be certain that a figure was repeated, and have a perfect image of the figure, or an imperfect image, or no image at all." "A comparison of mental images is not the normal method of recognition."

In this process of recognition, an element of certainty or uncertainty was always involved. This implied assent or doubt, and, consequently, an actual or suspended judgment. The final question, therefore, is to determine the psychological basis of this recognitive judgment. How was this judgment or feeling of certainty arrived at in the experiment? Moore's answer to this question is theoretical. The actual development of this feeling of certainty was not determined; but "recognition took place not only when there was no revived mental image of the past perception, but

when the present perception itself was too imperfect to leave any trace of mental imagery on the mind" (p. 173).

The real basis of this recognitive certainty, "was the series of associated concepts or appropriate mental categories which the sensations of the common element aroused." "The subject's first idea of the common element was made up of the sensations from the repeating figure plus the concepts or mental categories which these recalled. These two processes fused and formed a new psychological product,—the subject's first apprehension or idea of the common figure. When the common element was seen again, a new percept was formed and assimilated to the old." Just how this occurred Moore does not say. "The old series of associated concepts readily falls in with the new, and gives rise to the feeling of familiarity and certainty." How or why, Moore does not determine. "The new concepts thus formed readily fit in with the old. There is nothing to jar the process of their assimilation, but often a re-enforcement of at least some members of the associated train of concepts." The figure's series of associated concepts, therefore, not only formed the chief factor in perception, and the factor by means of which the subjects recalled the figures, but also the factor that enabled them to recognize the figure as having occurred before (p. 175). These mental categories or concepts were also the final product of the whole process of abstraction.

Two things were determined about this final product of abstraction or learning: (1) That the mental categories and concepts formed in the process of this experiment were the result of the subject's experience with the repeating figures of the groups. All other categories and concepts possessed by the individual are products of the individual's past experience in the process of learning. (2) These mental categories or concepts represent compound psychic processes which are separate and distinct from mental images and feelings,—a result supported by the author's careful summary of related studies in the psychology of thought with which his study begins.

The character and nature of these concepts was not determined or described; and the reader seeks in vain for a more detailed psychological history of the formation and development of the particular concepts formed in the course of the experiment. One is curious to know the nature and origin of the imageless mental contents arrived at; and how these actually operated in the process of forming other mental categories and concepts. The reviewer feels that a more detailed psychological history of the formation and development of the particular concepts formed in the course of the experiment would have told us much about the nature and origin of these imageless processes. The author should have told us exactly how the common element was perceived; he should have determined, by controlled and repeated observations, exactly how the feeling of recognitive certainty was acquired, how the common element was actually selected, etc.,—a very difficult but not impossible task which must be squarely met if we are to obtain the psychological facts. What, to the reviewer, seems to be needed most in all such studies as this is a complete psychological history of the processes studied. We should be supplied with enough accurate, cross-section analyses of the mental processes involved to enable us to trace, with assurance, the whole course of their development. This is a method and point of view, which is not clearly apprehended in the present study. In fact, the reviewer feels that the author is at times describing logical deductions instead of psychological facts; that he is trying to tell how he thought certain processes worked instead of giving us actual verified observations from his subjects. Nevertheless, Moore has done a careful and important piece of work. His study is, without doubt, the best in the field, for it not only makes an important contribution to the ever-growing psychology of the higher thought processes, but it also raises an array of definite problems for future work, some of which I have tried to indicate in this review.

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Technique de psychologie expérimentale de Toulouse, Vaschide et Piéron.
Par E. TOULOUSE et H. PIÉRON. Paris, O. Doin et Fils, 1911. Two
vols. pp. 303, 288.

This is the second edition, carefully revised and largely extended, of a one-volume work published by Toulouse, Vaschide and Piéron in 1904. In its original form, the *Technique* was practically a manual of mental tests, the sublimated result of ten years of teaching and investigation; it grew out of Toulouse's study of Zola (1896), where the need for precise methods was keenly felt. The authors accordingly made a clean sweep of experimental tradition, and started out to devise, on their own behalf, rules for the *examen des sujets* (see this JOURNAL, xvi., 1905, 139). The new two-volume work has a wider scope; "it allows a very large place to the methods customarily employed in the various laboratories of France, Germany, England and the United States of America;" it devotes a chapter to the doctrine of averages and the formulæ of correlation; it pays special attention to the 'higher processes;' it describes new experiments (especially in the domain of visual perception) and new apparatus; finally, it omits the theoretical framework of the earlier exposition, and so gains space for maxims of actual laboratory practice.

Nevertheless, the reader who turns to the book with the expectation of finding in it a monographic review of the methods of experimental psychology will be sadly disappointed. There is not a single reference that extends beyond the bare name of a writer, and the bibliographical index contains only four works, the manuals of Judé, Myers, Sanford and Titchener. There is no discussion of method proper: the methods of experimental psychology "are implied in our technique, but the man of science uses methods, and it is the philosopher who reflects upon them after the event and appraises or judges them; so that this is not our business." Unfortunately, the methods implied in the technique are of an empirical and proximate kind; the trail of the mental test is still apparent. However, the value of the book, at any rate to the American psychologist, is found precisely in its limitations. It has the qualities of its defects: it shows us what a course in experimental psychology becomes when the primary interest of the instructors is in individual psychology, and when the practical application of laboratory results is a constant motive in the shaping of the experiments.

As a paradigm of the writers' treatment we may take the first section of the work, the seven and a half pages allotted to Sensations of Pressure. The student is warned, at the outset, that the experiment is concerned, not with the limen of duality, but with the single sensation of contact; nothing is said of the nature of the sensation. A brief account is given (with figure) of von Frey's hair æsthesiometer. Then follows a longer account (with figure) of the haphæsthesiometric needle-points of Toulouse and Vaschide; von Frey's formula for tension values is stated in a foot-note. Beaunis' æsthesiometric needle-point is figured and described, at the expense of a page and a half, although it is difficult of manipulation, too heavy for the lower limens, and 'has hardly ever been employed.' So we come to the paragraphs headed *Technique*. A circle of at most 2 mm. in diameter is marked with a rubber stamp on the front surface of the observer's wrist, where there is no hair. The observer's eyes are blind-folded, and his hand placed in a fixed position on a felt-covered table. He is instructed that he will be touched from time to time on the back (!) of the wrist, and that he is to say Yes whenever he feels the pressure. Rate, order of touches and blank experiments, ready-signal, form of enquiry for the experimenter, are all prescribed. The series is ascending, from light to heavy; when the observer says Yes, the point evoking this response is to be used in a special series, consisting about one-half of actual touches and one-half of blanks; if the reply is always correct, the next lighter point is taken. Thus one obtains either the limen of certainty or a lower limen of

arbitrary value (touch sensed in three-quarters or less of the cases). Pauses must be made, in order to the avoidance of fatigue. Preliminary experiments are required, that the observer may understand the problem. The point must be applied slowly and in a strictly vertical position; it must remain on the skin for a constant period of time. All points must be kept, during the experiment, in a dry-chamber of 38° temperature.—Here is no lack of details; and yet the account of the experiment leaves much to be desired. At what place within the circle of 2 mm. diameter is the point of $\frac{1}{10}$ mm. diameter to be set down; always at the same spot or indifferently at any place within the circle? How is the hand of the observer to be 'immobilised' upon the felt-covered table? Even if the method employed—an ascending series, interrupted by blank experiments—does not call for explanation and justification, why is not a descending series taken, and the two results averaged (this procedure has been recommended in the Introduction, p. 23)? What meaning will the student attach to the technical term *seuil*, when its value may be correspond to 100% of right cases, to 75%, to 'a lesser number,' or (p. 20) to four successive right answers? Are the limens obtained by the various members of the class to be averaged, whether or not they chance to be derived from pressure-spots? These and similar questions must be answered, if the experiment is to be taken in psychological earnest.

Finally, what of the instrument used? The hair æsthesiometer is rejected, partly on account of the hygroscopic properties of the hair, partly on account of the variation (presumably as the result of bending) of its stimulus-value. We have, instead, the haphiæsthesiometric needle-points of tempered steel. We are warned, however, that the manipulation of these points is by no means easy. The holder is small, oftentimes very small; too sudden an application brings the head of the needle against the roof-plate of the holder; an application in any but an exactly vertical position means friction of the needle in the guide, so that the experimenter must rest his elbows solidly on the table, fixate the head of the needle, and follow its course from various points of view; the points must be kept in the dry-chamber, at a certain temperature, or a temperature sensation will precede or accompany the sensation of contact; and the heavier points (this caution would hardly have been given had not experience proved it necessary) are liable to rust. The reviewer has had no experience with the haphiæsthesiometer, but on general principles he must believe that the use of the points calls for the constant supervision of the instructor, and in particular that the error arising from friction is serious. If the authors are absolutely determined against the introduction into the laboratory of 'capillary organic matter,' and if the experiment requires stimulus-points of minimal diameter, it would seem better to have recourse to Thunberg's standardized glass 'hairs.'

All these criticisms carry a single moral: that the experiment of experimental psychology is one thing, and the mental test another. In psychophysics the limen must be rigorously defined; for test purposes, an arbitrary limen may be set up, to meet the special conditions of the tests. In the psychological laboratory, the choice of method and apparatus is determined by scientific reasons and by these alone; in the case of mental tests, it is influenced by other considerations,—simplicity, ease of manipulation, portability, quickness of application and of calculation. It seems that in this first experiment, on pressure, the authors of the work under review have fallen between the two stools: their method is unduly rough, and their instruments are too delicate for any but the most skilled and careful use.

The reviewer, however, wishes to bring out the qualities of the two volumes; and this end can, perhaps, best be accomplished by way of a complete synopsis of their contents. Pt. I. discusses the measurement of the elementary phenomena of sense. Ch. i., on the measurement of cutaneous

sensations, opens with (1) the experiment on sensations of pressure which has just been described. Sensations of temperature (2) are measured by means of a thermoæsthesiometer, which deposits on the skin drops of warmed or cooled water. Kiesow's cone is figured. Sensations of pain (3) are obtained by the compression, in a pincers-like algoæsthesiometer, of a fold of the skin; the direct-pressure instruments of Macdonald and Chéron are figured. Sections follow on (4) the electrical sensations (1) produced by faradisation of the skin, and (5) miscellaneous cutaneous sensations,—caustic sensations, due to the application of caustic potash in various strengths of solution; sensations of traction; pilary sensations; sensations of tickling. Finally, directions are given for the determination (6) of the duration of cutaneous sensations (method of intermittent stimuli). Ch. ii., on the measurement of the subcutaneous sensations, is devoted to (1) sensations of vibration, evoked by the tuning fork, and (2) kinæsthetic or, as the authors prefer to call them, kinesic sensations. The latter are of three kinds: sensations of muscular effort (myoæsthesiometer: a set of holders and weights), static sensations (schesiæsthesiometer: an adjustable support for the hand, whereby positions of the arm may be varied and reinstated), and dynamic sensations of passive and active movement (schesiæsthesiometer, moving car; boards with grooved patterns, moving car). Ch. iii. brings us to the measurement of sensations of taste and smell. The instruments recommended are the guesiæsthesiometer (geusiæsthesiometer?), a set of flasks containing standard solutions, with droppers inserted in the corks, and the osmiæsthesiometer, a set of wide-mouthed bottles containing 34 aqueous solutions of camphor, of known degrees of concentration, 10 typical scents (for recognition), and 5 strengths each of liquid ammonia and of aqueous sulphuric ether (for testing the tactual sensitivity of the mucosa). Ch. iv. deals with the measurement of visual sensations. Two experiments fall under the heading (1) sensations of light. The *minimum perceptibile* is determined by an instrument constructed on the principle of Charpentier's photometer; the source of light is controlled by the Blondel-Broca photometer. Bouguer's and Blondel's diaphragms are figured. The differential limen is determined either by a differential photoæsthesiometer built on the same principle (rays from a single source are directed by two total-reflection prisms upon diffusing screens, which are viewed through tubes containing a diaphragm) or by rotating discs. (2) Sensations of color have four experiments. For the absolute limen of color, and for the differential limen of chroma ('color intensity'), the authors recommend a chromatoæsthesiometer, made up of colored solutions in rectangular glass vessels; directions are given for the preparation of the solutions. The extreme limits of color sensitivity are fixed by reference to a spectrum. The differential limen of hue is spectrometrically determined. The equation of tint, in the determination of the differential limen of chroma,—a precaution neglected in the initial experiment,—may be effected, though only at the cost of much labor, by suitable combinations of the colored solutions; it is better, therefore, to have recourse to rotating discs; these then permit also of the determination of the absolute color limen, under the same conditions; they permit, further, by the addition of increments of black or white, of the determination of the differential limen of *nuance* (*nuances claires* and *nuances foncées*, that is, changes of tint involving at the same time changes of chroma. Nothing is said of the equation of tint and chroma as a preliminary to the finding of the differential limen of hue.) (3) Visual acuity is measured by a set of optotypes, resembling those of Sulzer, (4) and the extent of the field of vision by Polack's perimeter. (5) The duration of visual sensations (method of intermittence) may be ascertained either by rotating discs (Pierre Janet's arrangement, with regulator and counter) or by Michotte's tachistoscope. Ch. v. deals with the measurement of auditory sensations. (1) The acousiæsthesiometer (drops of water fall upon a sloping plate of

aluminium which gives the a of 217.5 vs.: apparently, the authors count in complete vibrations) takes the place of the more familiar acoumeters. (2) The lower limit of pitch is found by the siren; the upper limit by Koenig's cylinders or the Galton whistle; the differential limen by forks of variable pitch. (3) The temporal limen of tone is also found by the siren; the duration of tonal sensation by means of Sanford's pendulum and tuning fork. Ch. vi. describes the measurement of the labyrinthine sensations. (1) Sensations of rotation require a turn-table, (2) sensations of translation a car running upon rails.

Pt. II. discusses the measurement of the complex phenomena of sense. Ch. i., on the measurement of the perceptions connected with cutaneous sensations, opens with experiments on (1) the cutaneous space-sense. Here we have four main experiments, the first of which, on the differentiation of contacts, has three subdivisions: the distinction of simultaneous contacts (haphiæsthesiometric compasses of the writers, and of Michotte), the spatial distinction of successive contacts, and the spatial discrimination of two points, the one of which is fixed, while the other is moved away from it at an uniform rate (Michotte). Then follow experiments on the localisation of a contact, absolute or relative; on the various movement limens (kinesimeter with horse-hair stimulator); and on the cutaneous perception of form (stereoæsthesiometer). (2) The stereognostic perception of form is determined by means of a series of copper balls varying from sphere to ovoid; the forms are rolled between thumb and finger tips (dynamic stereoæsthesiometer). (3) The perception of the position and movement of the body is effected by a vertical tilt-board (somatic perception of the vertical) and an adjustable swing (perception of displacement in the vertical direction). (4) The concluding experiment measures the illusion, with passive pressure, of open and filled space. Ch. ii. takes up the perceptions connected with associated visual sensations. (1) The perception of depth is studied by means of luminous points in a dark room. (2) Five experiments are grouped under the heading of perception of magnitude: the discrimination of the lengths of horizontal and vertical lines; of the relative position of two points within a circle (the one point is fixed, the other is radially and angularly variable); of angles of different magnitudes; and of the areas of circles and squares. (3) The perception of form may be measured either by the solid forms of the stereoæsthesiometer, or by way of series of plane figures, in which the circle changes to the ellipse or the square to the oblong. (4) The movement limens are determined by the aid of luminous points in a dark room. (5) A few typical phenomena of stereoscopic vision are observed, with and without the Brewster prisms. (6) The time of perception is roughly measured by a simple form of tachistoscope (photographic shutter). The concluding experiments are devoted (7) to some typical geometrical illusions (length of lines, magnitude of areas, direction of lines) and (8) to the size-weight illusion. Ch. iii., on the measurement of perceptions connected with associated auditory sensations, comprises a single experiment, on the localisation of sound (the acousiæsthesiometer is employed). Ch. iv. describes two experiments on the measurement of perceptions connected with associated sensations of diverse modalities. (1) The sense of time requires somewhat elaborate apparatus: the authors describe a new-pattern electric chronoscope (vol. ii., p. 31), fitted with Piéron's interrupter. Short, moderate and long times (*e. g.*, times of 150, 600 and 2,400 σ) are limited by impressions of light (Plücker's tube) or sound (bell). (2) The sense of rhythm is studied by means of notched discs actuating an electric interrupter. Ch. v. brings us to the measurement of sensory attention. (1) The difference between surprised, reflex and voluntary attention is measured by the brief exposure, in a dark room, of a series of objects presented for recognition,—without signal, with a flash of light given 0.02 sec. before illumination, and with a preparatory 'Attention!' (2) The reinforcement of sensory intensity is

shown by the lowering of the differential limen (Michotte's movement-limen, determined as in ch. i., compared with the same limen under simple distraction), by sustained precision of perception with monotonous repetition of stimuli (special form of the cancellation test), and by the fluctuation of a liminal stimulus (acousiæsthesiometer). (3) The acceleration of mental processes in attention is proved by experiment (1) as just described, and by change in the duration of simple and choice reactions. (4) The fluctuations of attention are indicated by the mean variation of the reaction times. Ch. vi., on the measurement of sensory affectivity, recommends experiments by the serial method or the method of paired comparison on colors and tones, taken singly or in groups of simultaneous or successive terms.

Pt. III. discusses the measurement of phenomena of objectification. A subjective experience may be objectified in two ways: by stimulating to a motor reaction, which produces consequences in the external world, and by arousing an affirmation of external existence. The second or affirmative mode of objectification is treated in ch. i. (1) Assurance of testimony is measured by the familiar test-picture and questionnaire. (2) Suggestibility is measured, in the same experiment, by the addition of suggestive questions, and (on its sensory side) by the subjects' liability to an illusion of warmth. Ch. ii., on the motor mode of objectification, opens (1) with an account of the apparatus required for the reaction experiment; we have already noticed the new electric chronoscope, run by a 50 or 100 vs. electric fork; the authors' complete set of instruments for visual, auditory and cutaneous reactions is shown on p. 39. (2) Rate of voluntary movement is measured by a simple tapping test and by a sorting test. (3) Accuracy of movement is measured, most simply, by a tracing test; with sensory-motor adaptation, by an aiming test; symmetrically by rectilinear arm-movements across a vertical surface. (4) Motor fatigue and the fluctuation of voluntary effort are studied by the dynamograph or ergograph. (5) Motor suggestibility is measured by Binet's belted wheels; tendency to involuntary movement by one or other of the familiar instruments: the observer is instructed to inhibit all movement when a certain word appears in a list read out to him, and the instruction has a positively suggestive effect. (6) The limits of voluntary movement are determined by instruction to move the nostrils, ears, etc., and by experiments in free stereoscopy. (7) Motor inhibition is approached by way of the reflex wink and the knee-jerk; the authors figure a special reflexometer for the measurement of the patellar reflex.

Pt. IV. discusses the measurement of intellectual phenomena; ch. i. is devoted to memory. (1) The memory of elementary perceptions may be determined by the method of recognition, with all forms of sensory technique; a uniform interval of 1 min. is prescribed. (2) Special tests are outlined for the memory of complex perceptions: kinesic memory is investigated by means of tracing-forms; auditory memory by musical tones, triads, arpeggios, and melodic fragments (the material used is given in an appendix); visual memory by combinations of curved and straight lines, by pictures of simple objects, and (as memory of attitude and expression) by observation of an artist's lay-figure. This last test seems to the writer to be worthy of introduction into American laboratories. Memory of physiognomy and of complex scenes is tested by means of paired pictures, the members of the pair differing in slight details; the use of picture post-cards is recommended. Five experiments are described under the heading (3) verbal and intellectual memory. A preliminary section deals with the manner of presentation of the material; a simple exposure-screen, with two openings for alternate use, is figured. The experiments—or rather tests—are concerned with the memory of letters and figures, of words and syllables, of phrases, and of ideas (meanings), and with types of memory; samples of material are given in an appendix. The tests present

nothing new; and we therefore pass to (4) the tests of acquisition. These are merely outlined, under the headings: time of learning (nonsense syllables), economy of learning (optimal interval; partial *vs.* global learning), the influence of fatigue, the mutual influence of the perceptual elements (series of words with first or last syllable identical, etc.), and motor apprenticeship (typewriting with change of keys). (5) The phenomena of forgetting are examined in two ways: by increase of the interval elapsing between impression and recall, and by counting the repetitions necessary for the rememorising of a forgotten syllabic series. (6) A final experiment is devoted to the phenomena of localisation, *i. e.*, the reproduction of a presented temporal or spatial order. Ch. ii. deals with the measurement of the phenomena of association. Here we have (1) experiments on free association; rate is measured by the reaction time of the single association, wealth of ideas by the number of associations effected in a given time; the forms of connection may be classified as intellectual, verbal, and accidental. (2) Constrained association may be simply studied in the same two ways; a special experiment is devoted to abstraction (superordination). Imagination is also brought under this rubric: an elementary test consists in the presentation of words (visual or auditory) for reproduction in the reverse (literal or syllabic) order; complex tests are the building up of a sentence from a word, or of a narrative from a phrase, and the description of a picture. Ch. iii. advances to the measurement of logical phenomena. (1) Understanding is tested by the time required for the solution of a very simple geometrical problem. (2) Judgment is tested, ingeniously, by the presentation to the subject of sentences or pictures, some of which are reasonable and others absurd; the element of improbability is to be indicated. (3) Reasoning is tested by the characterisation of completed syllogisms as correct or incorrect, and by the drawing of a conclusion from presented premises. (4) Ingenuity is tested by a puzzle (arrangement of blocks).

Pt. V. is entitled "Determination of the Individual Synthesis." Ch. i., written in collaboration with Dr. M. Mignard, treats of voluntary control, *i. e.*, of the synthesis and direction, in the concrete case, of the elementary functions already measured. The apparatus used for the reaction experiment is employed to determine starting-times (action), stopping-times (inhibition) and times of change (decision); the actions called for are simple,—tapping, continuous addition or subtraction, etc. Mental stability is tested by the performance of experiments under various forms of distraction. Finally, the extent of the field of attention is measured by the assignment of a twofold instruction (alternate types of constrained association; addition and counting metronome beats), and the comparison of the results with those of the corresponding regular experiments. Ch. ii. discusses functional correlations and the comparison of individuals. It is impossible, if we start out with the tests, to reach a general measure of character or ability, or to determine the aptitude of the subjects for special vocations. On the other hand, we may start out with subjects of known ability or disability, and by examining them may be able to establish a norm of test-performance; or with any selected group of individuals, whom we may rank in the order of their standing under some particular test. These partial comparisons involve a comprehension of the nature of averages, deviations, and formulas of correlation, to which accordingly the main body of the chapter is devoted. Ch. iii. then takes up the rôle of observation in the determination of individual type. The peculiarly social side of human nature is beyond the reach of experiment; we cannot study our fellow-men as we can study animals, under the all-inclusive rubric of behavior. Even if we have at our disposal the backward forms of civilised mentality, or specimens of the backward races, a measurement of total human capacity is beyond our reach; subjective estimation must still play a part. Indeed, a field will always remain, in psychology, for

observation: individual acquisition, the intellectual sentiments, automatic and affective tendencies, the forms and degrees of self-control,—topics of this kind can be approached only by the observational method, which “deserves a kind of *technique* of its own.” Whether the authors intend to write this supplementary manual, as Dr. Hallion has undertaken to write upon physiological psychology, we are not informed; but the book is advertised to appear in the series.

E. B. T.

BOOK NOTES

Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, von G. E. MÜLLER. 1 Teil. Leipzig, Johann Ambrosius Barth, 1911. 403 p. 1. Abteilung, Zeitsch. f. Psychologie, hrsg. von F. Schumann Erg.-Bd. 5.

This work is divided into four parts. 1. A general introduction, statement of problems, discussion of types and their mixtures, etc. 2. Self perception, especially in experiments on memory; here are included the discussion of subjective and objective observation, the psychic process in the description of an outer object, the operation of self-observation in its various forms, methods of reminiscence, etc. 3. This part treats of the investigation of prominent events of memory. Here we have accounts of Rückel's number, sense and other tests. 4. The fourth division treats of the complexes built during the process of learning.

Les localisations cérébrales. Esquisse médicale et psychologique, par JEAN FERRAND. Paris, Jules Roussel, 1911. 87 p.

This writer concludes that the point of departure of writers of researches on cerebral localizations is false. Upon certain erroneous facts has been built a wrong psychic doctrine destined to give intelligence a material explanation. Certain clinical, anatomical and physiological facts have been used to serve a philosophic cause; and this has been allowed to go on on account of the preoccupations of metaphysicians. One result is the condemnation of the theory of images and the magnification of associationism, which seems now to have triumphed over the old philosophical spiritualism.

The function of suspense in the catharsis, by W. D. MORIARTY. Ann Arbor, Mich., George Wahr, 1911. 61 p.

The author thinks he can do justice to this subject by refraining from such questions as the history of the Aristotelian catharsis and competing theories and confining himself to discussing the function of suspense in general, then in the drama, and third, in the catharsis. Starting, then, from the drama, the author, we may say, cuts loose from Aristotle. The author does not deem it necessary to define exactly beforehand the meaning of suspense, any more than critics agree upon what catharsis itself is. In the higher psychocrasis the author distinguishes the functions of entanglement and disentanglement and denouement; and in the last part, on the nature and scope of the catharsis, he tells us of its surface theories, its deeper basis, the reasons for diverse views, and its true scope.

La théorie du rythme et le rythme du français déclamé, par EUGÈNE LANDRAY. Paris, Librairie Honoré Champion, 1911. 427 p.

This comprehensive work is divided into three parts, the first on the theory of rhythm, its relations to movement, perception, art and discourse. The second part treats of rhythm in French contemporaneous declamation, deals with energy, duration, accent, pauses, rhythmic divisions, syllabization, verse scansion, metre. The third part is devoted to examples of declamation, for instance in comedy, Mounet-Sully, the poets, Italian verse, nuance of duration in music, etc.

Experimental studies of rhythm and time, by J. F. WALLACE WALLIN. Reprinted from the *Psychological Review*, March, 1911. Vol. 18, p. 100-131.

This investigation leads the author to the following conclusions. 1. The different thresholds for time are invariably smaller than the first rhythm limen although the difference is not large. 2. The different thresholds are relatively smaller for the longer intervals. 3. If we compare the two methods, using the same pattern, it appears that the threshold for the continuous method is slightly smaller. 4. As to patterns, the limens are smaller for the trochaic than for the iambic type of measure. 5. As to the size of the time limens, the smallest relatively to the interval length is 4.5%.

The essentials of mental measurement, by WILLIAM BROWN. Cambridge, University Press, 1911. 154 p.

This work is written for the professed psychologists who are interested in quantitative methods and in biometric methods of correlation. The correlation theory ought to interest educational psychologists. In the first part, on psychophysics, the author discusses mental measurements and psychophysical methods, and in the second part, on psychophysical methods, its mathematical theory, its history, its experimental results and its significance in psychology. In an appendix he discusses the theories of Fechner, Müller and Urban, with correlation table, etc., with an excellent bibliography. The work is to quite an extent mathematical.

An introductory psychology, with some educational applications, by MELBOURNE STUART READ. Boston, Ginn & Co., 1911. 309 p.

This writer holds that the main truths of psychology can be presented in simple, straightforward, interesting fashion such as he here attempts. He strives especially to avoid technicalities that would tend to repel rather than attract, and give the impression that the topics are abstruse and far away instead of being closest of all. The author attempts nothing original but merely seeks to make selections, to emphasize, etc. On this principle, he first discusses consciousness and then the nervous system, attention, instinct, impulse, habit, the senses, apperception, feeling, interest, association, memory, imagination, concept, emotion, sentiment and will.

The essentials of psychology, by W. B. PILLSBURY. New York, the Macmillan Company, 1911. 362 p.

The author attempts here to present the accepted facts of psychology, emphasis being placed upon fact rather than theory. Where theories conflict the better one has been chosen and the others merely neglected. His point of view is functional with attention to what mind does rather than what it is. He stresses the outer manifestations of consciousness, and yet uses the results of structural psychology, making large use of the hypothesis of the synapse. Thus he treats first of the nervous system and neural action in relation to consciousness and behavior, and then discusses sensation, selection and control, attention, retention and association, apperception, memory and imagination, reasoning, instinct, feelings, the emotions, action and will, work, fatigue and sleep, interrelation of mental functions, and finally the cell.

Introduction to psychology, by ROBERT M. YERKES. New York, Henry Holt & Co., 1911. 427 p.

The author regards psychology as a description of consciousness, and so after an introductory chapter discusses this topic in various aspects, *e. g.*, concrete experiences or varieties of consciousness, analysis and the problem of psychological elements, syntheses and complex experiences, sensations as elements of consciousness and their properties, psychic complexes of apperception, feeling, memory and imagination. The next part discusses psy-

chology as the history of consciousness or genetic description in the individual and in the race. This subject is despatched in short metre and the author does but very slight justice to it. The next part treats psychology as generalizations dealing with observations, laws, principles, as found in apperception, association, affection and memory. The next part is psychology as explanation and correlation, physical and psychic, bodily and mental processes, behavior and consciousness, while the last part deals with the control of the mental life, education, eugenics, etc.

Some neglected factors in evolution; an essay in constructive biology. By HENRY M. BERNARD. Edited by Matilda Bernard. New York, G. P. Putnam's Sons, 1911. 489 p.

This book contains the mature results of twenty years of biological research to which the author brought a mind trained by mathematical and philosophical studies. In 1889, under Haeckel at Jena, he took up the problem of the origin of the crustacea. He later became greatly interested in entoptic phenomena and made a comparative study of vertebrate retinas which he thought did not consist of cells, as is usually stated, but a network of nodes which are formed by nuclei. This led him to doubt that the cell is, after all, the unit of structure of all tissue; he felt that it needed explanation. He finally reached the conclusion that in all living organisms there is a "protomitomic network." There must be more than one unit of structure. Periodicity and rhythm interested him also.

Medical revolution, by SYDNEY W. MACILWAINE. London, P. S. King & Son, 1911. 162 p.

This author pleads for a national preservation of health based on the natural interpretation of disease. He has lately retired from a long experience and puts his criticisms of the present practice of medicine in a plain, simple way in the form of an appeal to the people. His conception is based on Darwinism. All depends on the conception of disease, whether it is only negatively a deficiency of health or a symptom group with a special causation as in specific diseases. Diseases are of two kinds: those arising from the environment, extrinsic, and these may be of three kinds—parasitism, poisoning and traumatism; and the second class rising from the patient's constitution. These are intrinsic and fall into five groups— incomplete development, constitutional defect, overwork, deficient work, wear and tear. Now to diagnose is to determine which series the disease belongs to. Merely to determine the symptom group is not to find the cause. Specialism must disappear and the hospital system must be reformed.

La pensée contemporaine: les grands problèmes. Par PAUL GAULTIER. Paris, Hachette et Cie, 1911. 312 p.

The great problems here discussed are convention in the sciences, the reality of the sensible world, the inner life, the originality of sentiment, the reign of liberty, the beauty of art, the virtue of morals, social reform, political necessity, the end of monism, the future of pluralism, and the value of action. These he deems the chief problems of our day in both the theoretical and the practical field. He strives to be at once idealistic and practical.

L'année psychologique, publiée par ALFRED BINET. 17th year. Paris, Masson et Cie, 1911. 498 p.

In this volume the bibliographical analyses include pages 389 to 496 and follow the usual rubrics; all the rest of the volume is taken up with original memoirs. In one Binet discusses what is an emotion and what is an intellectual act; Cruchet, the psycho-physiological development of the infant from birth to two years. There are articles on special methods in psychol-

ogy; the relation of the school and society; psycho-physiology and mystic states; new studies on the measure of intellectual level of school children; psychic functions in mental diseases; morbid altruism; the delirium of interpretation and systematized delusions; definition of alienation; mental confusion; parallels in the classification of alienists.

Pubertät und Sexualität; Untersuchungen zur Psychologie des Entwicklungsalters, von AUGUST KOHL. Würzburg Curt Kabitzsch, 1911. 82 p.

The author first discusses the time of unconscious sexuality or of ignorance as to the nature of these phenomena, which he describes as a period of longing, yearning, vague and indefinite as is its nature. In the second chapter he characterizes the pubertal development of the young man and devotes another chapter to the young woman. The best trait of the book is the description of the mental characteristics of the dim and vague mental trance of this period of life.

Die Traumdeutung, von SIGM. FREUD. 3d. enl. ed. Leipzig, Franz Deuticke, 1911. 418 p.

Although nine years elapsed between the first and the second, only a little more than one year passed between the second and the third editions of this work. In this new edition, the writer has taken note of his coadjutors, particularly Steckel and Otto Ranck, who have co-operated with him in making additions and particularly the new citations of literature.

Recherches sur les sensations de rotation, par B. BOURDON. Rennes, Oberthur, 1911. 46 p. (Extrait du Bulletin de la Société scientifique et médicale de l'Ouest. t. XX, no. 1, 1911.)

Il subcosciente, da ROBERTO ASSAGIOLI. Firenze, Biblioteca Filosofica 1911. 30 p.

Un nouvel accoumètre, par B. BOURDON. Extrait du Bulletin de la Société scientifique et médicale de l'Ouest, 4^e trimestre 1910. 6 p.

L'anima. Firenze, Anno 1, Numero 2, Febbraio 1911. pp 35-62.

Psychologische Studien, hrsg von WILHELM WUNDT. Leipzig, Wilhelm Engelmann, 1911. 140 p. (Neue Folge der philosophischen Studien.) VII. Hefte 1 und 2.

Trasformazione e sublimazione delle energie sessuali, da R. ASSAGIOLI. Bologna, Emiliano, 1911. 11 p. (Estratto dalla Rivista di Psicologia Applicata, pubblicata e diretta da G. C. Ferrari. Maggio-Giugno 1911, Anno VII, N. 3.)

Proceedings of the Society for Psychical Research. August, 1911. p. 364-476. Printed for the Society by Robert Maclehose & Co., Limited, Glasgow, University Press.

Proceedings of the American Medico-Psychological Association at the 66th annual meeting, held in Washington, D. C., May 3-6, 1910. Published by the Medico-Psychological Association, 1910. 514 p.

On certain electrical processes in the human body and their relation to emotional reactions, by FREDERICK LYMAN WELLS and ALEXANDER FORBES. Archives of Psychology, No. 16, March, 1911. New York, Science Press. 39 p.

Subakute Raucherparanoia und einige andere Fälle von diffusem Beachtungswahn aus dem Gefühle subjektiver Unruhe oder unbestimmter Angst (drohenden Unheils), unbestimmter Erwartung, und aus dem Gefühle allgem. erhöhter Importanz der Eindrücke, von MAX LÖWY. Zeitschrift für die gesamte Neurologie und Psychiatrie, 1910. Band 5, Heft 4. p. 605-632.

- Stereotype "pseudokatatone" Bewegungen bei leichtester Bewusstseinsstörung (im "hysterischen" Ausnahmzustande)*, von MAX LÖWY. Zeitschrift für die gesamte Neurologie und Psychiatrie. 1910. Band 1, Heft 3, p. 330-340.
- Indian languages of Mexico and Central America and their geographical distribution*, by CYRUS THOMAS, assisted by JOHN R. SWANTON. Accompanied by a linguistic map. Gov't printing office, 1911. 108 p. Smithsonian Institution, Bureau of American Ethnology, Bulletin 44.
- Antiquities of the Mesa Verde National Park, Cliff Palace*, by JESSE WALTER FEWKES. Gov't printing office, 1911. 82 p. Smithsonian Institution, Bureau of American Ethnology, Bulletin 51.
- Indian tribes of the lower Mississippi valley and adjacent coast of the gulf of Mexico*, by JOHN R. SWANTON. Gov't printing office, 1911. 387 p. Smithsonian Institution, Bureau of American Ethnology, Bulletin 43.
- Preliminary report on a visit to the Navaho national monument, Arizona*, by JESSE WALTER FEWKES. Gov't printing office, 1911. 35 p. Smithsonian Institution, Bureau of American Ethnology, Bulletin 50.

BERLIN RESEARCH FELLOWSHIP

It is perhaps not generally known that the Sarah Berliner Research Fellowship for Women (an annual fellowship of \$1,000 "open to women holding the degree of doctorate of philosophy or to those similarly equipped," and "available for study and research in physics, chemistry or biology"), and the biennial prize of \$1,000 offered by the Naples Table Association "for the best thesis written by a woman, on a scientific subject, embodying new observations and new conclusions based on an independent laboratory research in biological, chemical or physical science" are open to workers in psychology. Applications for the exact conditions should be made to Mrs. C. L. Franklin, Chairman of the Sarah Berliner Committee, 527 Cathedral Parkway, New York, or to Dr. Jane Welch, Baltimore, Md., of the Naples Table Association.

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