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THE THEORY OF BINOCULAR COLOUR MIXTURE. II[1]

BY SHEPHERD DAWSON.

1. *Past theories.*
2. *The attention-theory.*

(The numbers in brackets refer to the literature appended to the paper "The Experimental Study of Binocular Colour-mixture," This *Journal*, 1917, VIII. 549.)

1. PAST THEORIES.

Controversy in the past has centred round the possibility of producing binocular colour mixture: very little interest has been shown in explaining either it or binocular rivalry. Haldat gave an explanation which was based on his theory of stereoscopic vision. Binocular colour mixture he explained in the same way as single vision with two eyes. An object appears single although it stimulates each retina and so evokes two sensations, because touch has shown that it is really single. Double objects would therefore appear single when, their impressions falling separately and simultaneously on corresponding points of the retinae, the same sensations were experienced as excited the idea of a single object. Therefore, in his experiments, although different colours stimulated the two eyes, they did so at points whose correspondence had been so established by habit that there could result only a single impression, which consequently was composed of the effects of the two stimulations.

[1] This and the previous paper constitute a thesis approved for the degree of Doctor of Science of the University of London. The writer desires to express his gratitude to Dr H. J. Watt for seeing them through the Press—a task which he himself was unable to undertake on account of absence abroad on military service.

This kind of explanation fails to meet the facts, for if binocular colour mixture be determined solely by similarity of the contours given in the uniocular fields, then there should be no rivalry between fields alike in form but different in colour: and all observers are agreed that rivalry does arise under these conditions provided that the colours are sufficiently different.

While Helmholtz never found that the result of binocular mixture was like that of uniocular mixture, yet he did not deny that others might succeed in doing so, and showed how such mixture might be explained by his theory of colour vision and his assumption that the "contents of each visual field come to consciousness separately without being fused with the other by means of any physiological mechanism, the fusion of the two fields being a purely psychical act" (23, p. 771).

According to his theory of colour vision any complex colour is due to the projection of three different colour sensations in the same part of the visual field. The three fundamentally different sensations evoked by the stimulation of a point on one retina have the same local signs, and are therefore not localised separately with regard to directions in the visual field and are fused in one composite sensation which in general "is the sensory sign of a single object." The only difference between the physiological processes conditioning uniocular and binocular colour mixture is that the activities of the photo-chemical processes which give the three fundamentally different colours are distributed over one retina in one case and over two in the other. In both cases it is an act of judgment which decides whether the composite colours ought to be considered as the sensory expression of a single quality of one object, or as that of two different qualities of two objects. Hence "it is not impossible that in the binocular superposition of two colours we may abstract from the difference between this kind of impression and that of uniocular mixture, and combine the colours as they would be combined in uniocular mixture. In fact, the resultant colour is nothing more than the addition of three different impressions which exercise no influence on one another but have the same localisation, and the acts of judgment which give sometimes combination, sometimes separation, may vary a great deal from one observer to another according to the practice and experience of each" (23, p. 781).

Several theories of binocular rivalry have been propounded. Dove attributed it to fluctuations in accommodation of the eyes which, being incompletely achromatic, could not simultaneously be accommodated for lights of different wave-lengths; each eye was therefore accommodated in turn to the light falling on it, and, as only the limits of these changes produced any change in consciousness, each colour was seen alternately. This theory will be considered in detail later in connection with binocular lustre, but in any case it obviously evades the psychological problems and cannot account for that form of rivalry in which splashes of the uniocular colours are seen simultaneously in different parts of the binocular field.

Wheatstone, Helmholtz, Meyer, and others have looked for an explanation in the fluctuations of attention. Attention, they say, is always fluctuating: as soon as it can discover nothing new in an object, it passes naturally to something else: this is what happens when the retinae are stimulated by light of different wave-lengths—attention is directed on each image alternately.

This theory receives some support from the fact that the fluctuations are to some extent controllable by the will. Helmholtz stated that he could always see the image to which he voluntarily attended. Funke, Dingle, Voelkers, Volkmann, Weber, and Welcker made the same claim. McDougall, by measuring the duration of the rival images, showed that volition had an appreciable effect in prolonging the appearance of one of them. Fechner thought that voluntary attention could produce a change in the binocular field, but could not determine its direction, *i.e.* could determine only a change in colour, but not the nature of the change. This change he believed was due immediately to eye-movements which themselves were controlled by the will. Wundt held a somewhat similar view.

One of the essential features of attentive consciousness is the coming to clear consciousness of certain ideas and the obscuration of others. In this respect (as we shall see later) binocular rivalry does resemble fluctuations of attention. This theory, however, implies more than this, namely, that the alternations are due to fluctuations in a peculiar form of mental activity which itself is only partly determined by external conditions.

Another type of theory seeks to account for the facts physiologically. Panum (36, 37) attributed them to intra-cerebral interaction of the nervous impulses from the retinae. He supposed that the image produced by the stronger nervous impression predominated: contours predominated because, contrasting strongly with their background, they made a stronger nervous impression than a uniform ground. Such an explanation seems too general to account for all the facts. It is not clear when a given nervous impression is the stronger, and why it should predominate.

McDougall, while assenting to the attention-theory, has endeavoured to find the physiological basis of the fluctuations of attention. Briefly, his theory is as follows. Though the cerebro-retinal mechanisms of the two eyes are in no part of their course identical, yet there is some anatomical connection between them in virtue of which the neurones from one retina can drain energy from corresponding or neighbouring parts of the other, and so produce partial or complete inhibition of the cortical processes of those areas. As the competition between these neurones for the energy set free depends on the relative condition of the synapses, which are readily fatigued and as readily recover, there is rapid alternation of the direction of the nervous current through the cortical areas of the right and left eyes respectively, and hence a corresponding fluctuation in consciousness.

It must be assumed that the specific nature of any colour sensation element (*e.g.* red, blue) is due either to the specific character of the energy which passes along the cortical neurones, or to the specific character of the neurones themselves. In neither case will this 'drainage' hypothesis account for the facts.

For (1) if the specific character of a colour sensation be dependent on the nature of the energy which travels along a cortical neurone, and if there be as many specific forms of energy as there are fundamentally different colours, binocular rivalry will be impossible. Suppose red and blue be due to the transmission of different kinds of nervous energy, and let one retina be stimulated by blue light, the other by red. Then, if the neurones stimulated by red light drain energy from the others, the energy which passes along these draining neurones must be complex and must give the colours red and blue; purple, therefore, should be seen, and not red. The two colours must be present, and purple must be seen, both when there is no drainage from one neurone into another, and when there *is* drainage either partial or complete; for whatever be the paths along which the nervous energies pass, the same changes will be evoked in consciousness.

The only way of replying to this objection would be by supposing that by some mechanism one form of energy is transformed into another kind when it is drained from one neurone into another, a hypothesis which has not been suggested.

(2) If, on the other hand, we assume that the specific character of a colour sensation is due to the nature of the path along which energy travels, and not to the specific nature of the energy itself, we are still unable to account for all the facts. As before, suppose energy to be drained from the neurones of the eye stimulated by blue light into those which are stimulated by red light, and suppose the drainage to be complete. Then the energy transmitted along the draining neurone must be greater than it was before. But what of the brightness or saturation that is presumably dependent thereon? Will not the increase of energy effect an increase in either or both of these? Unfortunately for the theory, the predominating red is never purer than the uniocular red: it is generally less pure and more or less tinged with blue. This difficulty might be evaded by supposing that the neurones which drain energy from the others are themselves fatigued to such an extent that the energy passing along them is never greater than it is when the other eye is closed: but there is no evidence of such rapid fatigue. McDougall's theory would therefore require to be supplemented by at least one subsidiary hypothesis even more speculative than his main assumption.

All the facts of binocular colour mixture and rivalry were summed up by Hering in his law of the 'complementary sharing of the retinae in the field of vision,' which he stated thus: "In binocular vision each retina can in the common visual field make the sensation belonging to it effectual only with a fractional part, and in such a way that the sum of these fractions is unity" (25, p. 596). Thus if one eye contributed to the binocular field $1/n$ of its sensation, the other contributed $1-{}^1/n$ of *its* sensation. In successive rivalry each eye alternately contributed the whole of its sensation to the binocular field; in simultaneous rivalry and mixture each participated with a part only. The two eyes could not at the same time participate with the whole of their sensations, for in that case the binocular field would have been brighter than the uniocular field, as bright, in fact, as when both stimuli acted simultaneously on the same part of one retina. When both eyes were observing the same surface, there was rivalry between them, but it was not perceived because each made up the deficiency of the other.

Even as a summary of the facts this statement is open to some criticism. By what right do we assume the existence of a rivalry which is unnoticed, and what are we to understand by a 'part of a sensation'?

Two phenomena of binocular colour vision have earned for themselves special names and have generally been considered apart from rivalry and mixture, viz. binocular lustre and Fechner's paradoxical effect. Dove (14–17), to whom we owe the discovery of binocular lustre, attempted to explain it on the basis of the incompletely achromatic character of the eye, whereby differently coloured objects equidistant from the observer appear to be at different distances. Binocular lustre was the experience resulting from the attempt to combine these colours when they fell on corresponding parts of the retinae. In every case where lustre appeared he maintained there was a mirroring transparent or transluminous layer through which another body was clearly seen and that it was the consciousness of this clearly perceived body and the unclearly perceived transparent layer which constituted the experience of lustre.

The lustre that is produced by the binocular combination of white and black surfaces cannot readily be explained by this theory, because here the difference between the stimuli is not one of refrangibility of light rays, but of their intensities. Dove recognised the difficulty and tried to meet it in the following way. Black and white, he said, were related to each other in exactly the same way as two different colours, for they gave lustre just as colours did, and black and white lines in stereoscopic figures which gave lustre lay side by side in the same way as did coloured lines: black and white surfaces would therefore be seen at different distances. He confessed that he was unable to demonstrate this experimentally, but offered the following argument. The pupil widened in the dark and contracted with increasing illumination; it was also smaller when observing near objects than when observing distant objects. A darker object, would, therefore, under similar external conditions, appear more distant than a white object. As the accommodation of the eye for white objects was different from that for dark objects, it would, in passing from one colour to another of the same hue but different intensities, change in the same way as when different colours of like intensity were observed, and therefore the brighter was related to the darker as a less refrangible colour was to a more refrangible.

Brewster (3–5) opposed Dove's theory. He could see no reason for assuming that the colours which gave binocular lustre appeared at different distances, and were seen through each other. Dove's theory was quite incapable of explaining the complete absence of lustre when a white surface without boundary and a black surface of the same kind were seen through the stereoscope. The cause of the lustre of mother-of-pearl and of thin plates not in optical contact was quite different from the cause of binocular lustre. The latter he believed to be due to the effort of the eyes to combine two pictures and to the dazzle occasioned by the alternating intensities of the combined tints, the impression of one of the tints sometimes disappearing and reappearing. It was a species of lustre *sui generis*. It was a physiological, not a physical phenomenon, and had no relation to the lustre arising from the combination of lights reflected from the inner and outer surfaces of laminated transparent or translucent bodies.

In support of this view he adduced the following observations. Under the same circumstances and with eyes equally good, lustre was seen by one person and not by another. Those who did see it did not always describe it in the same way, and therefore probably did not have exactly the same kind of experience. Lustre was not seen when the shades of the combined surfaces were slightly, or very greatly, different. The effect given by slides with diagrams was different from that given by slides without diagrams.

Other objections have been raised against Dove's theory by Helmholtz and Wundt. Helmholtz objected that changes of accommodation do not give the nice appreciation of distance required by the theory. Wundt pointed out that if difference of refrangibility of the light rays stimulating the retinae were the determining cause of lustre, then colours farther apart on the spectrum, *e.g.* red and blue, would give more brilliant lustre than colours nearer together, *e.g.* yellow and blue; but they did not.

The lustre produced by combining black and white stereoscopic pictures was explained by Helmholtz in the following manner. A dull surface reflected light equally in all directions and so presented the same luminous intensity to both eyes. Lustrous surfaces on the other hand reflected light more or less regularly. One of the eyes might be in the line of the reflected light while the other was not, and thus the surface would appear more strongly illuminated to the first than to the second. Consequently, when pictures of different brightness were combined in the stereoscope, an experience was evoked which normally could only be produced by lustrous objects, and the surface therefore appeared lustrous.

The lustre due to the stereoscopic combination of different colours he explained in the same way. A shining object surrounded by coloured objects might send to each eye reflected light of different colours, and so present different colorations to the two eyes: a dull body under normal conditions would present the same coloration to the two eyes. Hence if in the stereoscope the same object presented different colorations to the two eyes, a sensory impression would be produced that could only be given by shining objects. These differences of colour were not very great, for which reason lustre was produced more easily by colours which were nearly alike than by those which were very brilliant and very different.

Helmholtz found that stereoscopic pictures which gave binocular lustre with prolonged observation also gave it with the momentary illumination of the electric spark. This he considered important because it showed that lustre was not the result of change of illumination or coloration.

This explanation, obviously, marks no great advance on the position taken up by Brewster, for it consists merely in pointing out the similarity of the external conditions under which binocular lustre is produced by stereoscopic pictures and by naturally lustrous objects.

Wundt's (55–57) explanation of binocular lustre was a corollary to his theory of monocular lustre. An essential condition of all lustre was, he maintained, contrast either of brightness or of hue between the colours producing it: if the colour-tones were similar, their brightness-difference would have to be fairly great, and if their brightness were the same, their colour-difference would have to be appreciable in order to give lustre (25, p. 320). Another condition was the separation of the constituents of sensation by means of judgment. "In sensation," he writes, "the impressions are mixed, however different the objects from which they proceed. But since in the idea every impression is referred to its object, there is ascribed to each its own amount of participation in the mixture. Thus the idea corrects, as it were, what is reported by sensation" (57). In this respect monocular lustre was like reflection; in fact, these two phenomena passed gradually into each other. Lustre appeared when one object was seen reflected in another in such a manner that, while they were both seen, each hindered the clear apprehension of the other. It gave place to reflection when the reflecting body was quite overlooked, and to mixture when both were located in the same place.

Binocular vision, he said, favoured the perception of lustre merely because it facilitated the spatial separation of sensations, while the fundamental condition of lustre, viz. the simultaneous perception of object and mirror-image, was unaffected. By means of the stereoscope the reflected image could be seen with one eye, while the reflecting object was seen with the other, and as two objects could not be seen in the same place, an illusion was produced; one was seen behind the other, and each prevented clear apprehension of the other.

Fechner's paradoxical effect is given by a slight modification of Haldat's experiment with coloured fluids. Fechner (18) found that if, after looking with one eye at the sky, he held before the other a piece of moderately dark grey glass and opened both eyes, the sky became perceptibly darker. As the addition of light falling on the hitherto closed eye produced, not an increase in the total brightness (as one might expect from purely physical considerations), but a decrease, this effect was called the paradoxical effect.

By using grey glasses of different absorption-strengths Fechner found that as the darkening medium became darker the brightness of the binocular field decreased, but only to a certain point, which he called the minimum-point; after this the field became gradually brighter until with the darkest glasses it was immaterial whether the covered eye was closed or not. He measured the absorption-strengths of his glasses and the brightness of the binocular fields and represented by means of a curve the relations between the two.

The position of the minimum-point varied with different observers: with moderate illumination (a bright sky) it was given by a glass which transmitted about four per cent. of the light that fell on it. Corresponding to any glass whose absorption-strength lay between infinity and the value which gave maximal darkening of the binocular field, there was another of less absorption-strength which produced the same amount of darkening of the binocular field: these were called conjugate glasses.

The curve was plotted by finding the conjugate of each of a series of grey glasses. Some difficulty was experienced in determining points on the curve, partly on account of inability to procure a properly graded series of grey glasses, and partly on account of the large number of doubtful judgments that appeared. The minimum-point was not really a point, for the curve changed its direction in that region only very slowly. Fechner maintained that the sky looked brighter in binocular vision than in uniocular, and found what he called the indifference-point of the curve of brightnesses. This point marked the brightness of the grey glass which, when held before one eye while the other was uncovered, gave a brightness to the binocular field which was equal to that of the field of the other eye alone. All glasses which let through more light than this one gave a binocular field which was brighter than the uniocular. The indifference-point varied, but on the average it was given by a glass which transmitted about two-thirds of the light which fell on it.

The positions of the principal points on the curve varied with different observers when tested under the same conditions and even with the same individual when he was tested at different times. Contradictory results were obtained unless great care was taken with the external conditions of the experiment, such as the duration of the observation, holding the glasses alternately before each eye, etc. Variations in the brightness of the sky did not affect the positions of the principal points of the curve. This opinion, however, seems to have been based rather upon general impression than upon any precise attempt to study the question experimentally.

Aubert (1) using his episcotister instead of grey glasses repeated Fechner's experiments and verified his results. He found, however, that the position of the minimum-point varied very considerably with the brightness of the object. He also used coloured glasses as well as grey glasses and got similar results, the only difference being that minimal traces of colour were detected with greater certainty than minimal darkening and that rivalry was more frequent with coloured glasses.

The paradoxical effect, Fechner maintained, could not be due to sympathetic widening of the pupil of the uncovered eye, for it was observed when such changes were precluded by using an artificial pupil. He considered three other possible explanations: two he dismissed as untenable, the third he favoured, but could not work out in detail. These he called the combination-theory, the attention-theory, and the antagonism-theory.

According to the first, the darkness produced by the grey glass was added to the brightness of the image of the uncovered eye and gave a field whose brightness was the mean of those of the bright and dark images, whereas when one eye was closed, only the bright image of the other was seen. To this Fechner objected that if the brightness of one image combined with the darkness of the other, then combination ought to take place between the bright image and the complete darkness of the closed eye. If the brightness of the binocular field were the mean of the brightnesses of the uniocular fields, then, as one image was gradually darkened, the binocular field should gradually darken until one eye was completely dark; but, as a matter of fact, the field darkened and then brightened again.

According to the attention-theory, the paradoxical effect was due to distraction of the attention: instead of being concentrated on the bright eye, as happened when the other was closed, it was divided between the two. To this he objected that there was no apparent reason why the division of attention should proceed only to a certain point as the brightness of one image was gradually decreased, nor why the distribution of attention between two retinae should explain a result which did not appear when attention was distributed over different parts of the same retinal field. When attention was distributed over two parts of the same visual field, one of which was dark, the other bright, there was no reduction in the brightness of one nor increase in that of the other. Further, he maintained that the phenomenon was independent of the voluntary direction of attention, and therefore was probably not due to the cause suggested.

The antagonism-theory, which Fechner favoured, is rather a re-statement of the facts than an explanation. According to it "the combination of light on one retina with light on the other exercises an inhibitory influence on the sensation of light" (18, p. 462), and this inhibitory influence increases to a certain point (the minimum-point) and then decreases. He made no attempt to work out the details of the theory, but suggested that this antagonism referred not only to the psychic activity of sensations, but also to the physiological activity underlying it, and that it was akin to the relations of antagonism which existed between different parts of the organism.

Helmholtz (23) demonstrated the dependence of the paradoxical effect on contours. Looking at a well-lighted white door he held before one eye a grey glass and got the paradoxical effect, but when he placed between the glass and the door a large sheet of white paper just as bright as the latter, this effect disappeared immediately. "This modification of the experiment," he writes, "shows that we are not concerned here with a modification in the sensation of light, but only with a modification of our judgment on the real colour of the object. When one of the visual fields is dark or filled with a uniform feeble light (the image of the white paper seen through the dark glass), instead of attributing this uniform lighting to the colour of the door, we base our judgment exclusively on the colour seen by the eye which distinguishes the contours of the door. At most the changes of illumination in the other eye show themselves under the form of a dark or luminous mist which spreads in front of the door and other objects. If, however, we recognise also the contours of the door with the darkened eye, and see them in the dark grey field, this grey, as well as the white of the other eye, appears to us to be part of the proper colour of the door, and for this reason the door itself appears darkened. It appears to us then as a grey body brightened by white light. This darkening evidently cannot occur either when the darkening produced by the glass is very feeble or when it is so great as to prevent us from distinguishing the objects" (23, p. 791).

McDougall (30) has explained the paradoxical effect by his 'drainage' theory, in much the same way as he explained binocular rivalry. He assumes that the neurones stimulated by the brighter illumination are rapidly fatigued, and that the consequent increase in their resistance enables the less fatigued neurones excited by the darker field to drain off some of their energy and so diminish the brightness of the sensation-elements contributed by them.

This theory is based on the results of experiments in which discs of light of different intensities were binocularly combined. In these experiments it was found that if fixation was prolonged, rivalry occurred, that the effect was greatest when the two areas were coincident, and that, if the darker area was much smaller than the brighter, there was no paradoxical effect. The theory, therefore, takes no account of the part played by contours within the areas which give the effect, and so neglects one of its principal conditions.

While it is difficult to refute the assumption on which McDougall's explanation of Fechner's paradox is based, there are some facts that are not readily explained by it. One of these is the appearance of the paradoxical effect after closing, and so resting, both eyes, when there can be no question of fatigue of the more strongly excited path. Another is the fact that the binocular field is never brighter than the brighter of the uniocular fields, as it ought to be, if drainage of energy into the more open path means an increase of energy in the latter and a corresponding increase in the brightness of sensation. Yet another difficulty is that of explaining why as a series of grey glasses of gradually increasing absorption-strengths is used, the darkening of the binocular field increases gradually to a maximum and then decreases.

2. THE ATTENTION-THEORY.

Binocular rivalry. Although no wholly satisfactory explanation of binocular rivalry has been propounded, that which seems to fit the facts best is the attention-theory. Binocular rivalry certainly is analogous to the fluctuations in experience which are usually attributed to fluctuations of the attention, and hence is probably to be explained in the same way. As the analogy is generally taken for granted and forms the basis of this theory, it may be advisable to detail here the specific points of resemblance between the two groups of phenomena.

They are alike both in their nature and in the conditions that affect them. The uniocular images drop into and out of consciousness in much the same way as do faint shadows, the feeble ticking of a watch, thoughts, and mental images. Every gradation between binocular mixture and complete predominance of one image is observable: similarly, there is every gradation between the full and complete awareness that is due to the direction of attention on one thing and the less clear awareness that follows the distribution of attention over several things.

There is one aspect of our experiences of which we know very little, namely, the qualitative and intensive variations in them that are due to variations in the direction of attention. Only a few of our sensory experiences are attended to: the rest form an obscure but very important background of which we know very little. Several attempts have been made to compare the intensity of a sound heard attentively with its intensity when heard inattentively, but although the evidence seems to point to the conclusion that the former is greater than the latter, it is not very conclusive. No attempt, to my knowledge, has been made to perform similar experiments with colour stimuli for the purpose of finding whether the hue, saturation, or brightness of a colour varies with the degree of attention that is given to it. If binocular rivalry be a case of fluctuation of attention, we must suppose that the changes which take place in the binocular field when the retinae are differently stimulated, indicate the nature of the variations in colour sensations which are produced by variation in the direction of attention.

It may be objected that a colour may disappear although attention be directed on it continuously, and that rivalry therefore cannot be due to fluctuations of the attention. The same objection may be and has been raised against a like interpretation of the fluctuations of faint shadows, feeble sounds, and even of mental images and thoughts. It may be valid if the word attention be used in a very restricted sense to mean consciously self-directed activity; but in any case it cannot legitimately be raised against the position we are trying to establish, viz. that there is a very close resemblance between binocular rivalry and the fluctuations of sensations, images, etc., which have traditionally been ascribed to fluctuations of attention.

Not only is there a general resemblance between these fluctuations and rivalry, but they are affected by similar conditions. Differences of intensity and quality have similar effects in both cases. It is almost impossible to keep simultaneously prominent in consciousness two strong impressions, *e.g.* an emotion and an exacting piece of thinking, a picture and a song, or even a taste and a smell. Either one steps forward while the other recedes into the background, or they are held together loosely and both lose much of their strength. In so far as one element in experience is vivid, it is so at the expense of others: several elements may be equally prominent, but only if they be relatively weak. Similarly in binocular experience we can at one and the same time see both of the uniocular colours, but only when there is very little difference between them (see pp. 518–527)[1].

Binocular rivalry is produced somewhat less readily when there are similar contours in the two images than in the absence of such contours (pp. 538–549). Similarly, two heterogeneous impressions, *e.g.* a picture and a song, can be made equally prominent by being made subsidiary to a unity of which each forms a part.

Practice seems to have precisely the same effect on binocular colour vision as on any other experience. It leads to the more easy separation of the images rather than to mixture. Repeated attention to the elements of any complex experience makes it more difficult to keep

[1] "The experimental study of binocular colour mixture," This *Journal*, 1917, VIII. 510; cited in the following pages in the same way as now—without title or number.

them together. In support of this view may be cited the experience of the microscopist who on first using a monocular instrument is much disturbed by the field seen by the eye which is not looking through the instrument, but after some practice is able to keep both eyes open and see only the field of the other eye. Helmholtz seems to have experimented to such an extent on binocular vision that he had complete control over his images and could always distinguish them even when they were seen simultaneously. It seemed to me that in the course of my experiments I came to find it more and more difficult to get binocular mixture.

Local fatigue has the same effect in the two cases. If one eye be fatigued more than the other by exposure to a colour stimulus, the image seen by the rested eye predominates when both eyes are opened. So it is in the more general field: if one sense be fatigued, the impressions from others will predominate, *e.g.* smells, tastes, and sounds readily distract one who has been attending for some time to visual impressions.

Volition also seems to have the same general effect in both cases and is subject to the same limitations. Helmholtz could voluntarily hold either image continuously for a very long time. McDougall demonstrated the effect of will by measuring the intervals during which a colour could be held and comparing it with the intervals during which it appeared when no attempt was made to hold it. Further, will produces its effects here in precisely the same way as in other cases, namely, by leading the subject to think about the colour he wishes to hold and causing him to keep asking questions about it (cf. 23, p. 770).

Other conditions which affect rivalry, *e.g.* change in one of the images, the presence of contours, etc., have their analogues in the conditions which determine the direction of attention to heterogeneous impressions.

These analogies between binocular rivalry and the oscillations which have been so often attributed to the attention are sufficient ground for believing that both are to be explained in the same way. Their explanation must be based on a general theory of mental activity, which is obviously beyond the scope of this paper.

Binocular colour mixture. Haldat (21, p. 397) held that binocular colour mixture could not be due to convergence of the optic nerves on a common cerebral centre whose functioning was correlated with visual experience, and later investigation has confirmed this conclusion. From the general character of the antagonism of contours Helmholtz concluded that "man has the faculty of perceiving separately the images of each visual field without being troubled by those of the other," and that consequently "the contents of each visual field come into consciousness without being tied by an organic disposition to those of the other, and, therefore, the fusion of the two visual fields in one common image, in so far as it occurs, is a psychic act" (23, p. 772). Sherrington's researches on binocular flicker led him to a similar conclusion. McDougall has adduced other arguments in support of this view, one of the strongest of which is the fact that there is no summation of brightnesses in binocular vision. My experiments with colours of different hue, brightness, and saturation (pp. 513 et seq.) and on Fechner's paradox (pp. 547–9) have strengthened this argument, for they have shown that in so far as binocular mixture is produced, it approximates to the colour that is given when half of the light that was thrown on one retina is combined on the same retina with half the light that was thrown on the other. If the light that was distributed over two retinae be thrown on only one, the resulting sensation is very much brighter than the binocular mixture of the same colours.

As the cortical centres of the retinae are physiologically distinct, we must assume that when a binocular mixture is produced, each cortical centre develops its own sensory elements and that by a psychical act these are combined in perception. Separateness of the cortical centres of the retinae will not, however, explain the separateness of the uniocular images in binocular perception. This must rest on some kind of difference between the images themselves, for, in the absence of any such difference, they must not only be indistinguishable in binocular perception, but must be incapable of producing any binocular experience different from that which would be produced by stimulating only one retina.

Uniocular images may differ in form, colour, duration, and position in time. Duration and position in time do not concern us here. The importance of difference of form and coloration in effecting separation of the uniocular images has already been demonstrated (pp. 518–549). They do not, however, of themselves explain why two colour stimuli acting on different retinae should give different experiences from that which is given when they act on one retina. There must be some other kind of difference.

The cyclopean character of binocular vision suggests that corresponding points have the same local signs; but this is not strictly true. Animals with uniocular fields of vision which do not overlap seem to use both eyes without confusing the right- and left-fields; presumably, therefore, the local signs of points on one retina are entirely different from those on the other. Human vision is not essentially different from this non-overlapping vision[1]; our eyes are merely placed frontally in the head so that points on each retina are stimulated by light from the same object: one field is, as it were, slid over the other. It is conceivable that corresponding points may have identical local signs, but the reversal of perspective which is produced by interchanging the right- and left-eye views of a stereoscopic slide showing a geometrical solid proves that their local signs are really different, for, in the absence of some kind of attribute distinguishing the right and left fields, it is impossible to explain the reversal.

It is, I suggest, these differences which frequently prevent the binocular fusion of different colours; otherwise we cannot understand why red light and blue light should always give the sensation purple when they stimulate the same part of one retina, but not when they stimulate corresponding parts of different retinae. If this be granted, then not only have we a psychical basis for the separateness of the uniocular constituents of the binocular field, but we can suggest the basis of the 'act of judgment' which, according to Helmholtz, decides whether the uniocular images are to be combined in the perception of a single object or held apart.

The basis of this act of judgment is revealed in the results of the experiments described in the preceding paper. It consists in the possibility of interpreting the uniocular images as pictures of the same object. Anything which prevents this destroys binocular mixture.

In support of this theory the following experimental results may be adduced. Uniform discs of colour give a binocular mixture like the corresponding uniocular mixture only when the difference between the colours is very slight, so slight that it resembles such differences as might be due to differences of illumination of the retinae or to slight pigmentation of the optic media With larger differences of coloration the binocular field is unlike the uniocular mixture; either it changes or, if change cannot be detected, its colour more clearly contains the uniocular colours. The latter is a curious experience which is not easily explained. The subject may see with one eye a reddish grey and with the corresponding part of the other a greenish grey, and, though he can detect no change in the binocular field, he frequently says it is both greener and redder than the corresponding grey uniocular

[1] Cf. Watt, H. J., "Some problems of sensory integration." This *Journal*, 1910, III 323-347.

mixture. Whether this is because the binocular field changes from green to red and back again and the subject is unable to detect the process of change, or because some parts of the field are more red or green than others and these local differences are not detected as such, I do not know. Sometimes, when I tried to answer the question by observing more closely, I thought that slight changes or slight local differences of coloration were detectable; but the problem cannot be solved in this way, for as soon as one begins to observe the field carefully the eyes cease to move, which, as we have already seen, favours rivalry. It may have been this experience which led Helmholtz to say that he never saw a binocular mixture like the corresponding uniocular mixture, but always the two colours simultaneously and in the same place.

The effect of contours on binocular mixture is another piece of evidence in favour of this view. Contours, either because of habit and practice, as Helmholtz suggested, or more probably, on account of innate psycho-physical disposition, are more readily attended to than uncontoured fields, and hence predominate in the binocular field.

Uniocular fields in which are markedly dissimilar contours cannot be interpreted as right- and left-eye images of the same object, and hence do not combine to give a binocular field like that which would be given by throwing the pictures on one retina. Similar contours, on the other hand, facilitate binocular mixture, and the more so the more abundant the contours. These similar contours apparently counteract the dissimilarity between the two images that is due to difference of coloration. Very interesting in this connection are the results of the experiment described on page 538 in which were combined green and red postage stamps printed from slightly different dies. Occasionally the binocular field was distinctly red or green, but generally it was grey with a slight tinge of red or green. The portrait in the binocular field had not the singleness of either of the uniocular images, and yet it was not what would have been seen had the prints been superposed by stamping the red and green dies successively on the same sheet. By examining any part of it carefully, some of the lines, but not the colour, of one of the uniocular images could be recognised; these lines changed, but the change was not easily detected. The production of mixture in this case shows that it is not so much exact similarity of contours that facilitates mixture, as similarity of their meaning. The experience evoked by this slide may be compared with the binocular perception of depth in which the slightly disparate uniocular images cannot be detected unless an unnatural attitude is adopted and search is made for them.

Binocular colour mixture raises in an acute form one of the great problems of colour vision, viz. the explanation of fusion. When two tones, say *c* and *e*, are sounded simultaneously, they do not disappear and give an intermediate tone *d*; each is still audible in the total sound-complex: but when light-rays which separately evoke the colours blue and red are thrown simultaneously on the same part of one retina, they do not evoke a colour-complex in which each of the colours red and blue is visible; they evoke another—purple—which is intermediate between the two. The difference between tone and colour in this respect is still more marked if the light-rays be those which separately evoke complementary colours, say red and green; then the colour-complex does not even resemble either of the complementary colours.

In binocular vision there is sometimes an approach to what we find in sound, for occasionally the two uniocular colours seem to be visible simultaneously in their common field without forming a mixture like the corresponding uniocular mixture. This and the fact that steady binocular mixture is seldom observed seems to indicate a separateness of the uniocular images in the binocular field analogous to the separateness of tones in a tone-complex.

Fechner's paradoxical effect is merely a particular case of binocular colour mixture, and is no more paradoxical than any other case of binocular mixture. There is no essential difference between the darkening of the binocular field which is produced by holding a grey glass before one eye and the coloration of the same field by using one or two coloured glasses. The only striking difference is in the amount of rivalry that is observed.

It is to the effect of similar contours in the uniocular fields that we must attribute the gradual darkening and subsequent brightening of the binocular field which is observed when one eye is uncovered and grey glasses of gradually increasing absorption-strength are held before the other. Until the point of maximum darkening of the binocular field is reached, objects are seen through the grey glasses sufficiently clearly to enable both fields to be equally prominent in consciousness; but after this point has been passed, objects become less and less clearly visible through the grey glasses, and the field of the uncovered eye comes to predominate more and more.

The following evidence in favour of this view may be quoted from the records of the experiments of the previous paper. Until the region of maximum darkening is reached, objects are clearly visible through the grey glasses and the brightness of the binocular field is the mean of the brightnesses of the uniocular fields (see p. 548), *i.e.* it is like what I have called the 'corresponding uniocular mixture.' With darker glasses objects are not clearly visible, and the binocular field is brighter than the mean of the uniocular brightnesses.

The paradoxical effect is not given by uncontoured areas of different brightness, *e.g.* by combining white and grey, or white and blue discs: these give rivalry or lustre, not the steady effect given by grey glasses (pp. 523–7, 540–5).

If a contoured area be seen by one eye and an uncontoured area by the other, the former predominates almost unchanged in the binocular field. A white disc and a blue one with similar contours give the paradoxical effect, unless the outlines on the blue disc are obscured by the deep coloration, but, when the blue disc is replaced by another without contours, the white contoured area predominates alone (pp. 542–4).

If the blue disc be very dark so that its contours are scarcely seen, the binocular field is white with a slight tinge of blue; but, if the contours on the blue disc be made to stand out more clearly, *e.g.* by painting them with some white medium, then rivalry occurs or the binocular blue becomes perceptibly darker (pp. 541, 544).

The change in position of the minimum-point with increase or decrease of illumination, which, of course, changes the distinctness of the outlines, is further evidence for this view. Aubert (1) has shown that the minimum-point is lower, *i.e.* given by darker glasses, with bright illumination than with less bright, a result which I have myself confirmed.

Additional evidence can be found by placing in the stereoscope a line drawing of a geometrical figure like that shown in Figure 2, and holding grey glasses before one eye. Until the minimum-point is reached, the drawings give the stereoscopic effect with darkening of the binocular field, but after it has been passed this effect is destroyed; the image of the uncovered eye then predominates, but at times portions of the figure behind the grey glass can be seen appearing and disappearing, the periods of appearance and disappearance becoming less and less frequent as the glasses get darker; at these times a slight darkening of the binocular field can be detected. The disappearance of the stereoscopic effect with absence of darkening of the binocular field is another proof of the dependence of the darkening on the presence of contours.

Transparence. While binocular mixture and the paradoxical effect are due to the influence of similar contours in combining different colours or brightnesses in the perception of a single object, transparence—the seeing of one object through another—is due, at any rate under natural conditions, to the effect of dissimilar contours in evoking the perception of two different objects in the same part of the visual field. Transparence is somewhat like mirroring. We seem to see at one and the same time the surface of a polished table and the reflection of objects in it. Here enough of the colour and form of both the reflected image and the reflecting surface is present to produce the perception of each, and they are therefore seen simultaneously.

Our awareness of the transparence of natural objects can be explained in the same way. The transparence of an object is observed most readily when this object moves relatively to the objects seen through it. Here is a sequence of visual impressions in which colour and form change in a manner that can only be explained on the assumption that we are looking through one object at another. There is a separation of the sensory elements of the visual field and an integration of them into different objects.

In my experiments with coloured discs the conditions were sometimes analogous to these. When the colours in the binocular field were such as suggested that two coloured discs were present simultaneously, the observer seemed to see one colour through the other. This experience was given very clearly when on account of inaccurate convergence the two discs did not exactly coincide, and most clearly when they were moving towards or away from each other. Certainly, when attention was directed on the area where the discs overlapped, either one colour or a mixture of the two was seen—generally each uniocular colour was pure about the edge of its own disc—but the visual field as a whole gave the impression of seeing two discs of colour simultaneously in the same place one through the other.

Such incomplete coincidence was not, however, necessary, for the colours of perfectly coincident areas sometimes changed in such a way as to give the experience of transparence. This happened often when one of the colours was seen pure in some parts of the field, and the other in other parts, while elsewhere there was a shimmering lustrous effect like that observed on looking through plate glass.

This explanation of binocular transparence receives some support from the introspective records. It has already been remarked that although one colour seemed to be seen through the other, they were not perceptibly separated by space as are objects that are really at different distances from us. This is because the conditions are lacking here which normally produce the perception of depth. Binocular depth is dependent mainly, if not entirely, on slight differences of form[1] in the uniocular images: such disparity was lacking in the coloured discs used in my experiments; hence the true stereoscopic effect did not appear.

The absence of these sensory conditions of the binocular perception of depth accounts also for the fact that the transparent colour was sometimes the brighter, sometimes the darker, since binocular transparency being dependent merely on the accidental arrangement of the colours and on non-sensory conditions, the absence of retinal disparity makes the relative distance-values of the colours ambiguous. This transference of transparence from one colour to the other is somewhat like the reversibility of perspective in certain geometrical illusions, *e.g.* the truncated pyramid, and is equally dependent on non-sensory conditions.

As the perception of transparence is largely a matter of interpretation, and as this must vary with non-sensory as well as sensory conditions, *e.g.* the 'attitude' of the subject, it will readily be understood why this effect was not steadily present in every observation in which colours of different brightness were combined.

The entire absence of transparence, when colours of the same brightness were combined, is probably due to the influence of past experience. The fact that there are always very pronounced differences of brightness in the visual field when one thing is seen through another sets up an attitude which makes it impossible to get the experience of transparence from colours of the same brightness.

Binocular lustre. One of the essential conditions of binocular lustre is the presence of differences of brightness in corresponding parts of the uniocular images, for lustre is never given by combining colours of the same brightness (p. 526). Another is the presence of contours, for, as Brewster has proved (4), it is not produced by uniform fields of different brightness. The demonstration of the dependence of binocular lustre on differences of brightness finally disproves Dove's theory and calls for some modification in that of Wundt, for lustre cannot be dependent on difference of wave-length of the rays of light which stimulate the retinae, nor can it be based solely on subjective

[1] Cf. Watt, H. J., "Stereoscopy as a purely visual, bisystemic, integrative process," This *Journal*, 1916, VIII. 142 ff.

colour contrast, since red and green of equal brightness contrast quite as much as white and grey, but do not produce lustre.

Difference of brightness and abundance of similar contours in the uniocular images are not, however, sufficient, for it is impossible to produce lustre by holding before one eye a grey glass (see p. 548), or by combining in the stereoscope highly contoured surfaces of uniformly different brightness (see pp. 542–5). The arrangement of the light and dark areas in the uniocular images must be such that in the binocular field their combined brightness is apprehended as light reflected by a body so irregularly that clear mirror-images are not formed. In each of the uniocular images of a naturally lustrous object high lights and deep shadows are present in close proximity, their relative positions being dependent on the form of the object and the position of the source of illumination, and these variations of brightness are apprehended as imperfect mirroring. In this respect lustre resembles reflection: in the latter the mirror-image is seen clearly, generally much more clearly than the reflecting surface, but in the former, while the reflecting object is apprehended clearly, the mirror-image is so imperfect that its form is unrecognisable. This separation of object and mirror-image is obviously possible when small discs of uniform colour on a grey background are combined in the stereoscope, but impossible with highly contoured fields differing uniformly in brightness; hence the presence of lustre in the former case, and its absence in the latter.

Binocular lustre is in many respects analogous to the binocular perception of depth. Both are experiences due to the integration of uniocular images which differ in certain ways; the images which give lustre differ partly in brightness, those which give depth differ in visual form. They are affected similarly by the same conditions. Fixation of a point, especially on a stereoscopic slide, tends to destroy the perception of solidity; fixation similarly tends to destroy lustre (see p. 526). There are certain limits within which disparity of form must lie if things are to be seen singly and solid, but these limits may be considerably extended by eye-movement; there are also limits to the degree of difference of brightness which gives lustre, and these can be extended by eye-movement (see p. 526). In both cases the uniocular images must be possible right- and left-eye views of the same object.

The question now arises, if binocular lustre be due to the combination of images differing in brightness, how is uniocular lustre to be explained? How are we to explain the sheen of satin, silk, or polished metal that is observabl with uniocular vision? The sheen of these substances is most pronounced with binocular vision, but it also appears with uniocular; in the latter case it is most distinct when the observer, the object, or the source of illumination is moved; it is least pronounced with steady fixation. In a lustrous object high lights and deep shadows lie in close proximity, and, as the object or observer moves, their local relations change, what was bright becoming dull and *vice versa*. This seems to indicate that uniocular lustre may be produced by a succession of images differing in the local relations of their light and dark areas. In this respect it is again like the experience of visual depth, which can be produced uniocularly from a series of successive, disparate images[1].

Lustre, like depth, may be given by a single image and may even be represented pictorially. This lustrous effect must be explained in the same way as the depth that is given by a drawing in perspective. In both cases a complex experience is evoked by a part only of its conditions. Visual depth normally is due to the integration of a single or double series of successive disparate images one of which may mean the more complex experience: the linear and aerial perspective, and light and shade of a single image are of themselves sufficient to evoke the experience of depth. Similarly with lustre; it is normally produced from simultaneous or successive images differing partly in respect of brightness, but one of these may alone evoke the more complex experience.

[1] Cf. Watt, H. J., "Some problems of sensory integration," This *Journal*, 1910, III. 323–347.

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THE APPLICATION OF MENTAL TESTS TO CHILDREN OF VARIOUS AGES.

By M. E. BICKERSTETH.

(*From the Psycho-physical Laboratory, Oxford.*)

1. *The possibility of establishing by means of mental tests reliable age norms graded by years.*
2. *Subjects of this experiment.*
3. *Apparatus, procedure and results of the several tests.*
4. *Deductions:*
   (1) *The correlation of mental with physical age.*
   (2) *The correlation of mental with motor ability.*
   (3) *Evidence for independent mental capacities.*
   (4) *The correlation of memory with intelligence.*
   (5) *Individual difference during practice.*
   (6) *The mental differences between town and country children.*
   (7) *General conclusions on the value of mental tests to show the increase of capacity with age.*

1. THE POSSIBILITY OF ESTABLISHING BY MEANS OF MENTAL TESTS RELIABLE AGE NORMS GRADED BY YEARS.

The course of the development of the various mental capacities in children at different ages has come to be recognised as one of the problems awaiting determination by the methods of experimental psychology, and the value of mental tests as a means of diagnosing mental deficiency and of increasing our knowledge of mental heredity has been frequently pointed out in recent psychological literature[1].

[1] B. Hart, M.D., and C. Spearman, *Mental Tests of Dementia.* Cyril Burt, "The Measurement of Intelligence by the Binet Tests," *Eugenics Review*, April, July, 1914. W. McDougall, "Psychology in the Service of Eugenics," *Eugenics Review*, Jan. 1914. J. E. W. Wallin, *The Mental Health of the School-child*, 1914.

The lack of any standard of what constitutes mental efficiency has been pointed out more than once in the Reports of the Chief Medical Officer of Health to the Board of Education in dealing with the problem of mentally defective children, while Mr Burt[1] in reports to the London County Council and elsewhere draws attention to urgent practical need for standardised tests and age-norms in determining where the line between the defective and ordinary child is to be drawn. "The lists of children," he writes, "suggested as fit for a mentally defective school vary enormously from one school to another, both in the numbers which they contain and in the degrees of defect which they include. In some schools children nominated prove to be backward by about one year only, or in one subject only. In other schools children three or four classes behind the normal level in two or three subjects are still dealt with in the ordinary classes. These apparent inconsistencies are often explicable by peculiarities in the organisation or in the neighbourhood of the schools. But, for the most part, they are due to a divergence of opinion, or to an absence of opinion as to the amount and extent of mental insufficiency at each age which should arouse a suspicion of defect in the technical sense."

The need for some standard of mental efficiency at each age has been emphasised by Wallin, who writes: "The greatest possible obstacle to genuine progress in psycho-clinical work is lack of reliable age norms for the fundamental mental capacities; until these are supplied the work of routine inspection and consultation will be more or less blind and guideless. The practical value of such norms is probably greater than that of the corresponding anthropometric standards.... We now possess a set of fairly reliable physical development norms for various ages by means of which we are able to determine the physical status of the child of a given age, the need is for a set of psychic norms of development of various traits and capacities[2]."

Up to the present time the only systematic attempt to establish standards of mental efficiency for the various ages is that of Binet and Simon, whose scheme of tests was first published in *L'Année Psychologique* for 1905. Since then the tests have been used with more than 5000 children in England, America, Holland and Belgium

[1] Cyril Burt, *The Distribution and Relations of Educational Abilities*, 1916. Also *Report of the Council's Psychologist*, 1914. Cf. M. E. Bickersteth and C. Burt, "Some Results of Mental and Scholastic Tests," *Report of the Fourth Conference of Educational Associations*, p. 30.

[2] J. E. W. Wallin, *Mental Health of the School-child*, p. 183.

and the results have been exhaustively dealt with in recent literature[1].

The Binet-Simon scale is, in Mr Burt's phrase[2], one of "externally graded tests." It is true, they do not require any scientific apparatus, and can readily be applied by the ordinary teacher. But as a method of diagnosing innate intelligence they have been much criticised on the ground that they test acquired capacity rather than intelligence—while the complete absence of system makes it impossible to compare results for the tests applied are not the same at each age. And it has been urged that "for all exact and scientific purposes, the principle of external gradation—of constructing a scale out of a list of heterogeneous tests, different at different ages, arranged in order only of relative difficulty—will have to be given up." Instead, we need experiments carrying the same test through every age, giving age-norms throughout in terms of the same test.

More recently an attempt has been made by Squire[3] in America to find norms of performance in different mental capacities for children of each age from 6–13, but the number tested at each age, 10, is too small for the results to be more than suggestive. And in this country age-norms have been attempted for various scholastic tests (arithmetic, reading, etc.) and a few psychological tests, by such investigators as Ballard, Burt, Moore and Green.

Aim and History of this Research.

In order to contribute to the question of the course of the development of mental capacity in children of school age the following experiment was commenced in January 1914 at the suggestion and under the direction of Dr McDougall. The tests described were given by the writer at the Oxford Higher Elementary Schools for Girls[4] on four days

[1] Cyril Burt, "The Measurement of Intelligence by the Binet Tests," *Eugenics Review*, April, July, 1914. Rogers and McIntyre, This *Journal*, VII. Oct. 1914, part 3. Huey, "Present Status of the Binet Tests," *Psychological Bulletin*, 1912. Laurence, *Psychological Clinic*, 1911, v. 7. Terman, *Ibid.* [2] *Eugenics Review, loc. cit.*

[3] C. R. Squire, Ph.D., "Graded Mental Tests," *Journal of Educational Psychology*, III. Sept. 1912.

[4] The main part of the experiment took place at the Central Girls' School, but as there were not quite 400 girls attending the school, the numbers for certain age groups were made up from Cowley St John's and the Convent School in Winchester Road. These are all Higher Elementary Schools where the children pay a small fee, and the groups made up from the three schools are homogeneous.

At the Central School the writer was given opportunities of taking various classes for ordinary school work during the whole period of the experiment, and as each of the children a week for five terms, a room being given up to the experiment where the children could be tested individually free from interruption. Care was taken that the hour of day and the order in which the various tests were given should be the same in the case of each subject—and as a rule only one test was given on each occasion. The motor tests were given first—the 'Plunger' proving a very good introduction to the series of experiments as the nature of the task is quickly grasped by the subject and it was a very popular test at all ages.

The Group tests were given by the writer in class to each form in turn allowing a week's interval between the first and second application, —those like the individual tests involve much concentration of attention and genuine effort on the part of the children who, at the same time, thoroughly enjoyed the variation the experiment introduced into the ordinary school routine.

The Group tests were afterwards repeated for purposes of comparison in a large number of elementary schools in various parts of the country and in the Worcester and Ripon High Schools for Girls and at the Kirby Secondary School at Middlesbrough—the results for these schools are given in the summaries of age-norms in Appendix I.

Principles governing the Selection of Tests.

The tests used in the experiment were selected so as to cover as far as possible the whole field of mental ability, and at the same time be such as could be applied to children of widely different ages; and further testing only natural capacity they should give results wholly uninfluenced by the extent of the child's acquired skill or knowledge. The elimination of age, sex and social difference in each group makes the norms of performance for the various tests strictly comparable.

Precautions taken to ensure Uniformity of Conditions.

Every precaution was taken to ensure uniformity of conditions when conducting the experiment—and this was the more easy because a preliminary trial of the apparatus was undertaken when every test was given twice to a group of about 30 boys of 12 to 13 in one of the Oxford schools.

was tested individually on about a dozen occasions the results are no hasty or superficial estimate of the subject's mental capacity. The numbers for three age groups, 5, 6 and 7, were augmented from the results of the same tests at several of the Oxford Infant Schools —only children who would go on to a Higher Elementary School being included in the averages.

In this way details in the setting of the tests were worked out and possible sources of error eliminated, and the ensuing experiments necessarily gained much from this preliminary standardisation of method. The instructions used were not reduced to a set formula, as has often been insisted on, in order that all individuals tested, receiving the same instructions, may be treated alike. As Woodworth[1] points out, a set formula is no guarantee that the subjects are treated alike, for some may not understand the formula, and especially when the tests are given to children of different ages. The important point is that the subject shall clearly understand what he is to do and that the directions should be as simple as possible, the emphasis being made on what the task requires in each case, *e.g.* speed in the 'Plunger,' attention to the connections between the Words in the Related Memory Test, and so on. A sufficient amount of practice should always be allowed, but as this necessarily varies with each test, the degree is indicated in the description of the several experiments.

2. SUBJECTS OF THIS EXPERIMENT.

Group I.	550 girls, Oxford Higher Elementary Schools.
Group II.	300 girls, Secondary Schools.
Group III.	150 boys and girls, Oxfordshire Village Schools.
Group IV.	300 boys and girls, Ripon Elementary Schools.
Group V.	600 boys and girls, Leeds Elementary Schools.
Group VI.	600 boys and girls, Yorkshire Dales Elementary Schools.

3. APPARATUS, PROCEDURE AND RESULTS OF THE SEVERAL TESTS.

List of Tests.

1.	Power of sustained effort (Tapping)	Motor tests.
2.	Precision and speed of movement (Plunger)	
3.	Alphabet test	Tests of Discriminative Selection.
4.	Number ,,	
5.	Combined ,,	
6.	Memory for narrative	Memory tests.
7.	Memory for related words	
8	Memory for unrelated words	
9.	Spot pattern test—Test of analytic and synthetic apperception.	
10.	Repeated discriminative selection	Attention tests.
11.	Discs and sentences	
12.	Completion of analogies—Reasoning test.	

[1] Wells and Woodworth, "Association Tests," *Psy. Monographs*, December 1911.

Motor Tests.

1. Power of Sustained Effort. 2. Precision and Speed of Movement.

1. *Power of Sustained Effort (Tapping).*

The apparatus for this test was suggested by Dr McDougall and measures the subject's capacity for making a sustained effort.

The apparatus is very simple. Four counters, of the kind used in registering the number of telephone calls are mounted on a small wooden base. The subject taps with a small instrument as quickly as possible for 60 seconds, working on each counter in turn for 15 seconds and moving to the next without a pause as the word 'change' is given by the experimenter, who times the performance with a stop-watch. A few seconds practice is allowed, but this must not be prolonged or the subject tires before the test begins.

The extent of the effort made by the subject is measured by subtracting the number of taps in the last 30 seconds from the number made in the first 30, and given as a percentage of the total score.

Results. Table I gives the result of the test for 527 girls at the Oxford schools. The averages are taken from the amalgamation of tests I and II[1].

Table I. *Power of Sustained Effort (Tapping).*

Oxford Higher Elementary Girls.

Age	Number tested	Test I & II. Av. taps in 60 seconds	M.V.	Test I & II. Av. fall off	M.V.	Test I & II. Av. of per cent. fall off	Test II. Range of individuals	Av. gain in speed in no. of taps	Coefficient of reliability	P.E.
15·5	17	297·7	30·7	16·7	8·7	5·6 %	343–246	8·8	·56	·11
14·5	47	301·4	29·9	17·7	8·3	5·8	360–212	4·0	·69	·06
13·5	69	288·5	20·1	20·9	·10·1	7·2	344–148	11·4	·66	·05
12·5	56	281·7	19·5	19·8	7·5	7·0	354–203	8·0	·60	·06
11·5	50	266·2	23·5	20·6	11·8	9·2	349–133	8·8	·47	·08
10·5	57	259·2	22·5	24·1	9·3	9·2	331–142	9·7	·69	·06
9·5	47	240·7	20·3	19·3	6·4	8·0	299–179	13·7	·65	·07
8·5	59	209·8	25·3	21·4	8·5	10·1	299–133	2·3	·69	·07
7·5	45	199·3	24·8	23·3	7·2	11·1	272–130	−3·0	·75	·04
6·5	40	185·2	18·8	20·3	12·3	10·9	230–109	3·8	·35	·13
5·5	40	172·9	18·2	20·9	8·7	12·1	225–113	3·6	·31	·13

[1] The coefficient of reliability and the correlation between the different tests was worked out by the Footrule of Spearman. (C. Spearman, This *Journal*, II. Part I. July 1906, "Footrule for Measuring Correlation ")

There is a steady increase of motor power with age, the child of 5 taps on an average 172·9 in 60 seconds, the speed rising rapidly but unevenly to 301·4 taps at 14 years. At 15 there is a slight fall off; but the number tested, 17, is so small that the figures may not have much significance. The greatest increase of speed comes at 9 years, when the average has risen from 209·8 at 8 to 240·7 a difference of 30·9 after an average increase of only 12 taps from 5–8. The smallest gain is at 13, when only 6·8 taps are added to the average of the preceding age[1]. The actual drop in score between the first and last 30 seconds does not vary much with age, but the percentage of fall off in relation to the total score is interesting showing a gradual increase in the power of sustaining an effort, but with less regular age differences than for the speed of tapping. The child of 7 is less capable of sustaining an effort than at 6, a result which is in agreement with the fact that a child of 7 is more easily fatigued physically than at 6 or 8.

The marked resistance to fatigue at age 9, when the fall off is only 8 per cent., corresponds with the great increase in the speed of tapping at that age.

These results are in agreement with those of Gilbert and Bolton, who found a steady increase of motor ability with age.

2. *Precision and Speed of Movement* (*the Plunger*).

The apparatus for this test, devised by Dr McDougall, consists of a heavy brass plate 23 c.c. square, mounted on a wooden base, containing 24 raised sockets 2 cm. high arranged in a circle 9 mm. in diameter.

The test consists in inserting a small steel instrument, mounted in a wooden handle, into each socket in turn as rapidly as possible, the time to make a complete revolution being timed with a stop-watch, and varying from 12 to 30 seconds according to the age or skill of the subject.

A preliminary trial is allowed before the experiment begins, the subject going once round, or in the case of young children half round, as for them an equal amount of practice causes fatigue to the extent of affecting the test. A normal subject's rate, however, becomes quicker at each revolution reaching the maximum at the third (the fourth

[1] The effect of practice on Tapping appears to be very slight. In test 2 the average shows a gain of only 3·6 and 3·8 taps at 5 and 6 years, while at 7 years—3. The improvement was greatest at 9 years, a gain of 13·7 on the average for the first test. The reliability of the test varies considerably with age, it is only ·31 and ·35 at 5 and 6 years rising to ·75 at 7 and falling to ·47 at 11.

counting the practice attempt), after which as a rule fatigue sets in and there is a considerable fall off in speed.

The subject makes three attempts after the preliminary trial, resting a second or two between each, and the best attempt—which is usually the last—is taken as a measure of his capacity.

The test was repeated on three different occasions with an interval of about a week between each, allowing a practice and three subsequent attempts as before. The test was enjoyed by the children, who seldom failed to remember their previous record.

Results. Table II gives the average, mean variation, and best and worst individuals score for 480 girls tested at the Oxford schools.

TABLE II. *Precision and Speed of Movement (Plunger).*

Oxford Higher Elementary School Girls.

(Averages = number of seconds for one revolution.)

Age	Number tested	Tests I, II, III Average	M.V.	Test III Range of indivs.	Av. gain with practice
		secs.		secs.	secs.
14·5	46	$17\frac{2}{5}$	1·2	$13\frac{4}{5}$–$22\frac{1}{5}$	2
13·5	70	$17\frac{1}{5}$	1·3	$11\frac{4}{5}$–$23\frac{4}{5}$	$1\frac{1}{5}$
12·5	66	17	1·2	12 –$19\frac{1}{5}$	2
11·5	60	18	1·4	$14\frac{1}{5}$–22	$2\frac{3}{5}$
10·5	44	$19\frac{3}{5}$	1·5	$14\frac{2}{5}$–$22\frac{1}{5}$	$2\frac{3}{5}$
9·5	50	$19\frac{3}{5}$	1·7	15 –$23\frac{4}{5}$	2
8·5	40	$21\frac{1}{5}$	1·8	$15\frac{2}{5}$–29	$2\frac{1}{5}$
7·5	30	$24\frac{3}{5}$	2·6	$17\frac{1}{5}$–$27\frac{1}{5}$	$2\frac{3}{5}$
6·5	39	$24\frac{3}{5}$	2·0	$17\frac{1}{5}$–28	$2\frac{3}{5}$
5·5	35	$28\frac{2}{5}$	3·2	22 –33	$1\frac{3}{5}$

The averages in the table are taken from the amalgamation of the three separate experiments, the amount of gain in speed as the result of practice is small, and does not vary much with age. Some subjects showed no improvement during the whole experiment—this was the case for 9 girls at 11 years while at 14 one-third of the number tested gained under four-fifths of a second. There is a gradual increase in precision and speed of movement with age, from the average of $28\frac{2}{5}$ seconds taken to perform the test at 5 years to 17 seconds at 12 when the maximum is reached, and the curve begins to fall. There is a marked increase in speed at 6 years none between 6 and 7 or between 9 and 10. The best individual score—$11\frac{4}{5}$ seconds at 13—is the shortest time in which the test has been performed.

The reliability of the test is high, ·80 at age 7 and ·74 at 12. Whipple points out that a high rate of tapping does not always go with high speed in other motor tests, and in this experiment the correlation between the Tapping test and Plunger is $\cdot 13 \pm \cdot 10$ at 13, $\cdot 17 \pm \cdot 10$ at 12, $\cdot 22 \pm \cdot 11$ at 11 and $\cdot 37 \pm \cdot 09$ at 10, the correlation decreasing with age.

Tests of Discriminative Selection.

1. Alphabet test. 2. Number test. 3. Combined Alphabet and Number.

Papers, each containing a square 4 by 4 inches in which the whole alphabet and the numbers 1 to 26, are printed in large type, are used in these experiments. This form of the Alphabet test, which differs slightly from that found to correlate so highly with the intelligence of Oxford school children by Mr Burt[1], would enable the test to be carried out with a group of children simultaneously in class.

The task of the subject in the Alphabet and Number test is to cross out the letters A to Z or figures 1 to 26 in order as quickly as possible, the letters and figures in the square being arranged so as to avoid any natural sequence in their relative positions[2].

Before giving each test, a full explanation and black-board demonstration was given to the class; the children were urged to avoid passing over any of the letters (or figures in the Number test) but otherwise to work as quickly as possible[3].

The Alphabet test was given first, the papers were distributed face downwards, the subjects instructed to write their name and age on the back, and at a given signal to turn the paper over and proceed to cross out the letters in order as quickly as possible till the order was given to stop. The papers were then collected by the experimenter, a black-board example of the Number test given, and carried out in the same way. In the Combined test, which was given after the other two, the task of the subject is to cross out the letters and figures alternately.

[1] Cyril Burt, "Experimental Tests of General Intelligence," This *Journal*, III. 1 and 2, pp. 138–141.

[2] The tests, which all require to be performed at the subject's maximum speed, involve a steady effort of attention. They can be used either for individual or group work. In the former case the subject crosses out the whole sequence of letters or figures, the time required to complete the task being taken with a stop-watch. When used as a group test, as in this experiment, the subjects are allowed 50 seconds in the case of the Alphabet and Number, and $2\frac{1}{2}$ minutes in the Combined test, and their ability tested by the number crossed out, the time allowed being the same for each age.

[3] As the time for the experiment—50 seconds—was too short for any but exceptional children to complete the task, any attempt to cross out the letters or figures without finding them in order was easily detected, but this was only the case about half a dozen times out of many hundred children who were given the test.

A, 1: B, 2: C, 3: etc.; this being considerably harder a longer time ($2\frac{1}{2}$ minutes) is allowed.

The arrangement of letters and numbers is different on each paper, so subjects with good memories have no advantage over the rest of the group. In marking the papers every letter or figure erased correctly = 1 mark,—2 for any letter or figure passed over.

Results. The following tables give the average for each age from 5 to 15 for the girls tested at the Oxford schools.

TABLE III. *Alphabet Test.*
Oxford Higher Elementary Girls.

Age	Number	Av. I	Av. II	M.V.	Range of indivs. Test I	Range of indivs. Test II
15·5	40	11·2	14·7	3·9	26–5	26–5
14·5	58	11·8	13·6	3·6	26–4	26–5
13·5	55	11·2	11·3	3·7	26–3	26–4
12·5	50	11·6	11·2	3·8	26–2	26–6
11·5	58	9·4	11·9	3·8	21–3	22–3
10·5	61	7·3	11·7	3·2	15–4	20–2
9·5	54	6·8	9·3	2·6	18–2	23–1
8·5	40	4·9	6·8	2·6	12–1	15–1
7·5	40	3·8	6·9	3·6	10–1	11–1
6·5	33	3·0	3·0	1·5	8–0	9–1
5·5	31	2·3	3·4	1·0	5–0	6–1

TABLE IV. *Number Test.*
Oxford Higher Elementary Girls.

Age	Number	Av. I	Av. II	M.V.
15·5	40	15·3	17·2	4·4
14·5	60	12·3	15·4	4·3
13·5	60	11·6	13·4	4·0
12·5	53	10·5	11·0	3·9
11·5	62	9·3	12·3	3·7
10·5	69	8·4	9·4	2·9
9·5	40	6·3	9·5	2·3
8·5	40	5·2	7·7	2·3
7·5	40	4·0	5·8	2·3
6·5	40	2·9	4·6	1·4
5·5	40	2·1	2·7	1·3

TABLE V. *Combined Letter and Number Test.*
Oxford Higher Elementary Girls.

Age	Number	Av. I	Av. II
15·5	40	30·2	31·5
14·5	41	26·0	30·5
13·5	57	23·2	27·2
12·5	52	23·5	27·6
11·5	60	19·0	22·4
10·5	60	16·2	19·6
9·5	40	15·0	18·5
8·5	31	12·1	14·9

The difference between the averages for the successive tests is very small, many of the children crossed out the same number of letters and figures each time. With the exception of age 6 in the Alphabet test, the percentage of improvement calculated on the subject's gain on his first score is greatest during the years from 5 to 9 with a marked decrease at 12 and 13, the older girls probably grasping the nature of the task better and doing themselves more justice than the younger children in the first test.

The Combined test was found much the most difficult by the children, who were evidently handicapped in finding the letters and numbers by having to keep the sequence of both in mind simultaneously. A comparison between the above results and those of the test given under similar conditions by the writer at Somerville College and by Miss May Smith at Cherwell Hall shows that the difference in the power of discriminative selection between children and adults increases considerably when the test is complicated in this manner[1].

The reliability of the alphabet test appears to be moderately low, as was found by Mr Burt[2]. The reliability of the modified form differs but little from that obtained with his form. The reliability of the tests varies with age. The Combined test seems the most satisfactory, ·78 at 13 and ·84 at 11.

TABLE VI. *Coefficient of Reliability.*

Age	Alphabet	Number	Combined
13	$\cdot59 \pm \cdot07$	$\cdot37 \pm \cdot08$	$\cdot78 \pm \cdot04$
12	$\cdot44 \pm \cdot08$		
11	$\cdot54 \pm \cdot07$	$\cdot62 \pm \cdot06$	$\cdot84 \pm \cdot04$
10	$\cdot43 \pm \cdot08$		
9	$\cdot61 \pm \cdot07$	$\cdot50 \pm \cdot09$	

MEMORY TESTS.

1. Memory for Narrative. 2. Memory for Related Words.
3. Memory for Pairs of Words.

1. *Memory for Narrative.*

This test is one of logical memory demanding recall of ideas, not an exact verbal reproduction of the original.

[1]

	Number tested	Alphabet Av. I	Alphabet Av. II	Number Av. I	Number Av. II	Combined Av. I
Somerville students	24	13·4	13·5	16·7	22·1	38·8
Cherwell Hall graduates	50	16·1	—	18·7	—	50·0

[2] *Loc. cit.* p. 139.

The experiment was given as a group test, the story read to the class once and reproduced in writing immediately afterwards; 15 to 20 minutes being allowed according to the age of the group. The reliability of the test is low and decreases with age.

The averages were taken from the extract printed below and show a gradual gain in logical memory with age as was found by Binet—there is a slight loss at 12 years and at 13 the maximum of 23·5 ideas recalled is reached.

Test 1. The Priest. (31 marks including Title.)

Tang, | when his | wife | died, | left home | and became a priest | of a particular order. | Some years afterwards | he returned | dressed in the garb | of his order | and carrying his praying mat | over his shoulder; | and after staying | one night | he wanted to go away again. | His friends however | would not give him back | his cassock and | staff, | so at length | he pretended | to take a stroll | outside the village | and when there | his clothes and other belongings | came flying | out of the house | after him | and he got safely away.

TABLE VII. *Memory for Narrative.*

Averages = number of ideas recalled out of a possible 31.

Oxford Higher Elementary Girls.

Results.

Age	Number	Average	M.V.	Range of indivs
15·5	39	23·3	3·9	31–13
14·5	40	22·7	4·0	30–10
13·5	54	23·5	2·9	31–15
12·5	48	21·4	2·7	29–13
11·5	51	22·1	3·8	29–11
10·5	60	18·6	3·6	28– 8
9·5	49	16·9	3·6	27– 5

2. *Memory for Related Words.*

The test requires the subject to remember a series of words when, as a contrast to the material used in tests of immediate memory, the recall of each word is favoured by the relation in which it stands to the word that preceded it.

The lists of 21 words given below were used in this experiment:

Test I	Test II	Test III	Test IV
Bird	*Top*	*Cat*	*Pen*
Nest	Spin	Mouse	Ink
Bush	Spider	Trap	Black
Garden	Fly	Cheese	Dog
House	Run	Yellow	Bark
Roof	Cricket	Buttercup	Tree
Chimney	Bat	Meadow	Oak
Fire	Night	Hay	Ash
Heat	Cold	Stack	Ashes
Boil	Marble	Thatch	Fire
Milk	Round	Cottage	Poker
Cow	Ring	Village	Tongs
Grass	Light	Green	Sugar
Green	Moon	Lawn	Sweet
Colour	Man	Tennis	Honey
Paint	Policeman	Net	Bee
Dry	Beat	Fisherman	Sting
Dust	Stick	Sea	Nettle
March	Glue	Blind	Ditch
Hare	Gum	Eye	Water
Red	Tooth	Needle	Cart

The nature of the test with examples was explained to the subject, who was told to notice the connection between each word and that following it, and to try to repeat the series in order on being reminded of the first word.

The lists were read at the rate of one word in two seconds, or still more slowly in the case of young subjects, who were told to say 'yes' as soon as they noticed the connections, this proved a very necessary precaution as children of 9 and 10 sometimes took 3 or 4 seconds to see the easy relationships between the words in the lists used.

The first word was then given, and the subject attempted to repeat the series in order, being allowed 10 seconds in which to recall each word, words not remembered in that time being given by the experimenter. If the subject passed over a word but gave it on being told he had missed one out, it was counted as half a mistake. The test was given several times to the older subjects but only once to the 9 and 10 year old groups, so the averages are taken from Test I, Bird, Nest, etc.

The second test, Top, Spin, etc., was found considerably harder by the children, probably owing to the larger number of words it contains used in a double sense, this difference in difficulty experienced by the subject in lists of the same length makes it important that for all purposes of comparison the actual list used as well as its length shall be the same.

Table VIII gives the number of words correctly recalled at each age from 9 to 14 out of a possible 20 (the first word not counted in scoring).

TABLE VIII. *Memory for Related Words.*

List of 21 words. Oxford Higher Elementary Girls.

Age	Number	Average	M.V.	Range of indivs.
14·5	31	15·3	2·5	20–7
13·5	41	14·6	2·6	20–9
12·5	45	13·8	3·4	20–7
11·5	41	13·9	2·6	20–7
10·5	49	13·8	2·9	20–6
9·5	37	13·9	2·2	20–6

Results. There is practically no change in the curve for memory in this experiment from 9 to 12, but at 13, 14·6 words are recalled, and at 14, 15·3. An adult has little difficulty in remembering a list of 20 words, so there would probably be no fall off after 14 as in the case in some Memory tests. The results seem to depend largely on an intelligent grasp of the relations between the words; and clever children always showed a marked superiority in this test; it necessitates a steady effort of attention on the part of the subject, but that it is memory and not attention which is involved seems clear from the low correlation of the test with the Dotting.

TABLE IX. *Correlation between the test of Memory for Related Words and the Dotting Test.*

Oxford Higher Elementary Girls.

Age	
14	·32±·13
13	·35±·09
12	·03±·10
11	–·11±·12
10	·31±·11
9	·00±·15

Memory for (A) *Related Pairs of Words.* (B) *Unrelated Pairs of Words.*

The aim of the test was to compare a subject's reproduction of a list of related and of unrelated words given under identical conditions. The following series of words was used, (A) in which there is a connection between each pair of words, and (B) in which there is no obvious connection.

	(A) Test I		(A) Test II	
1.	Book	Shelf	Garden	Rose
2.	Garden	Spade	Blotting-paper	Pink
3.	Letter	Stamp	Sweep	Chimney
4.	Watch	Time	Letter	Word
5.	Rain	Umbrella	Fire	Coal
6.	Spider	Spin	Cow	Horn
7.	Church	Tower	Stones	Heap
8.	Negro	Black	Bark	Tree
9.	Orange	Yellow	Hen	Chicken
10.	Sea	Fish	Miller	Flour
11.	Fire	Heat	Donkey	Tail
12.	Snow	White	Acorn	Oak
13.	Pen	Ink	Basket	Handle
14.	Wash	Soap	Ivy	Wall
15.	Rabbit	Burrow	Picture	Frame
16.	Cloud	Thunder	Bird	Feather
17.	Angry	Frown	Card	Pack
18.	Snail	Shell	Leaf	Green
19.	Oil	Lamp	Marmalade	Orange
20.	Pond	Duck	Rook	Black

	(B) Test I		(B) Test II	
1.	Slate	Hare	Bottle	Lead
2.	Wheel	Soot	Fence	Ink
3.	Wren	Screw	Wing	Bun
4.	Desk	Spark	Lead	Spider
5.	Paper	Tree	Pig	Envelope
6.	Poker	Duck	Crowd	Weed
7.	Gruel	Fire	Stool	Tree
8.	Water	Shelf	Cushion	Hoof
9.	Cotton	Matches	Ball	Nail
10.	Reins	Potatoe	Fish	Collar
11.	Ink	Poppy	Bell	Skin
12.	Wool	Moon	Coal	Mouse
13.	Gum	Watch	Nettle	Cork
14.	Shrub	Porter	Soap	Pen
15.	Star	Glove	Grass	Wine
16.	Lamp	Pin	Rent	Shark
17.	Hen	Gas	Bone	Hail
18.	Net	Tower	Bird	Muff
19.	Roof	Basket	Wick	City
20.	Well	Verb	Cricket	Map

The experiment was given as a group test; after a short explanation the first list of twenty pairs of Related Words was read to the class at the rate of two words in four seconds, then the first word of each pair was dictated by the experimenter the children writing down the related word in blanks provided for the purpose. Ten seconds were allowed for this, then the first word of the next pair dictated and so on. The children found the test easy, the average never falls below 17 out of a possible 20 at any age from 10 to 16. The test, like the Individual Test for Related Words, does not measure the Memory span, and seems to be within certain limits uninfluenced by the age of the subject.

The test of Memory for Unrelated Words was then given in exactly the same way, the whole experiment taking about a quarter of an hour. The results bring out very clearly the part played by meaning in True Memory as distinct from Memory which is conditioned by motor habit: while an absence of correlation between the two kinds of memory tests is further evidence that memorising involves two fundamentally different factors.

TABLE X. *Memory for Related and Unrelated Words.*

Oxford Higher Elementary Girls.

	Averages of number recalled out of a possible 20	
	Related	Unrelated
Age	Av.	Av.
16·5	17·6	6·6
15·5	17·7	6·0
14·5	18·2	8·0
13·5	17·3	5·7
12·5	17·3	5·0
11·5	17·7	3·7
10·5	17·6	4·3

TEST OF ANALYTIC AND SYNTHETIC APPERCEPTION (SPOT PATTERN TEST).

This test has been described at length and illustrated by Mr Burt in his account of his experiments at Oxford[1], and is now included by Whipple in the second edition of his Manual (Test 25 A). Its object is to find out how correctly an irregular pattern may be reproduced.

In these experiments each card was exposed three times instead of five before the subject attempted to reproduce the pattern on squared paper corresponding with that on which the pattern is plotted.

The test was only given twice at age 14, so the averages are in each case from Test I.

[1] *Loc. cit.* p. 150, and Fig. 1, p. 151.

TABLE XI. *Spot Pattern Test.*

Oxford Higher Elementary Girls.

Average = number of spots correctly reproduced out of 33.

Age	Number	Average	M.V.	Range of individuals
15	22	16·4	3·6	20–13
14	31	15·8	2·8	21– 9
13	47	14·3	2·6	21– 9
12	38	13·6	2·9	23– 7
11	43	13·1	2·0	21– 6
10	50	12·5	2·3	19– 7
9	40	12·0	2·8	23– 6

Results. There is very small but steady improvement with age in this test, the difference between 9 and 15 years being only 4·4 more spots correctly reproduced out of 33, while the gain at each age only exceeds one at 14 years.

The test appears to be affected much more by the intelligence than the age of the child, as is shown by the close correspondence between the range of the individuals at each age. Mr Burt found a correlation with intelligence of ·76 at the Oxford Preparatory and ·75 at the Oxford Elementary School[1]. The *reliability* of the test has always been found to be rather low—·50 and ·55 by Mr Burt. In this case it was only ·50 ± ·09 (age 14) and decreased with younger subjects. In the 14 year old group one or two clever children improved considerably, but in the majority of cases there was little advance on the first attempt. Young children were often disturbed by the working of the shutter, starting each time the illuminated pattern of spots was exposed, and the test was not given to any number of subjects under 9 on this account, though one or two children from 5 years upwards made very fair attempts.

TESTS OF ATTENTION.

1. Test of Sustained Voluntary Attention, Dotting.
2. Test of Divided Attention, Discs and Sentences.

1. *Test of Sustained Voluntary Attention, Dotting.*

The apparatus used in this experiment was the Spiral Dotting Machine, this being Dr Schuster's modification of the original Tape Dotting Machine devised by Dr McDougall to test and record the power of sustained voluntary attention. The form used by Mr Burt was the original tape machine[2]. "The essential part of the Spiral Dotting Machine is that the irregular row of small circles which is printed along the tape of the other machine is arranged spirally on a

[1] *Loc. cit.* pp. 94, 177. [2] *Loc. cit.* p. 153.

paper disc 13 inches in diameter. The paper disc is mounted on a nickel plate which is made to rotate by clockwork at a constant rate" (one revolution in 20 seconds in the case of this experiment). "A cover is supported above the plate in which a slot one inch wide is cut, in such a position and of such a length that as the disc rotates every point in the spiral row of circles presents itself to view through the slot. The subject rests his wrist on the cover and aims at the circles in the same way as when using the tape machine. He starts at the inner end of the spiral and works his way outwards over it as its whole length presents itself point by point through the slot. This produces the effect that he has to aim at the circles at a gradually increasing rate.

"The point at which the rate of aiming becomes too rapid for the subject and he breaks down is taken as a measure of his capacity for sustained voluntary attention, and is shown clearly by the marks made on the disc, and can be read off by the operator at any time after the test is completed." The test is very quick, taking only a few minutes, including all necessary explanations and instructions.

No practice was given before the test except in the case of children of 5 and 6 who were not found to be able to carry out the instructions properly without a preliminary trial. The break-down point is measured when the subject makes 3 misses in 10 consecutive circles, circles completely missed counting one, and circles aimed at but not hit half an error.

Children with defective eyesight were not tested.

The following table gives the average number of circles crossed out at each age for 485 girls tested at the Oxford Higher Elementary Schools. The averages are calculated from the amalgamation of the repeated tests.

TABLE XII. *Test of Sustained Attention (Dotting).*

Oxford Higher Elementary Girls.

Age	Number of subjects	Average no. of dots crossed out	M.V.	Range of indivs. Test II	Coefficient of reliability	P.E.
5·5	36	15	4·3	31– 6	·72	·06
6·5	46	27	11·8	55– 13	·82	·03
7·5	33	37	11·8	74– 13	·55	·09
8·5	45	54	12·7	124– 22	·72	·05
9·5	43	67	14·6	122– 16	·65	·06
10·5	51	71	21·1	162– 26	·70	·04
11·5	47	96	26·8	220– 46	·78	·04
12·5	65	112	24·5	188– 40	·80	·03
13·5	57	118	25·7	210– 50	·66	·05
14·5	44	139	28·2	233– 68	·83	·03
15·5	18	142	27·3	238–110	—	—

There is a steady increase of the power of sustaining voluntary attention with age. At 5 years an average of 15 circles are crossed out, at 15, 142. There appear to be clearly marked changes in the growth of attention at age 8, 11, and 14. From 10 to 12 the power of attention is growing rapidly, but the greatest increase is at 11 years when the average rises from 71 circles crossed out at age 10 to 96; the best and worst individual scores at 11 years—which were both considerably better than those of any child of either 9 or 12—correspond with the marked gain in the capacity of attention at this age. The least increase is between 9–10 and 14–15, but the number tested at age 15 is too small for the figures to be more than suggestive.

There was very little gain as the result of practice on the second application of the test. Of children under 12, 25 per cent. showed no improvement in Test II, and over 12, 27 per cent. no improvement. The reliability of the test is high, but varies somewhat at each age, it is higher at age 6 than at 7 and there is a drop in the reliability again at 9 and at 13.

There is a correlation which increases with age between the Dotting and Tapping test. This seems in harmony with Mr Burt's suggestion that these and other similar tests are in part affected by some specific factor—such as motor dexterity. He found tapping and dotting to correlate to the extent of ·55 and ·48 at the two Oxford schools. But the correlation between the Dotting test and two tests which showed high correlations with Intelligence—the Analogies and Memory for Related Words—is uniformly low, or inverse at some ages. The correlations are given below.

TABLE XIII.

Oxford Higher Elementary Girls.

Dotting and Reasoning. Coefficient of correlation

Age	
15, 14	$-·17 \pm ·12$
13, 12	$·09 \pm ·09$
11, 10	$·11 \pm ·09$
9	$·23 \pm ·06$

Dotting and Memory for Related Words. Coefficient of correlation

Age	
14	$·32 \pm ·13$
13	$·35 \pm ·09$
12	$·03 \pm ·10$
11	$-·11 \pm ·12$
10	$·31 \pm ·11$
9	$·00 \pm ·15$

2. *Test of Divided Attention* (*Discs and Sentences*).

This test has been recently devised by Dr McDougall for the measurement of the capacity for divided attention. The apparatus consists of a wooden stand 18 by 6 inches into which are screwed three brass rods 12 inches in length; on the central rod are threaded 15 black and 15 white discs of wood. The test consists of two distinct tasks. In the first the subject has to separate the black and white discs, lifting each in turn from the central rod and dropping it on to one of the side rods, placing all the white discs on one side and the black on the other, using one hand only, and at the same time keeping his eye fixed on a given spot on a card which is held in position at the level of his eye by means of a small reading desk fixed to the back of the stand. The subject has to perform the task without watching his hands, but each disc is brought within the field of vision as it is lifted from the central rod, so it can be placed to the right or left according to its colour without error. The time taken to complete the task is taken with a stop-watch, and varies from 40 to 70 or more seconds according to the age or ability of the subject, who is instructed to work as quickly as possible.

No practice is allowed before beginning the test, the experimenter quickly demonstrating the nature of the task.

The subject is told not to stop if he drops a disc or places one on the wrong side, one second being added to his score for each disc that he fails to place correctly.

The second task is to repeat the operation and at the same time to read aloud a series of disconnected sentences in each of which one or more words are missing[1]. The subject has thus to divide his attention between the task of separating the discs and filling in the blanks correctly; the discs are arranged irregularly so that the correct placing of each one gives an opportunity of choice and involves an act of judgement, and the task never becomes automatic. The time is again taken with a stop-watch, and from it subtracted the number of seconds that was required to separate the discs alone, the difference is then taken as a measure of the subject's capacity for divided attention.

[1] This is a modification of the original test, in which a series of 30 pairs of circles was used, drawn so that one in each pair, *A*, *B*; *C*, *D*; etc. was almost imperceptibly larger than the other. The subject's task was to judge whether *A* or *B*, *C* or *D*, in each case was the larger, but it was found that in the school where the experiment was tested that the boys in many cases called out *A* or *B* indiscriminately and so were able to perform the double task as quickly as when merely separating the discs.

The task is considered finished as soon as the last disc is dropped on its rod, the number of sentences actually finished in the same time is ignored; older subjects read through nearly the whole 22 in the time required to separate the discs (in rare cases finishing the sentences first), but children of 8 and 9 do not as a rule read more than half-way through the list. Slight errors in the choice of words in filling the blanks are passed over, the experimenter merely insisting that the words supplied should make sense before the subject passes to the next sentence.

The following is one of the series of sentences used in the experiment. For children of 7 and 8 special editions in large type were used, but the actual sentences were the same for each age.

Completion of Sentences.

1. Red Riding Hood met the in the wood.
2. The man was the railings green.
3. Joan went into the to pick a rose.
4. The was in its cot fast asleep.
5. The roads were very bad when the melted.
6. The postman's was heard at the door.
7. Holly has berries and green .
8. The two dogs began to and their owners could not separate them.
9. The of the lions was heard all night when the Wild Show came to the town.
10. The rooks were very busy in the elm repairing their last year's .
11. The plough-man was the fields with three .
12. M is the letter of the alphabet.
13. The fell all and in the morning everything was white.
14. The redbreast built its in the hollow tree.
15. The longest are in summer and the shortest .
16. The sun never in summer in the far north.
17. The was watching the hole to a mouse.
18. The was very hot and the chestnuts were soon .
19. If you take away five from eleven and add you have twelve.
20. We burn coal in summer than in .
21. Spring is to as dawn is to day.
22. It is to do two things at once.

Table XIV gives the result of the experiment for 374 girls tested at Oxford Higher Elementary Schools.

It was not possible to obtain averages for the test under the age of 7 years, as many of the children were unduly handicapped by difficulties in the reading of the sentences, but the table shows a steady decrease in the difference of time taken to perform the double test at each age from $41\frac{3}{5}$ seconds at 7 years to 24 seconds at 15, with the exception of a loss of ability at 13, the girls at that age taking an average of 3 seconds longer than at 12.

TABLE XIV. *Test of Divided Attention. Discs and Sentences.*

Age	No. of subjects	Av. in secs. Discs	M.V.	Av. difference in secs. Discs and sentences	M.V.	Range of indivs. Discs.	Range of indivs. Discs and sentences
15·5	17	51	—	24	10	$37\frac{4}{5}$– $61\frac{4}{5}$	5 –$45\frac{2}{5}$
14·5	46	$48\frac{1}{5}$	$3\frac{1}{5}$	$24\frac{3}{5}$	$9\frac{4}{5}$	43 – 61	5 –$56\frac{4}{5}$
13·5	50	50	3	$29\frac{2}{5}$	$10\frac{3}{5}$	45 – $60\frac{2}{5}$	$5\frac{1}{5}$–$70\frac{4}{5}$
12·5	50	$51\frac{1}{5}$	4	26	$9\frac{4}{5}$	42 – $64\frac{1}{5}$	$4\frac{2}{5}$–53
11·5	40	$54\frac{3}{5}$	$4\frac{1}{5}$	$32\frac{3}{5}$	$11\frac{4}{5}$	45 – $70\frac{2}{5}$	7 –$67\frac{3}{5}$
10·5	51	$56\frac{1}{5}$	$5\frac{3}{5}$	34	$9\frac{3}{5}$	45 – $73\frac{1}{5}$	$7\frac{4}{5}$–66
9·5	41	61	$8\frac{1}{5}$	$36\frac{3}{5}$	$12\frac{4}{5}$	$49\frac{1}{5}$– 90	$7\frac{1}{5}$–80
8·5	49	$65\frac{1}{5}$	$6\frac{1}{5}$	$40\frac{4}{5}$	$15\frac{1}{5}$	$52\frac{2}{5}$– $89\frac{1}{5}$	10 –95
7·5	30	$69\frac{2}{5}$	$7\frac{2}{5}$	$41\frac{3}{5}$	$13\frac{3}{5}$	60 –122	$18\frac{1}{5}$–89

There was little or no improvement in the individual records when the test was repeated, and this corresponds with the lack of practice effect in the Dotting test, and has been noticed in tests of Attention by other observers.

The reliability is $\cdot 79 \pm \cdot 05$ for age 10 and $\cdot 61 \pm \cdot 06$ for age 14. This test differentiated the children more than any other; there were very few ties, and this is a contrast to the Dotting test, where the number of children at each age whose attention broke down at the same place was considerable.

The double test method seems to have been first used by Loeb in 1886 and since then by many experimenters[1]. Objection has been taken to many of these experiments in that in too many cases one of the processes requires no constant attention, such as the finger movements or tapping used by Binet and Jastrow and De Sanctis, but the placing of the discs in the Discs and Sentences test never becomes automatic and the interference of attention in the second part of the test manifests itself clearly in the increased effort made by the children, in hesitation and frequent errors in placing the discs and in the lengthening of the time taken to complete the task. Any special difficulty in filling in one of the blanks was almost invariably accompanied by an error in the discs owing to the difficulty the children experienced of dividing their attention at such moments.

[1] W. G. Smith, *Mind*, N.S. x. p. 54. J. Jastrow, "Interference of Mental Processes," *A. J. P.* IV. 1891, p. 219. Geissler, "Measurement of Attention," *A. J. P.*, xx. 1909, pp. 47–9. Dr Hylar, "The Distribution of Attention," *Psy. Review*, x. 1903, p. 373.

Between 40 and 50 children of each age from 7 to 14 were tested in both the Dotting and the Discs and Sentence test, and the low correlation obtained points to very little relationship between the capacity for concentrated and divided attention. At 8 years of age the correlation is only ·17 ± ·12, at age 10, ·25 ± ·10, with an inverse correlation of – ·17 ± ·10 at 11 and of – ·14 ± ·10 at 13. This absence of positive correlation does not give any support to the view that there is one capacity of attention, but that rather like the Memory it is highly specialised with the individual differences pointed to by Meumann.

REASONING TEST.

Completion of Analogies.

This is sometimes known as the mixed relations test. It was first used as an intelligence test by Mr Burt[1] with Liverpool children. It is given by Woodworth and Wells in a standardised form, and has since been included by Whipple in the second edition of his Manual of tests (Test 34 A). The subject is told to note the relation of the second word to the first and then find a word standing in the same relation to the third word. Thus in the example: 'Grass : green :: Chalk : —,' green gives the colour of the grass and it is required to give the colour of chalk. In the example 'Early: late :: long —,' early and late are opposites and the task is to find the opposite of long. Several examples were worked on the blackboard before giving out the test papers in order that the children should realise that it is not always the same relation that is needed, but a new relation each time as indicated by the first two words in the line.

The test was given twice on separate occasions and the averages are in every case from the second paper (Analogies 2) given below. The third paper (Analogies 3) is that used when the test was repeated a year later, and also for purposes of a comparison of the results of the test given individually. Three marks were given for each correct analogy and one mark, or two, for fair attempts. No marks were deducted for blanks, wrong answers, spelling or grammatical errors. The usual precautions were taken that subjects should not see each others' papers during the experiment; the time allowed was 15 to 20 minutes, which enabled all but exceptionally slow children to finish the test, 5 or 10 minutes being enough for the majority at each age.

[1] "Experimental Study of General Intelligenee," Address to the *Manchester Child Study Society*, 1909. Cf. also *Journal of Experimental Pedagogy*, 1911, I. ii. pp. 100. 101. Cf. Wells and Woodworth, "Association Tests." *Psychological Monographs*, No. 57, 1911.

	Analogies, test 2	Analogies, test 3
1.	Joy : Sorrow :: Feasting :	Page : Book :: Tree : ......
2.	Button : Buttonhole :: Hook :	Penny : Shilling :: One : ......
3.	Skin : Animal :: Bark :	Flour : Miller :: Soot : ......
4.	Policeman : Burglar :: Cat :	Pole : Punt :: Oar : ......
5.	Sweet : Bitter :: Sharp :	Gold : Miser :: Nuts : ......
6.	Day : Week :: Month :	Caravan : Gipsy :: Ship : ......
7.	Plough : Field :: Spade : ...	Notes : Chord :: Letters : ......
8.	Figures : Numbers :: Letters :	Plate : Rack :: Book : ......
9.	Pearls : Oysters :: Peas :	Beef : Bovril :: Grapes : ......
10.	Dog : Kennel :: Bird :	Horse : Barge :: Engine : ......
11.	Shallow : Deep :: Brook :	Stitch : Tapestry :: Bricks : ......
12.	Yes : No :: Right :	Dog : Blindman :: Lighthouse : ......
13.	Dawn : Morning :: Twilight :	Eyelid : Eye :: Blind : ......
14.	Picture : Frame :: Field :	Fly : Spider :: Worm : ......
15.	Inch : Length :: Pound :	Cones : Fir-tree :: Acorns : ......
16.	Black : Grey :: Red :	Bagpipe : Scotchman :: Harp : ......
17.	Rest : Movement :: Captivity :	Pedlar : Country :: Shop : ......
18.	Writing : Typewriter :: Voice :	Author : Pen :: Crossing sweeper : ...
19.	Ladder : Loft :: Stairs :	Sheep : Flock :: Card : ......
20.	Defeat : Victory :: Despair :	Knowledge : Ignorance :: Light : ......
21.	Spring : Summer :: Dawn :	Link : Watchchain :: Daisy : ......
22.	Forget : Neglect :: Remember :	Trap : Rat :: Snare : ......
23.	Riot : Law :: Tumult :	Ripple : Pond :: Wave : ......
24.	Harmony : Discord :: Music :	Individual : Crowd :: Bee : ......

Results. Table XV gives the result of the test for 409 girls at the Oxford schools.

TABLE XV. *Analogies. Test of Reasoning Power.*

Oxford Girls. Higher Elementary Schools.

Age	Number	Av. II	M.V.	Range of indivs.
15·5	40	51·8	8·7	67–24
14·5	40	46·6	8·4	69–20
13·5	70	45·1	8·4	65–25
12·5	70	43·0	8·2	62–19
11·5	70	39·0	10·0	65–11
10·5	64	34·0	8·6	57–13
9·5	40	28·8	6·1	45–14

There is a small but fairly steady increase of reasoning power with age, the difference between each average being most marked from 9 to 12. The individual scores show that from 11 to 13 the best children of each age are about equal, and the fact that the performance of the subject is, to some extent at least, independent of his age, is shown by the low correlation of the test with age (·47) for a group of 40 girls aged 9 to 16; bright boys and girls of 8, 9, or 10 not infrequently grasped the nature of the test better than less intelligent subjects of a much older age.

A high correlation of this test with intelligence was found by Burt; and this has since been confirmed by Vickers and Wyatt with Manchester children. As a result of their experiments these investigators conclude that it is an exceedingly suitable one for grading children in school entrance examinations, etc. The reliability of the test for the Oxford girls was $\cdot 35 \pm \cdot 09$ at 10 years, $\cdot 62 \pm \cdot 06$ at 11, $\cdot 78 \pm \cdot 04$ at 12, $\cdot 76 \pm \cdot 05$ at 13. There does not seem to be much difference as the result of practice in the test once its nature is grasped by the subject —the marks gained in series 2 and 3 were often identical; but as there are great individual differences in the ability to understand and profit by instructions the test would not in some cases be primarily one of reasoning power if the practice test was omitted.

The test is not very suitable as a group experiment under 9 years, but if the children are tested individually it gives fairly reliable results at 7 and 8. Children of 5 and 6 with various exceptions failed to grasp what was required of them.

4. DEDUCTIONS.

(1) THE CORRELATION OF MENTAL WITH PHYSICAL AGE.

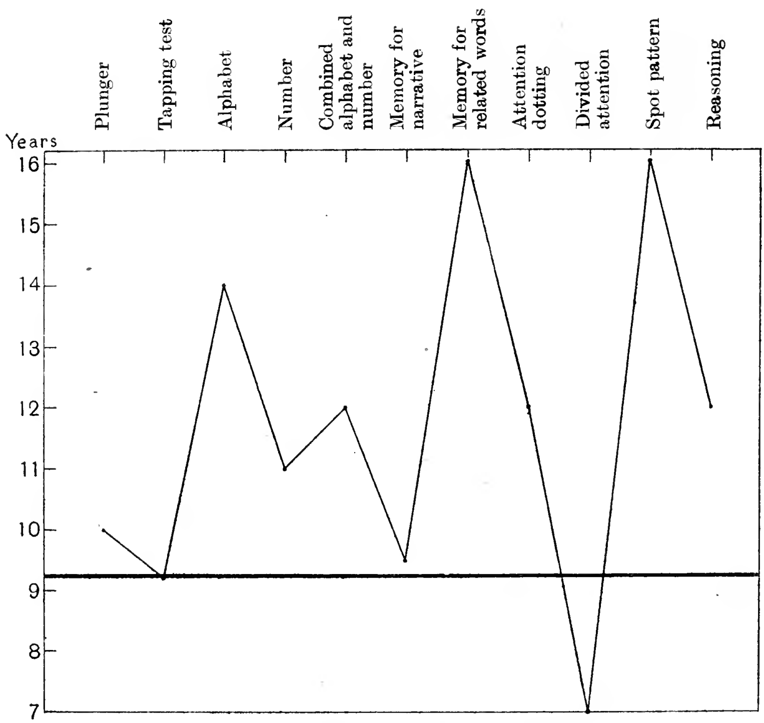
Although as the child grows older there are marked changes in the development of the mental functions, yet the degree of mental capacity cannot be inferred from age, and a comparison of the graphs, which show the ability of individual children in the various mental tests measured in terms of age, brings this out very clearly. The thick line represents the physical age of the child, the curve rising and falling above or below the line with the mental ability of the subject. It will be seen that the psychical chart of the child does not correspond with the flat mental age assumed by the Binet scale. The curve of ability, even when mental and physical age corresponds, seldom touches the same level for more than two of the given tests. (Some of the diagrams have already been published as illustrations of so-called "mental profiles" or "psychographs" in a paper read before the Conference of Educational Associations[1].)

Graphs I–III are those of children of 9 years from the same form in school.

[1] M. E. Bickersteth and C. Burt, *Some Results of Mental and Scholastic Tests.*

Graphs I and II show an inverse relation between the Motor tests, and the test of Divided Attention, and the tests at the higher mental levels. A. T. (Graph I) was an able child, generally top of her form, while M. D. (Graph II) was a bright, well-grown child, but not conspicuously good at school work; she is ahead of A. T. in reasoning power but below her in the memory tests.

In the case of the third child (Graph III) ability in the tests is reversed for she is above the average in Motor tests and in Divided



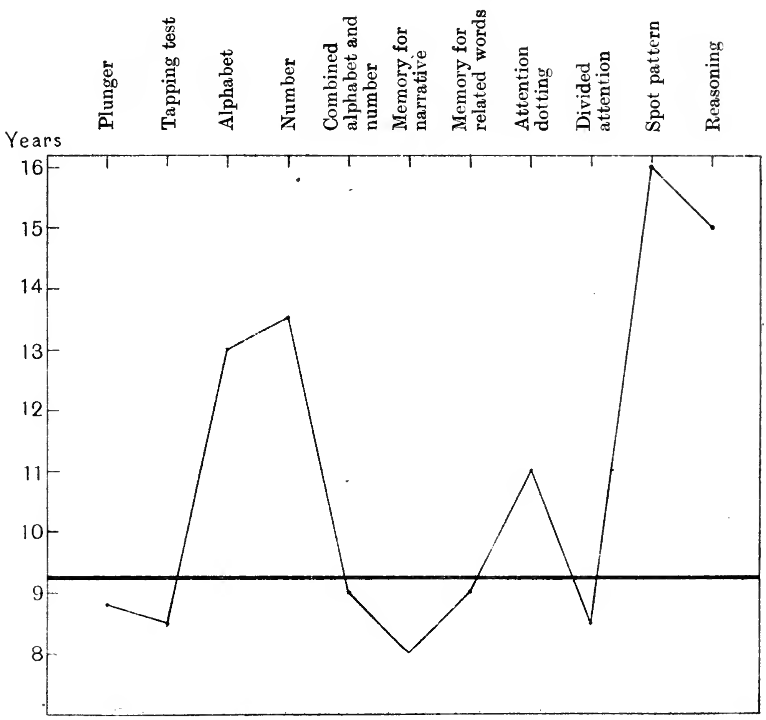
Graph 1. A. T. 9·3 years. Average mental age 11·7.

Attention, and two years below her age in Apperception and Reasoning power, and her average mental age shows a retardation of one year.

Graph IV is that of twin sisters of 10 years, both in the same form. Janet was five places above Joyce, and her average mental age is 8 months in advance of her twin. The curves for the two children are identical for the Motor and Memory tests, but the less able child excels in both the Dotting and the test for Divided Attention, while in Reasoning power she is six months and in Apperception two years below her sister.

Graph V is that of a child who is on the average 18 months ahead of her age, and like the clever children of 9 years she also is below her age in the Tapping and shows under average capacity in the tests of Attention.

Graph VI gives the mental curves of two girls of 11 in the same form at school. One shows marked ability in every test but Attention, the

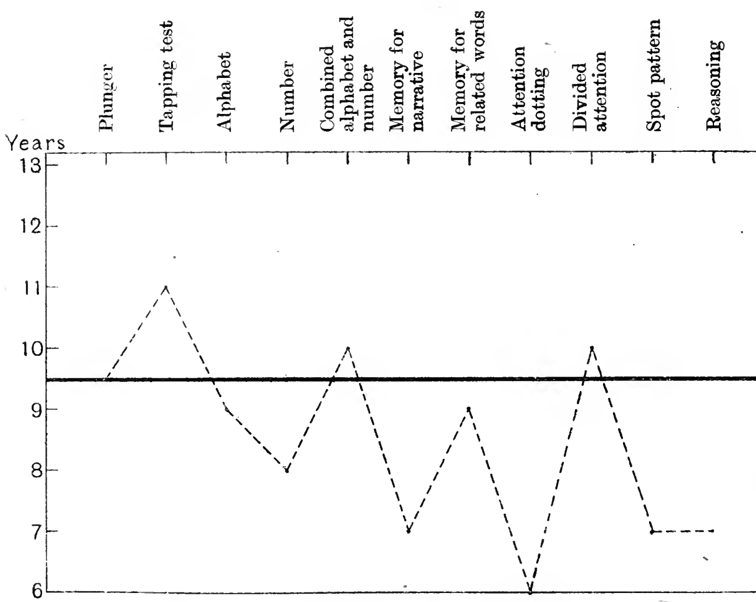


Graph 2. M. D. 9·2 years. Average mental age 10·9.

other is above the average for the Tapping and the Plunger, but except in Attention where the two are equal she is as much below her age for each test as the other is above, and the two curves are practically reversed.

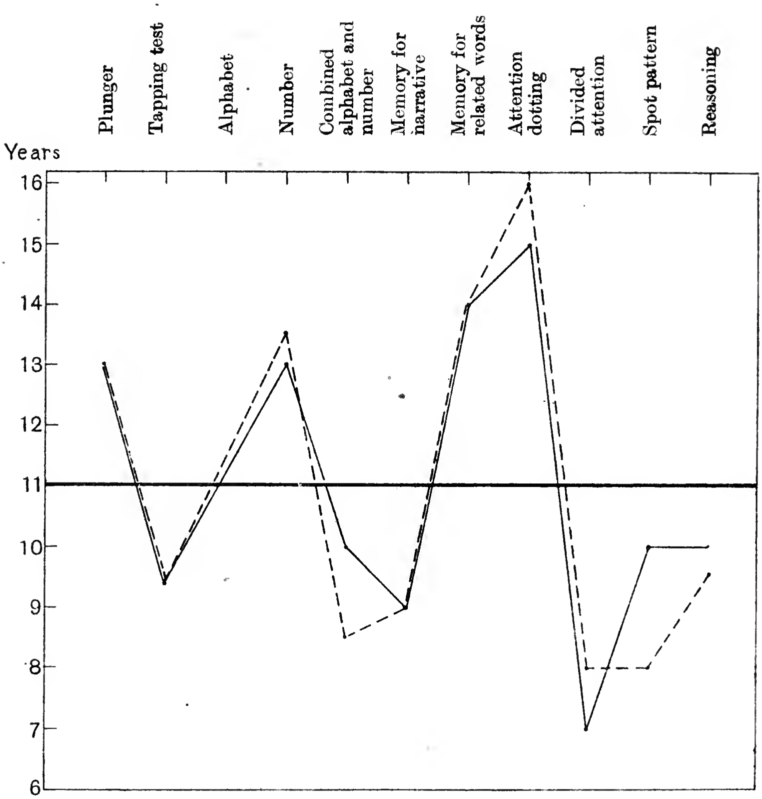
Two Graphs, VII and VIII, are given for age 12. E. J., Graph VII, is on the average two years above her age, her curve only falling below the average in the Tapping test, while F. B. is mentally three months below her age, her lack of ability showing in the three tests in which the more intelligent children do best, in Memory for Related Words, and in Apperception and Reasoning.

The Graphs show in nearly every case an inverse relation of the Motor tests with those of the higher levels. There is a greater capacity for Divided Attention in the less clever children who are also in several instances equal to or better than more able subjects in the Dotting test. As the children compared were taken from the same forms at school, and approximately therefore as far as acquired skill and knowledge are



Graph 3. K. H. 9·5 years. Average mental age 8·5.

concerned at the same mental stage, the difference in innate capacity which the mental tests bring out is striking, and the correlation of Mental with Physical age worked out in the same way for a group of 50 girls aged 9–11 is only $\cdot 31 \pm \cdot 08$, while for the same group in the tests of the higher mental levels, excluding that is to say the Plunger test and the Tapping test, the correlation between age and the total number of marks gained by each child is only $\cdot 26 \pm \cdot 11$, but in a larger group (100 subjects), including children from 9–16, the correlation rises to $\cdot 57 \pm \cdot 07$, the results being probably influenced by the greater range of age in the group and due to the relationship which exists between increasing age and a general development of capacity. A very able child of 9 may be able to perform given tests better than the average girl of 15 or 16, but is in no sense equal to the clever girl of the older age except in cases where the development of innate capacity may have reached its maximum.



Graph 4. (1) Joyce P. 11·1 years. Average mental age 10·9.
(2) Janet P. 11·1 years. ,, ,, ,, 11·5.

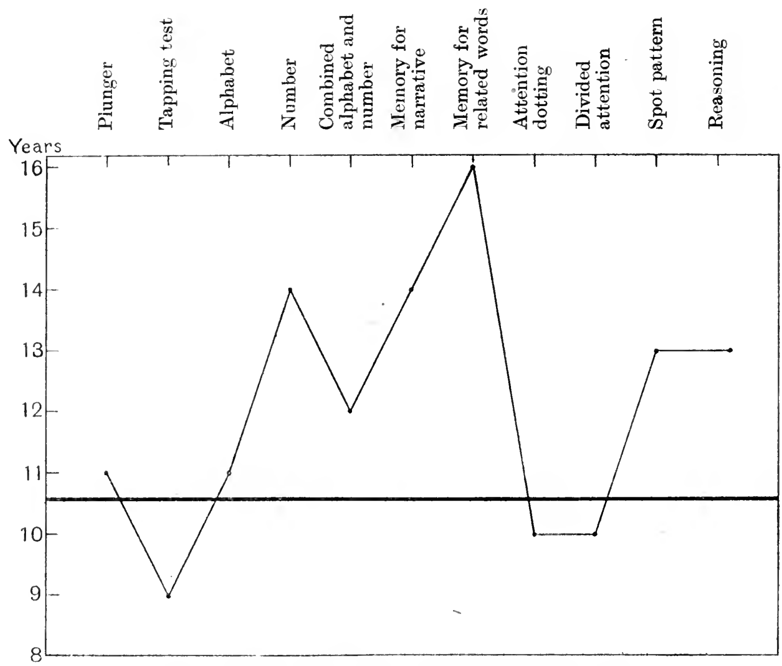
This low correlation of mental with physical age is in agreement with the result of Heron, Karl Pearson, Jones and Wait[1], none of whom found any definite relationship between the age of the children and the teacher's estimate of their mental capacity.

[1] *Biometrika*, 1906–7, v. p. 145. 1909–10, VII. 1911–12, VIII.

(2) The Correlation of Mental with Motor Ability.

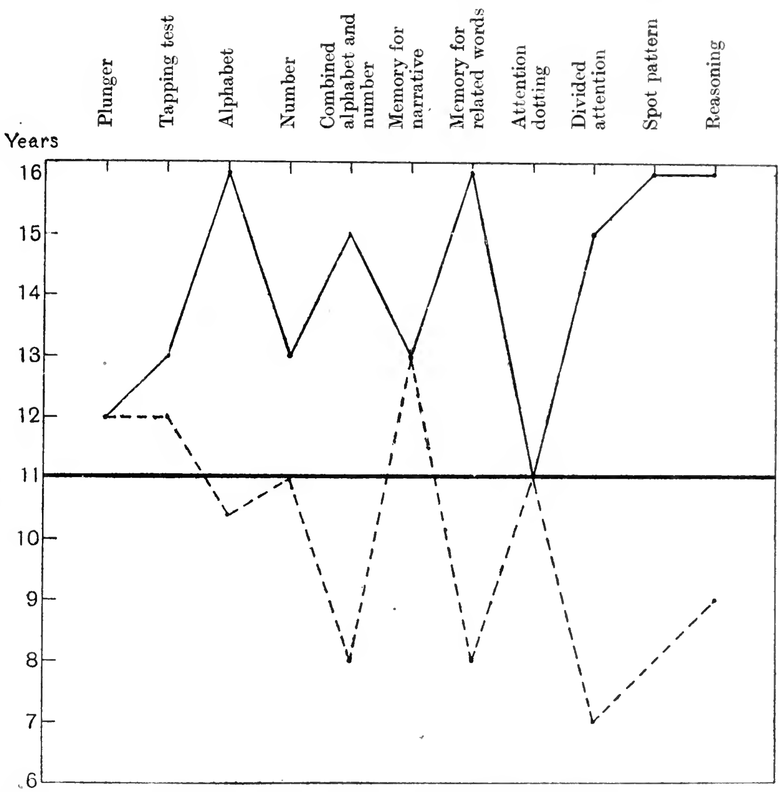
The problem of the connection of Mental and Motor Ability has been attacked by various investigators, Porter 1893, Gilbert 1894, Bagley 1897, Kirkpatrick 1900 and Bolton 1903, but the results leave the problem much as it was.

Porter, finding a marked relationship between weight and mental precocity and using examination tests as his test of mental efficiency,



Graph 5. M. D. 10·6 years. Average mental age 12.

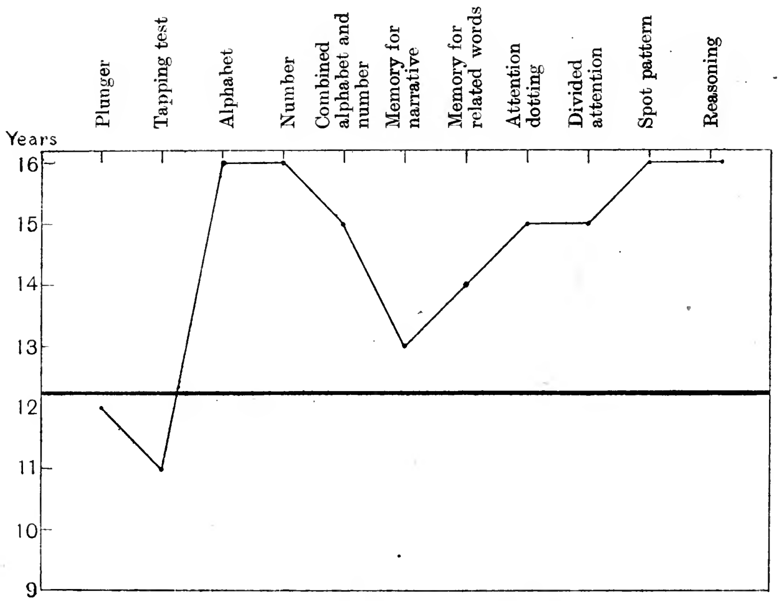
argued that a child increases in mental efficiency directly as he increases in motor ability. Gilbert using teachers' estimates of the children's intelligence found no such relation, his results justify Langes' opinion that physical growth takes up the strength and retards mental development. Bagley too found an inverse relation between motor and mental efficiency; the brighter children he says as a rule showed low motor ability and those best developed physically were generally deficient in mental capacity, with numerous exceptions and with a varying validity at different periods of development. These conflicting results were severally confirmed by Bolton and Kirkpatrick. Bolton apparently correlated with social status, but Kirkpatrick tested 500 children and found a decided correspondence between motor ability and intelligence as estimated by the teachers. In England, Burt and Moore have found that the correlation of simple motor tests with intelligence is positive, but relatively small compared with those given by more complex tests.



Graph 6. J. P. 11 years. Average mental age 14·1.
E. W. 11 years. " " " 9·9.

Abelson, on the other hand, working with defective children considered the simpler tests were quite as efficient. In all these results, with the exception of those of Bagley who used various forms of reaction tests, the data for the mental ability of the children was derived from non-experimental sources. In this experiment the degree of correlation of the motor with the mental tests used gives little support to the conclusions of Porter or Kirkpatrick. The correlation of Tapping with the amalgamated score of each child in the tests of the higher mental levels for 50 girls aged 9–14, was only ·31 ± ·08.

There is a low correlation at age 10 and 11 between tests of Reasoning Power and Tapping, an inverse correlation of − ·32 at 12 and − ·01 at 13, while the Reasoning test and Plunger test give no appreciable correlation except at age 11, in both cases the small relationship that exists decreases with age.



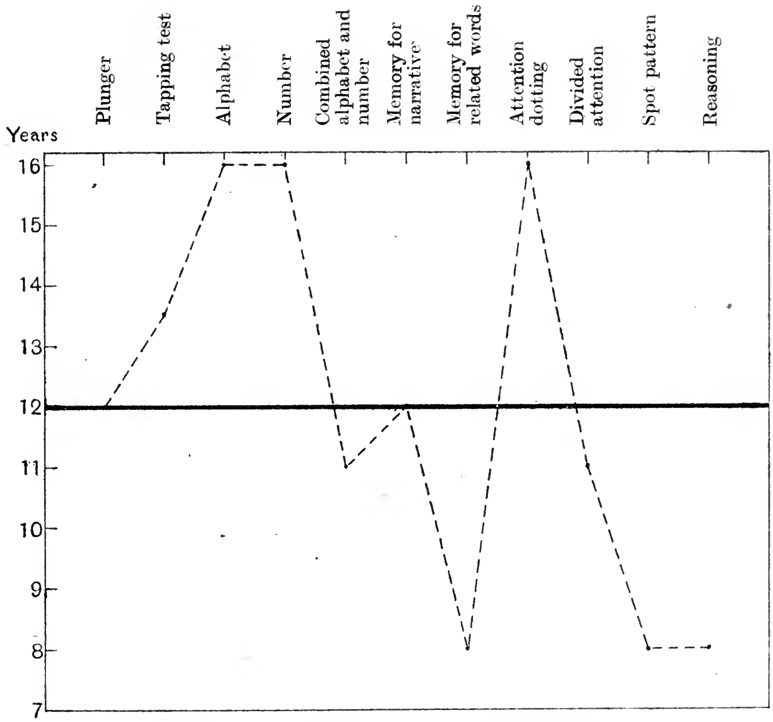
Graph 7. E. J. 12·2 years. Average mental age 14·4.

TABLE XVI. *Correlation of Motor with Mental Ability.*

Oxford Higher Elementary Girls.

Age	Reasoning test and the Tapping test	Reasoning test and the Plunger test
13	− ·01 ± ·10	·01 ± ·10
12	− ·32 ± ·09	·20 ± ·10
11	·20 ± ·11	·50 ± ·09
10	·20 ± ·10	·06 ± ·10

The result of the Tapping test and school rank, examination places, was also worked out for the Third Form (average age 13½) at the Girls' Central School, and gave a correlation of only ·28 while the correlation with the Head Mistress's estimate of intelligence was ·20 for ages 12 and 13 and ·37 for ages 10 and 11, the correlation with the experimenter's estimate for the latter group being ·32. The children in the third, fourth and fifth forms were also divided into two groups, those over and those under the average in the Analogies test. 75 per cent. of the girls of 12 to 13 years who were below the average in the Reasoning test were above the average in the Tapping test, but in the case of younger girls 10–11 only 51 per cent.



Graph 8. F. B. 12 years. Average mental age 11·9.

The inverse relation of the Motor tests with those at the higher levels has already been pointed out in connection with the graphs of individual children, but one of these, No. 6, that of a child whose superior ability is shown in the Motor as well as the Mental tests, bears out the finding of Bagley that there are numerous individual exceptions to the general absence of correlation of mental with motor ability.

NOTE. The decrease of correlation with intelligence among older children is confirmed by Burt and was also found by Binet.

(3) EVIDENCE FOR INDEPENDENT MENTAL CAPACITIES.

The high degree of independence of mental capacities even when the tests appear to measure almost identical functions has been pointed out by Thorndike[1], and in this investigation there is seldom a high correlation between the different tests. The figures for the various tests are given below in Table XVII and show that there is considerable variation in the relationship of different functions at different ages, the degree of correlation increasing and decreasing, or turning into inverse correlations. The correlation of the Tapping test with the Plunger test, and of those tests and the Reasoning test appears to decrease with age while the correlation of the Memory test with the Reasoning test and of the Memory test with the Dotting test increases—the correlation of tests, except those of motor, capacity is very low at 9 years, and higher at 12 than at 10 or 11—and there are several instances of inverse correlations at the latter age.

TABLE XVII. *Correlation of Mental Tests.*

Oxford Higher Elementary Girls.

Age	Dotting test and Discs and Sentences	Dotting test and Memory for Related Words	Dotting test and Analogics	Dotting test and Tapping	Tapping test and Plunger	Tapping test and Analogies	Plunger test and Analogies
14	·16±·10	·32±·13	–·17±·12	—	—	—	—
13	–·14±·10	·35±·09	·09±·09	·45±·08	·13±·10	–·01±·10	·01±·10
12	·22±·10	·03±·10	—	—	·17±·10	–·32±·09	·20±·10
11	–·17±·10	–·11±·12	·11±·09	·43±·08	·22±·11	·20±·11	·50±·09
10	·25±·10	·31±·11	·01±·15	—	·37±·09	·20±·10	·06±·10
9	·20±·12	·00±·15	—	—	·44±·08	—	—
8	·17±·12	—	—	—	—	—	—

(4) THE CORRELATION OF MEMORY WITH INTELLIGENCE.

Some degree of correlation of Memory with Intelligence has been found by Meumann-Winch[2], Binet, Jacobs[3], and Burt[4] and denied by Bolton[5] and Ebbinghaus, but most of the conclusions as to the connection of Memory and Intelligence have been derived from tests of

[1] *Educational Psychology*, New Ed. p. 188.

[2] This *Journal*, 1904, I.

[3] *Mind*, 1887, XII.

[4] This *Journal*, III. p. 143.

[5] *A. J. P.* IV. p. 362.

Immediate Memory. The result of the tests of logical memory and connected words in this experiment seems to be in agreement with that of Meumann who concluded that more intelligent children are endowed with better memories, and with that of Burt, who concluded that logical or rational tests of memory were more effective than tests of mechanical memory. To compare the memory of the more and the less intelligent children, a group of 100 girls of 9–14 years were divided according to intelligence, this being measured by school rank, the Head Mistress's and the Experimenter's estimates of intelligence and by skill in the higher tests. In the two memory tests the average of the more able girls, 52 in number, was 18·9, the average for the second group being 16·8. There were approximately the same numbers of each age in the two groups so the difference in memory was not influenced by the age factor in either case, but the difference between the individuals comprising each group was much more marked in the test for Related Words than for Narrative Memory.

(5) Individual Differences during Practice.

There were large individual differences in the degree of improvement at the second and third applications of the tests. Bolton, as a result of his experiment, concluded that the ability of a child to improve by practice represents educability and that such capacity might be taken as one test of mental strength, while Thorndike found that large individual differences increased with equal training, showing a positive correlation between high intellectual ability and ability to profit by learning.

Dr Whitley[1] points out that better tests should not show rapid improvement with practice and only a small variation in repeated trials (after the practice effort is allowed for) for the greater the variability, the less reliable is the single trial or the average of a few trials. The tests that showed the least improvement with practice were both tests of Attention and the Memory tests. In the Dotting test 36 per cent. of the children over 12 and 27 per cent. under 12 showed no measurable improvement.

Improvement in all the tests was least at ages 5, 6, and 7, and greatest from 9–12, but there was almost invariably less improvement at 7 than at 6, less at 10 than at 9 and at 12 than 11. The small improvement in the second test in the case of older subjects might be due to the

[1] "An Empirical Study of Certain Tests for Individual Differences," *Archives of Psychology*, No. 18, 1911.

fact that grasping the nature of the task more thoroughly they did themselves more justice at the first attempt than younger children of 9–12, but in some of the tests at least the difference caused by age seems to be negligible. In the Dotting test the improvement was worked out for a group of 120 children, 61 being over and 59 under 12 years of age, the gain of the older group was 74·7, making an average improvement of 1·2 more dots crossed out, while the children under 12 crossed out 80 more dots in the second test, an average improvement of 1·4, while in the Alphabet test for the same groups, the older children crossed out an average of 4·6 more letters on the second application of the test and the younger 3·8.

The influence of intelligence as a factor in improvement was brought out more clearly in the case of a group of 14 girls aged 11 years, on whom some of the tests were repeated daily during one week. The improvement in each test during the experiment, was calculated as a percentage of the subject's gain on her first attempt. The individual differences were very marked, some children showing improvement in one test and not in another, others the same degree of improvement at each test, one girl improving 18 per cent. in the Alphabet, 18 per cent. in the Number, 18 per cent. in the Reasoning and 17 per cent. in the Tapping test. The seven best children showed a total improvement of 126 per cent., and the less intelligent children only 66 per cent. As the girls were all the same age the difference in the two groups can only be due to the positive correlation between intellectual ability and ability to profit by learning that was found by Thorndike, with individual exceptions for one particularly able child only improved 46 per cent. while a child in the less intelligent group improved 157 per cent.

The problem of individual differences during practice has been examined by Hollingworth[1], who writes "in the direct application of mental tests it has been too often assumed that the actual performance of an individual in one or more dozen trials at a given task is in some way or other significant of that individual's final capacity for that work." The problem has also been examined by Whitley[2], who concludes that "the criticism that a single trial is unreliable is true but need not be exaggerated, since other facts also enter to make trials unreliable. To overcome this at least two trials should be made of any test. The criticism that giving only a few trials measures not the mental process

[1] W. J. Hollingworth, "Individual Differences during Practice," *Psy. Review,* Jan. 1914.

[2] Whitley, "The Relation of Practice to Individual Differences," *A. J. P.*, Jan. 1912.

supposedly tested but merely adaptability to strange conditions is seldom of weight." Hollingworth put 13 individuals through 173 repetitions of seven different experiments, and found that the preliminary trial was in no sense a measure of the final relative capacity of the individuals tested, for not till the eightieth trial did most coefficients rise to + ·90 and over. The result of repeated trials of the Tapping, Plunger, Alphabet, Number and Reasoning tests in this experiment are given in the following tables. To eliminate differences caused by age only subjects 11 years of age were included. The children exhibited no decrease in zeal day after day which would of course have nullified the results.

TABLE XVIII. *Repeated Tests. Group of* 14 *subjects. Correlation with final order.*

Oxford Higher Elementary Girls.

	Tapping	Sustained Effort	Plunger	Alphabet	Number	Reasoning
I and II	·14	·22	·61	·94	·35	·76
I	·00	·11	·11	·55	·60	·76
II	–·00	–·11	·35	·61	·19	·76
III	·35	·38	·26	·79	·38	·62
IV	·05	·20	·32	·71	·69	·69
V	·16	·57	·57	·69	—	—

The results of the Tapping test confirm those of Hollingworth, and except in the sustained effort where the correlation between the fifth test and final order is ·57, the amalgamation of the first and second test gives a higher correlation with final capacity than any of the following trials.

The fall off in correlation in the Alphabet and Reasoning test may be owing to the fact that after the third test the better children had reached their maximum capacity and the rest were gradually overtaking them. The difference in reliability of the successive tests was interesting, for the Plunger the first and second attempt gave a correlation of ·80, the second and third of ·48, the third and fourth of ·51; in the Reasoning test the reliability was ·76 between the first and second test and then decreased. In these tests at least correlation seems lowered by practice, as Binet assumed was the case in the correlation of all mental abilities.

The results of the experiment seem to indicate that in the Motor tests the first trials are much less representative of the capacity of the subject than tests of Discriminative Selection and Reasoning.

(6) Mental Differences between Town and Country Children.

In order to obtain some data as to the difference in the mental capacity of town and country children five of the group tests already described—the Alphabet, Number, Combined Alphabet and Number, Reasoning, and Logical Memory tests—were given to about 1200 children attending elementary schools in the Yorkshire Dales and Leeds. The Leeds schools were carefully selected so that the two groups should be as comparable as possible, in respect of the race and social standing of the children.

The Dales group proved to be the more homogeneous as to race, the Leeds as to social standing, for the Dales elementary schools were attended by the children of yeomen farmers and labourers, the former more numerous than the latter as the schools in the head of each Dale were reached; while the Leeds group was made up almost entirely of the children of artisans living in the better class districts of the town. The admixture of racial elements in the Leeds schools was ascertained by asking the children to find out the birthplace of their parents, grand-parent and great-grandparents and to bring the information in writing to school. The majority of the children were able to bring family records dating back three, four or more generations. A large proportion, 82 per cent., were of north-country origin, and the foreign and Jewish element, very large in some parts of Leeds, was in this particular group a negligible factor (2 per cent.).

The work of giving the tests to the Dales children was carried out in the autumn of 1914 and the spring of 1915, and experiments were conducted in 38 schools in Swaledale, Arkengarthdale, Wensleydale, Widdale, Bishopdale, Coverdale, Upper Wharfedale, Littondale, Lang-strathdale and Colsterdale, almost every school being visited in an area of about 900 square miles.

As a rule two mornings were spent at each school with a week's interval between each experiment, but some of the schools were only reached after a walk of many miles over the fells, and in such cases the whole series of tests was not given, but at these schools, far from any village, and attended by children living in lonely farms on the moors, sometimes three or four miles from the school, the performance at the tests was almost invariably above the average for the Dales as a whole[1].

[1] In these remoter Dales 84 per cent. of the children were above the average for the Dales as a whole in the Reasoning test, and 74 per cent. of the children in the Memory test, while very marked ability was not infrequently shown in the case of individual children.

Some of the children from these remote farms had never seen a railway, and one of the schoolmasters said the same would be true of many of the parents. For the Dales are each little miniature worlds with their own customs and traditions, and there is little intercourse even between Dale and Dale, separated as they are by bleak and wild fells, crossed only by rough mountain passes.

In the remoter Dales, where the mixture of blood is smallest, the physical characteristics of the people are strikingly Scandinavian, the tall, fair, long-faced, blue- or grey-eyed type which is a complete contrast to the darker people of the western moorlands.

There is little doubt according to Beddoes that the Scandinavian element suffered most severely in the devastation of the North by William I and Malcolm Canmore who invaded North Yorkshire after the departure of the Norman Army and completed the work of destruction, and the Domesday map of Yorkshire shows every village in Wensleydale and Upper Swaledale destroyed and depopulated, while Coverdale, which had six villages in the time of Edward the Confessor and which from its remoteness might well have escaped, is described as waste. To-day Coverdale has still only three villages and a couple of hamlets. While then the present population of some parts of the Dales is probably Scandinavian in origin, speculation as to the ethnological character and origin of its inhabitants is vitiated by our ignorance as to its re-peopleing, one theory is, that this may have been in part at least, from Norwegian Westmoreland.

The Dales children were keenly interested in the tests, little groups would wait outside the school after the second series, and ask very eagerly if there would be "more papers next week"—probably for the same reason as the small boy in Langstrathdale who whispered, as he handed his finished papers to the writer "Ya did reeght t'coom, it's neea lesson."

The tests were given to the Leeds children in March 1915; the schools, as already stated, were in the better parts of the city, and chosen by the Education Committee as the most suitable for the purposes of the experiment. Owing to the large numbers attending each school, it was found sufficient to conduct the tests in two schools for the lower ages, while a third school was added to make up the number in the case of the 13-year-old group.

As the method of giving the tests and the precautions taken on each occasion to ensure uniformity of conditions were identical in every respect with those already described for the Oxford group, they are not repeated here.

Results.

1. *Tests of Selective Discrimination.* Tables XIX and XX give the average number of letters and figures crossed out by the Dales and Leeds children respectively at each age. The percentage of improvement in the Combined test is calculated on the subject's gain on his original score.

TABLE XIX.

Yorkshire Dales.

1. *Alphabet Test.*

	Boys					Girls				
Age	No.	Av. I	M.V.	Av. II	M.V.	No.	Av. I	M.V.	Av. II	M.V.
13·5	40	8·3	3·1	10·6	3·1	50	7·4	3·6	10·4	3·1
12·5	50	5·4	2·2	9·2	3·4	60	6·8	3·0	10·6	3·0
11·5	55	5·1	2·4	8·4	3·0	62	5·3	2·1	8·7	2·6
10·5	51	4·1	1·8	6·3	2·3	42	4·7	2·3	7·2	2·1

2. *Number Test.*

	Boys					Girls				
Age	No.	Av. I	M.V.	Av. II	M.V.	No.	Av. I	M.V.	Av. II	M.V.
13·5	40	10·1	3·4	11·8	3·8	50	9·2	3·1	11·4	2·8
12·5	51	9·3	3·6	11·0	3·1	59	9·9	3·5	11·7	3·1
11·5	50	7·8	2·6	10·2	3·2	65	8·3	2·8	10·6	3·0
10·5	50	6·1	1·0	7·5	2·3	40	6·7	2·4	7·9	2·8

3. *Combined Alphabet and Number Test.*

	Boys				Girls			
Age	No.	Av. II	Av. I and II	% gain on I.	No.	Av. II	Av. I and II	% gain on I
13·5	40	23·9	20·7	35·8	47	23·8	21·8	22·7
12·5	50	19·9	17·9	24·5	60	22·8	20·3	28·1
11·5	44	19·7	17·9	21·6	62	19·6	17·6	25·6
10·5	44	16·5	14·2	37·5	42	17·5	15·8	22·9

TABLE XX.

Leeds.

1. *Alphabet Test.*

	Boys			Girls		
Age	No.	Av. I	Av. II	No	Av. I	Av. II
13·5	34	8·0	11·1	48	8·8	8·52
12·5	66	6·1	8·7	43	7·0	9·1
11·5	81	6·3	8·7	69	6·3	8·9
10·5	68	5·0	5·9	62	5·7	8·6

2. *Number Test.*

	Boys			Girls		
Age	No.	Av. I	Av. II	No.	Av. I	Av. II
13·5	18	9·4	13·5	31	8·9	11·7
12·5	69	8·2	9·1	48	7·5	10·3
11·5	84	8·3	10·8	73	8·7	11·3
10·5	57	6·2	8·2	60	7·9	10·0

3. *Combined Alphabet and Number Test.*

	Boys				Girls			
Age	No.	Av. II	Av. I and II	% gain on I	No.	Av. II	Av. I and II	% gain on I
13·5	20	26·0	22·9	30·7	32	20·9	20·7	1·5
12·5	66	19·8	18·3	17·8	46	23·6	20·5	34·2
11·5	80	21·9	19·3	31·1	67	23·3	20·8	26·6
10·5	57	19·5	16·9	36·4	62	19·7	18·2	17·3

The superiority of the Leeds group in the Alphabet test was very marked on the first occasion. In the Number test, which was given immediately afterwards, the superiority of the Leeds children was confined to ages 10 and 11, but in the Combined test, which introduces a new element, the Leeds were again ahead of the Dales children at each age.

This superiority in the first performance of the tests seems to point to a greater power of adaptation to a new situation in the case of the town child, but not to a fundamental difference in the capacity tested, as is shown by the fact that, except in the Combined Alphabet and Number, the uniform superiority of the Leeds children is not maintained on the second application of the test. In each test girls are slightly quicker than the boys of the corresponding age, except at 13 when the boys overtake the girls. There is a loss of ability at age 12 in the case of the Leeds boys in all three tests, and in the Leeds girls in the Number and Combined test.

In the Memory test, Table XXI, the girls are uniformly better than the boys of the corresponding age, and the Dales than the Leeds children; the difference would probably have been greater had not the children in the Leeds schools had some slight practice in reproducing prose substance, while in the Dales the test was quite new to the children.

A comparison of the averages for the logical memory of town and country children, Table XXII, shows in every one of the six averages from town schools in Oxford, Ripon, and Leeds a marked falling off in ability in both boys and girls at age 12; a feature which is absent in the Memory Curve of the Dales and Oxfordshire[1] village children which shows a steady increase from age 10 upwards. This falling off in memory power corresponds with the result of the Alphabet tests at the same age in the Leeds schools.

TABLE XXI.

Memory for Narrative.

Averages, mean variations and number of subjects.

Yorkshire Dales.

	Girls			Boys		
Age	No.	Av.	M.V.	No.	Av.	M.V.
13·5	55	22·5	3·6	44	22·0	3·1
12·5	66	22·3	3·0	52	21·5	3·6
11·5	52	21·0	3·6	41	21·0	4·4
10·5	37	19·8	3·6	42	19·3	5·3

Leeds.

	Girls			Boys		
Age	No.	Av.	M.V.	No.	Av.	M.V.
13·5	52	20·9	3·2	47	18·8	4·1
12·5	71	20·1	3·7	83	17·6	4·5
11·5	88	21·2	3·5	83	19·7	3·7
10·5	87	18·8	4·7	68	18·3	4·2

TABLE XXII. *Memory for Narrative.*

Summary.

	Higher Elementary Schools		Rural Elementary Schools				Town Elementary Schools				
			Oxfordshire Village		Yorkshire Dales		Leeds		Ripon		Oxford
	Oxford Girls	Oxford Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls
Age	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.
15·5	23·3	—	—	—	—	—	—	—	—	—	—
14·5	22·7	22·3	—	—	—	—	—	—	—	—	—
13·5	23·5	20·0	21·9	20·0	22·5	22·0	20·9	18·8	21·0	20·0	20·5
12·5	21·4	19·8	21·2	18·5	22·3	21·5	20·1	17·6	19·8	18·0	17·0
11·5	22·1	—	20·6	17·7	21·0	21·0	21·2	19·7	20·9	20·0	19·7
10·5	18·6	—	20·9	16·0	19·8	19·3	18·8	18·3	19·5	19·1	18·3
9·5	16·9	—	—	—	—	—	—	—	—	—	—

In the Reasoning test the boys are better than the girls of the corresponding group at age 11, 12 and 13, a sex difference which was not found by Mr Burt in similar tests; but the examples in this experiment were considerably harder than those used by Mr Burt, and this

[1] The group tests were repeated in seven village schools in Oxfordshire in the summer of 1915 and in all the elementary schools in Ripon.

may possibly account for the absence of any marked sex difference in his results.

TABLE XXIII.

Reasoning Test.

	Dales Boys				Leeds Boys			
Age	No.	Av.	M.V.	Range of indivs.	No.	Av.	M.V.	Range of indivs.
13·5	44	52·4	7·7	64–18	49	50·5	10·6	69–14
12·5	53	43·6	12·8	69–15	75	51·4	9·0	71–15
11·5	57	39·4	10·5	61–15	91	50·4	13·0	67–23
10·5	47	32·5	9·6	55–16	61	43·4	12·4	66–18
9·5	22	26·0	7·9	45–15				
8·5	15	20·4	5·0	32–11				

	Dales Girls				Leeds Girls			
Age	No.	Av.	M.V.	Range of indivs.	No.	Av.	M.V.	Range of indivs.
13·5	43	47·9	10·2	67–18	48	45·4	11·9	70–16
12·5	74	40·8	10·0	66–14	74	49·4	11·4	65–15
11·5	74	34·2	10·2	63–14	74	47·5	10·0	66–24
10·5	49	32·1	9·7	50– 9	61	42·0	12·3	65–12
9·5	18	28·0	8·2	51–16				

Not only were the Leeds children superior in the actual marks gained in the Reasoning test, but there was a considerable difference in the time taken to complete the Analogies. The longest time for age 13 was taken by a boy in Langstrathdale, who took 30 minutes to fill in his paper, and the shortest time was that of a Leeds boy who did the same test in under three minutes; in both papers every analogy was correctly given so the marks obtained by the two boys were identical. The Dales child has no lack of intelligence, but he is essentially slow and the time taken to complete an unfamiliar task is one not inconsiderable difference between the town and country child.

The table shows a steady increase of reasoning power with age with little or no sex difference at 9 or 10, and a superiority in favour of the boys from age 11 upwards, while the Leeds children are considerably better than the Dales, except at age 13. The poor performance of the Leeds children at age 13, which is noticeable in most of the tests, may be due to the fact that owing to the war more boys and girls than usual had left school obtaining labour certificates. The senior classes would thus contain an undue proportion of dull boys and girls, or of delicate children who had come late to school and had not kept the required number of attendances. The coefficient of Reliability for the Reasoning test is given below in Table XXIV.

TABLE XXIV. *Coefficient of reliability Reasoning test.*

Dales			Leeds		
Age	Boys	Girls	Age	Boys	Girls
13·5	·86 ± ·03	·80 ± ·04	13·5	·79 ± ·05	·87 ± ·03
12·5	·80 ± ·04	·86 ± ·03	12·5	·55 ± ·09	·77 ± ·04
11·5	·91 ± ·02	·85 ± ·03	11·5	·20 ± ·03	·72 ± ·05
10·5	·88 ± ·03	·78 ± ·05	10·5	·76 ± ·05	·86 ± ·02

The reliability of the test is very high except at age 11, Leeds boys. In the first test 10 boys got full marks, in the second which they found much harder there were no ties for the first nine places. This might explain the low reliability in this case.

THE INFLUENCE OF ENVIRONMENT.

Thus the averages of Leeds children are seen to be invariably superior in the Reasoning test and on the whole in the test of Selective Discrimination, and the Dales children in the Memory test; and the problem is how far this difference is due to the different environment of town and country.

It would be hard to imagine a greater contrast than the life of a child growing up among the nature influences of the fells and moorlands of one of the remoter dales, with little or no first-hand knowledge of the world beyond, and the life of a Leeds child among the disturbing influences of a great city. This difference of environment is not without its influence on the physique of the children, but how far the differences in mental capacity brought out in the children's performances in the tests are due to a difference of environment is a difficult and complicated question, and one of far-reaching significance.

To attribute differences between two groups to environment may be as Dr Heron[1] points out, to attribute to social conditions differences in mental traits which are as purely racial as hair or eye colour. If the superiority of the Dales children in the Memory test were indeed a racial character we should expect to find it unchanged in the Leeds children of Dales ancestry, but of 31 such children all but five[2] were considerably

[1] *Influence of Defective Physique and Unfavourable Home Environment on the Intelligence of School Children*, 1910.

[2] The five exceptions are interesting, for these children's ancestors came from the very remote Dales, where the physical characteristics of some of the people are still strikingly Scandinavian. The higher average ability of children in these lonely valleys

below the average for their respective ages in the Dales group, but the same children were above the Dales average in the Reasoning test.

To trace this further the writer visited some of the smaller Yorkshire Mill towns which have grown up since the Industrial Revolution, and which were to some extent populated by emigration from the Dales in the latter part of the last century. The children in the elementary schools of three of those towns whose families were of Dales origin were tested and the results confirmed those already obtained at Leeds—for most of the children were below the average in memory capacity of country children of the same age, but a small group whose grandparents or great-grandparents had come from Arkengarthdale showed the same marked ability which had been found in some of the boys and girls in the schools in the very remote valleys.

To ascertain whether any decrease of mental capacity could be traced after several generations of town life, the individual marks for the Analogies test of about 100 boys and girls whose families had lived in Leeds for three or more generations were compared with the averages for the same age and sex of the whole Leeds group. The results showed no indication of any inferiority in reasoning capacity in the case of these children—while the age norms for the smaller numbers corresponded closely with those for the larger groups.

(7) THE VALUE OF MENTAL TESTS TO SHOW THE INCREASE OF CAPACITY WITH AGE.

The extent to which physiological changes marking the various changes of development cause, or are accompanied by, intellectual changes of a qualitative kind not depending upon previous experience, is pointed out by Dr Schmitt[1], as one of the still unanswered problems of genetic psychology. The limitations of the application of laboratory methods to such problems are many and obvious, but a series of reliable tests that would measure the intellectual growth of the child from year to year, independently of the increase of acquired knowledge gained by experience and formal instruction, would be of immense service in

has already been pointed out, and the fact that Leeds children of the same origin are exceptions to the general inferiority of memory capacity in towns as compared with country children, is in agreement with Professor Karl Pearson's estimate of the influence of heredity as being much greater than that of environment when it is a case of a dual influence. Several of the Leeds children's connection with the Dales went back to their great-great-great-great-grandparents.

[1] C. Schmitt, Ph.D., "Standardization of Tests for Defective Children," *Psy. Monographs*, XIX. No. 3, July 1915.

providing a scientific basis for the training given in the school, and would throw fresh light on many questions raised in research like the present, which are of manifest significance to the theory of education, such as the interrelation of mental traits, the ages at which definite changes appear in different functions, the correlation between the physical and mental age of the child, the relative influence of heredity and environment. The great need seems to be for some standardisation of tests to avoid the present incomparability of results due to slight differences in the methods of experimenters.

Table XXV gives a summary of the result of the several experiments. The age at which the capacity tested appears to reach its maximum development is indicated. This is at 12 years in the Plunger test, 13 in the Memory for Narrative, and 14 in the Memory for Unrelated Words and Tapping, while in the other tests there is increase of ability with each succeeding year; this is very marked at age 16, but as the averages were taken from under 20 subjects the figures are merely suggestive, and were not for this reason included in the results of the several tests.

Thorndike[1] suggests that when a single figure is given to represent the changes with age in mental traits there is an almost inevitable tendency to assume that all children show that amount of change, the growth of averages, he says, does not accurately describe and may positively misrepresent the real growth of the individuals in the group—the rate of change as well as absolute ability being variable. Thorndike further contends that the children tested at one age need not represent what the children of a lower age will become and that the development of mental traits with age cannot be adequately measured by taking samples of each age, but only by repeating the measurements upon the same individuals. To discover how far the norms obtained for the various tests are representative of the different ages and to examine individual variations in the rate of change, three of the experiments, the Tapping, Dotting and Reasoning tests, were repeated after twelve months at the Oxford schools on various age groups. These tests were chosen as those least influenced by previous practice, for it would be difficult otherwise to determine how far the results might be due to a year's growth or to the repetition of the test.

In the 1914 Dotting experiment the difference of the averages at 10 and 11 years was 25, which was taken to represent a year's increase

[1] *Educational Psychology*, Second Edition, p. 110.

in the child's capacity of Attention. In 1915, 38 girls of 11[1] who had comprised part of the 10 year old group of the year before were retested and the new average 97 corresponded closely with the average of 96 obtained for age 11 in 1914. The average gain in the capacity of attention for the children retested was 20, the mean variation of the increase being very large, *e.g.* 14·6. Thus the age norms obtained from the 11 year old group represents what the 10 year old group becomes, but Thorndike is right in pointing out that the average increase does not represent the real changes of the individuals comprising the group. Of the children retested five made exactly the same scores as the year before, while three showed an actual loss in the capacity of attention. In spite of this a high correlation (·74) was obtained between the performance of the same subjects in the two experiments.

In the Tapping test the average for 11 years in 1914 was 266·2, the average of the same group in 1915 was 269·2. The gain of the 11 year old group on their average of 10 years (259·2) was thus 10 taps, while the difference between the averages at 10 and 11 in 1914 was 7 taps, but again individual differences in the rate of change were very great. In the Reasoning test, repeated at ages 11, 12 and 13, the new average corresponded closely with those already obtained, with the same large individual difference in the degrees of improvement.

These results then seem to indicate that if sufficiently large numbers are tested, the averages really are representative of the specified age and social standing.

Changes in the development of mental capacity from year to year are very small compared with the great individual differences found between children of the same age. The capacity for voluntary concentrated attention and sustaining an effort (tapping) show the most gain from year to year, and are the only instances in which decided breaks in ability occur, in the former marked changes appearing at 8, 11, and 14 years, and in the latter at 7, 9 and 14.

There is no one age at which there is any all round gain in ability, definite changes in capacity appearing at different ages in the various functions, and any special development at one age seems to be almost invariably followed by a period of readjustment; for instance, an increase in speed of 4 seconds in the Plunger test at age 6 is followed by

[1] The original group consisted of 51 girls but 12 of these had been tested less than a year previously so could not be included, and one girl had left the school. The average for the smaller number in the 1914 group was 77, the average obtained for the whole group being 71.

no increase in speed between 6 and 7, the average for 6 and 7 years being the same $24\frac{3}{5}$. At 7 years in nearly every test, there is less increase of capacity compared with that of the year immediately before and after. At 8 years there is a considerable gain in the Dotting test, at 9 in the Tapping and Alphabet tests, at 10 years little development of capacity occurs, while at 11 marked changes appear in the Dotting and Memory tests, but not in the Motor tests. At 12 there is a gain in both tests of Attention, but loss in almost every kind of Memory test. At 13 gain in capacity is less than between 11 and 12. At 14 comes another gain in the power of sustained voluntary Attention, accompanied by loss in the tests of Memory and in one of the Motor tests.

Although the average increase of capacity is very small from age to age yet there is naturally a considerable difference between the performance of children of 5 and 15, the summary showing a gain of 11 seconds in the time taken for the Plunger, of 124·8 in the Tapping test, of 126 more circles crossed out in the Dotting test and of 11·3 letters and 14·5 figures in the Alphabet and Number tests.

Table XXV.

Summary of Age Norms.

Oxford Higher Elementary Girls.

	Motor tests		Discriminative selection			Memory				Analytic and synthetic apperception		
Age	Plunger	Tapping	Alphabet	Number	Combined	Narrative	Related words	Unrelated words	Attention. Dotting	Discs and Sen.	Spot pattern	Reasoning. Analogies
5·5	$28\frac{2}{5}''$	172·9	3·4	2·7	—	—	—	—	15	—	—	—
6·5	$24\frac{3}{5}''$	185·2	3·0	4·6	—	—	—	—	27	—	(10·0)	—
7·5	$24\frac{3}{5}''$	199·3	6·9	5·8	—	—	—	—	37	41·6″	(8·0)	—
8·5	$21\frac{1}{5}''$	209·8	6·8	7·7	14·9	(16·5)	—	—	54	40·8″	11·1	—
9·5	$19\frac{3}{5}''$	240·7	9·3	9·5	18·5	16·9	13·9	—	67	36·7″	12·0	28·8
10·5	$19\frac{3}{5}''$	259·2	11·7	9·4	19·6	18·6	13·8	4·3	71	34·0″	12·5	34·0
11·5	18″	266·2	11·9	12·3	22·4	22·1	13·9	3·7	96	32·7″	13·1	39·8
12·5	**17″**	281·7	11·2	11·0	27·6	21·4	13·8	5·0	112	25·9″	13·6	43·0
13·5	$17\frac{1}{5}''$	288·5	11·3	13·4	27·2	**23·5**	14·6	5·7	118	29·4″	14·3	45·1
14·5	$17\frac{2}{5}''$	**301·4**	13·6	15·4	30·5	22·7	15·3	**8·0**	139	24·7″	15·8	46·6
15·5	$(17\frac{2}{5})''$	297·7	**14·7**	**17·2**	31·5	23·3	—	6·0	142	24·3″	16·4	51·8
16·5	—	—	—	—	**(32·0)**	—	—	(6·6)	**(175)**	**(20·4)**	**(20·3)**	**(53·4)**

The following conclusions are indicated by the result of the experiment.

1. In diagnosing mental ability the same test applied at different ages has greater value than a series of what Mr Burt has termed

"externally graded" tests. If the test material is graded in any way it should be after the difference in capacity at the various ages has been determined experimentally, for only then can the graded test have any scientific value.

2. There is only a very low correlation of the mental with the physical age of the child, the tests bring out striking differences in innate capacity between children of the same age and in the same form at school, and therefore at approximately the same stage in acquired knowledge and experience.

3. The degree of correlation of the Motor with the Mental tests gives no support to the evidence of any close relationship between motor and mental ability; the small correlation that is found decreases with age, and the graphs of individual children show in almost every case an inverse relationship of the Motor tests with those at the higher mental levels.

4. The interrelation of mental traits varies at different ages, the degree of correlation increasing and decreasing or turning into inverse correlations. The correlation of Motor tests with the higher tests appears to decrease, the correlation between tests at the higher mental levels to increase.

5. Intelligent children are endowed with better memories, but the correlation of Intelligence with Memory is highest in cases where the test measures true Memory or Meaning. There appears to be no correlation of Memory with age, and only a low correlation between Reasoning power and age.

6. There is a positive correlation of intellectual ability with ability to profit by learning.

7. There are marked mental differences between town and country children, the town children excelling in the tests involving speed and in the Reasoning test, the country children being invariably superior in the Memory test.

8. Changes in the development of mental functions from year to year are very small compared with the great individual differences found between children comprising the same age group. Marked gains in ability appear at different ages in the various functions, and any special development at one age seems to be followed by a period of readjustment in which there is little or no increase in capacity.

9. With the exception of the Motor tests, and some of the Memory tests, there is within the limits of the experiment, no indication of the age at which innate capacity reaches its maximum development.

APPENDIX

The Analogies and Memory for Narrative tests were repeated under identical conditions for purposes of comparison with the Oxford experiment, at three secondary schools for girls and the results as well as those of the several elementary schools for both boys and girls are given below in the following summaries.

The difference in memory capacity between the various ages and social groups is very small under the age of 11, there is, for instance, a considerable difference between the averages of the Oxford Higher Elementary and the Worcester High School girls, but they are practically equal at nine years.

North Country children appear to be superior in reasoning capacity, the averages for the Dales schools being much higher than those of the Oxfordshire villages, and the contrast between the two groups is greater than in the Memory test. Further evidence of the strength of reasoning capacity in North Country children is given by a comparison of the Leeds Elementary with the Oxford Higher Elementary averages, for except at age 13 when the difference is negligible[1] the Leeds girls show much more ability in the test—being at 12 years in advance of the Oxford girls at 14, and this is the more striking as the Leeds schools, though in the better class districts, were typical elementary schools, while the Oxford schools were all fee paying schools, and the children were thus to some extent a selected class socially.

Memory for Narrative.

Summary.

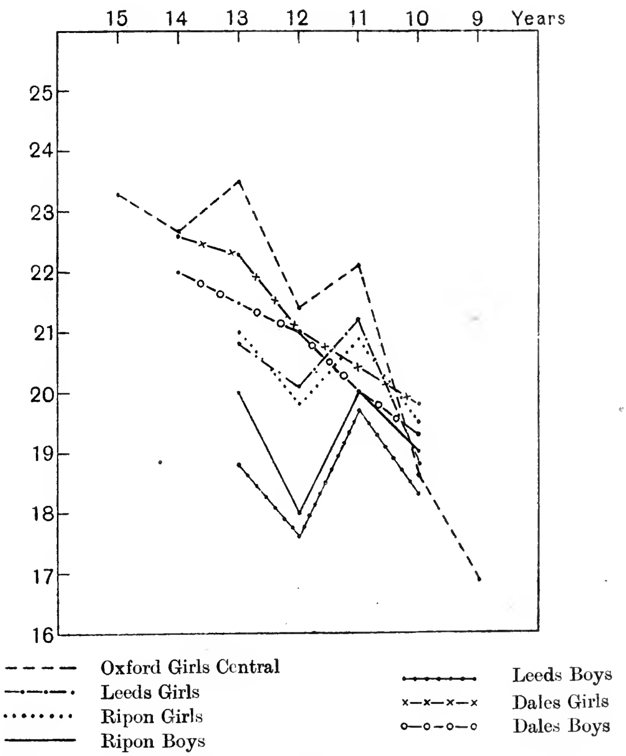
	Girls' Secondary Schools		Higher Elem. Schools		Rural Elementary Schools				Town Elementary Schools				
					Oxfordshire Villages		Yorkshire Dales		Leeds		Ripon		Oxford Girls
	Worcester High School	Ripon and Middlesboro' High Schools	Oxford Girls	Oxford Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys	
Age	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.
16·5	—	22·5	—	—	—	—	—	—	—	—	—	—	—
15·5	—	23·3	23·3	—	—	—	—	—	—	—	—	—	—
14·5	25·0	21·3	22·7	22·3	—	—	—	—	—	—	—	—	—
13·5	25·0	22·8	23·5	20·0	21·9	20·0	22·5	22·0	20·9	18·8	21·0	20·0	20·5
12·5	24·6	22·2	21·4	19·8	21·2	18·5	22·3	21·5	20·1	17·6	19·8	18·0	17·0
11·5	23·2	20·2	22·1	—	20·6	17·7	21·0	21·0	21·2	19·7	20·9	20·0	19·7
10·5	22·0	—	18·6	—	20·9	16·0	19·8	19·3	18·8	18·3	19·5	19·1	18·3
9·5	16·7	—	16·9	—	—	—	—	—	—	—	—	—	—

[1] The probable reason for the low averages at age 13 in Leeds has already been pointed out. The group owing to the war not being as representative as usual; the Oxford results on the other hand were uninfluenced by any such conditions.

REASONING TEST.

Summary.

Age	Oxford H. Elem. *Girls*	Oxfordshire Villages Elem. S. *Girls*	Elem. S. Y. Dales *Girls*	Elem. S. Y. Dales Boys	Elem. S. Leeds *Girls*	Elem. S. Leeds Boys	Elem. S. Ripon *Girls*	Elem. S. Ripon Boys
15·5	51·8	—	—	—	—	—	—	—
14·5	46·6	—	—	—	—	—	—	—
13·5	45·1	31·2	47·9	52·4	45·4	50·5	36·0	40·6
12·5	43·0	30·0	40·8	43·6	49·4	51·4	39·6	36·0
11·5	39·8	30·0	34·2	39·4	47·5	50·4	35·5	34·7
10·5	34·0	25·0	32·1	32·5	42·0	43·4	30·0	31·3
9·5	28·8	—	28·0	26·0	—	—	—	—
8·5	—	—	—	20·4	—	—	—	—



Memory for Narrative showing the loss at age 12 in town children.

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CHILDREN'S INTERPRETATIONS OF INK-BLOTS.

(A STUDY IN SOME CHARACTERISTICS OF CHILDREN'S IMAGINATION.)

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1. Introduction.

The object of the present experiment was to study Imagination in young children, in so far as this can be done by means of ink-blots. Work on these lines has already been done by Dearborn[2] in "A Study of Imagination"; by Kirkpatrick[3] in "Individual Tests of School Children"; and by Miss Sharp[4] among her studies in "Individual Psychology." In every case the material used in this connection was ink-blots, standardised or otherwise. Bartlett[5] too, in "An Experimental Study of some Problems of Perceiving and Imaging" employed ink-blots in part of his work.

[1] The work has been done under the guidance of Miss I. B. Saxby, Lecturer in Education in the Women's Education Department of the University College of South Wales and Monmouthshire, without whose unfailing help and interest it could never have been undertaken and carried through.

[2] *Amer. J. of Psychol.* 1898, IX. 183–190.

[3] *Psychol. Rev.* VII. 274–280.

[4] *Amer. J. of Psychol.* 1899, X. 329–391.

[5] This *Journal*, 1916, VIII. 222–266.

Dearborn's results are largely concerned with times of reaction; and the *first* object suggested by each blot was, for him, of special significance. His subjects ranged in age between 18 and 62 years, and averaged nearly 35 years. Kirkpatrick's work in this direction was incidental to a more extensive enquiry into other matters: he employed children of all ages from Grade I up to Grade VIII; while Miss Sharp[1], one of whose aims was "to find the value for Individual Psychology of experimentation applied to the more complex mental activities, as well as the practicability of certain specific tests," used in her work seven advanced students in the Sage School of Philosophy. For his tests on 'Imaging' Bartlett employed thirty-six subjects of whom only four were children.

Thus these investigations were mainly limited to adults: the present experiment was planned to see what results might be obtained from young children, and what relation these would bear to those obtained from adults in any of the above mentioned cases.

2. METHOD OF EXPERIMENT.

(i) The ages of the children taking part in this experiment ranged between 7 and $7\frac{1}{2}$ years. Of the ninety-seven children employed, fifty-two were boys and forty-five girls; and they were drawn from the Infant Departments of two elementary schools[2] in the same district, so that the type of child was similar in the two cases.

On the whole the children were well cared for, and, with few exceptions, appeared to be well developed physically: most of the fathers were shopkeepers or artisans.

The material used was series 1–10 of the standardised ink-blots mentioned by Whipple[3] in his account of Miss Sharp's work on Imagination.

(ii) Ten black blots were used in all, and these were shown to the subjects individually on two separate occasions, five cards each time with an interval of fourteen days between the two tests. This procedure was adopted on account of the high fatiguability of young

[1] *Amer. J. of Psychol.* 1899, x. 388.

[2] The writer is indebted to Miss Warman, Head Mistress of the Infants' Department of Gladstone Road School, Cardiff; and also to Mrs Tate, Head Mistress of the Infants' Department of Allensbank School, Cardiff, for their great kindness in allowing the experiment to be conducted among their pupils, and in giving her every opportunity for seeing the children as often as was necessary.

[3] *Manual of Mental and Physical Tests* (Edition 1910), 430.

children; the time of day and the room in which the experiment was conducted were the same on the two occasions, so that the conditions were as nearly alike as possible. As it was intended to treat the data more from a qualitative than from a quantitative point of view, the children were allowed reasonably unlimited time in which to name the associations that occurred to them.

Any explanation in words only, of what was required, would probably have been misunderstood by such young subjects. For this reason the experiment in each case was begun by showing the child one or more 'practice cards.' He was told that these had been made by throwing ink on post cards, and was asked of what they reminded him, or what they seemed to him to resemble. In most cases the child soon grasped his part of the work; in a few cases, however, it was necessary to show four or five 'practice cards' before the subject responded.

The fact that the experiment was begun with a series of practice cards, may prove to be a point for criticism, because (i) it might have occasioned a loss of interest by the time the real cards were shown, and (ii) because the combined time spent over the practice and the work itself might have caused fatigue. As a matter of fact, however, the children were the more eager to see the 'real' cards, if for no other reason than that these were *not* the real ones; and the time spent over this preliminary work—in most cases a matter of three or four minutes—was not of sufficient duration to tire the subject.

As soon as each child understood what was required of him, he was shown cards 1–5 of the standardised series, in turn. All the associations named by him, together with the total time taken in giving them, were recorded in writing by the experimenter, since the children were too young to record their own associations. A fortnight later as nearly as possible at the corresponding time, the cards 6–10 were shown to each child in the same way.

3. Discussion of Results.

(i) *Method of Classification adopted.*

It was most convenient in the first place to divide the various associations given by the children, into groups classified according to type of association, somewhat after the method employed by Miss Sharp[1] in part of her work on "Individual Psychology." The associations distributed themselves into the following groups:

[1] *Op. cit.* 371–373.

A. Animal Associations.

I. Animals which were apparently seen in their entirety.

(i) Domestic animals, *e.g.* "Cat," "Dog."

(ii) Those the children were unlikely to have seen, *e.g.* "Bear," "Elephant," "Camel."

(iii) Those the children probably had seen, other than Domestic, *e.g.* "Monkey," "Rat."

II. Animals of which only a definite part was mentioned, *e.g.* "A dog's face," "A rabbit's ear."

B. Human Beings.

I. Humans which were apparently seen in their entirety, *e.g.* "Like a baby," "Like a woman."

II. Humans of which only a definite part was mentioned, *e.g.* "Like a lady's hand," "Like a man's leg."

C. Mythological Creatures, *e.g.* "Dragon," "Giant."

D. Vegetables, *e.g.* "A tree," "A banana."

E. Wearing apparel, *e.g.* "A Frenchman's hat," "A boot."

F. Objects such as the children would be likely to have seen frequently in the house or street, *e.g.* "A Hammer," "A Ball," "A Jar," "A Lamp post."

G. Objects likely to have been suggested by the War, *e.g.* "A Bomb," "A Zeppelin with people in it."

H. Architecture, *e.g.* "A Castle," "Steps," "A Bridge," "A Church."

K. Landscape, *e.g.* "A Mountain of snow," "A little river," "Something like a hill with two little children sitting on top of it."

M. Reminiscences, probably from school or from books, *e.g.* "A Map," "A Hindoo like they have in India."

P. Associations which merely described the blot, *e.g.* "It looks now as if that part there was round—it isn't though—and all dotty specks."

(ii) *Discussion of Associations obtained*[1].

The average of the total number of associations per child was 35·74, with a standard deviation of 27·64. The lowest number of associations obtained by any individual for the whole series was 1, the highest 147; the subject in each case was a girl. The average number of different classes of associations per child was 8·47 (S.D., 2·58); with a single exception all the children gave associations of different classes. The fact that one of a certain class was mentioned did not necessarily involve

[1] Before quoting from the Tables, it will be convenient to note the following points:

(*a*) The formulae used for obtaining the standard deviation and the standard error of difference, are those given in Yule's *Introduction to the Theory of Statistics* (Edition 1912), 134 and 346 respectively. In the present experiment, results are considered to have suggestive value only, when the difference is more than twice, and less than three times the standard error; and to have no definite significance unless it is at least three times the standard error.

(*b*) Asterisks * denote differences which are less than the standard error.

(*c*) Brackets $\underbrace{\quad}$ indicate a comparison between the columns so bracketed others of the same type being given: the average per cent. of the number of cases for each child, when one object did suggest another of the same class, was 6·16 (S.D., 2·81), as against an average per cent. of 2·56 (S.D., 1·5), when one association of a certain type was given by itself. Kirkpatrick[1] found that "most of those who named three or four of the spots named more than one object of a class as, two kinds of vegetables or two animals, showing that the presence of one idea of a group made others of that group more suggestible."

Before turning to a consideration of the various methods of description employed by different children, there are a few further points of interest about the actual associations which are worthy of notice.

(*a*) Firstly, there was considerable variety in the associations obtained, not only in the interpretation of a blot by one subject, but in the interpretations of the same blot by each of the ninety-seven children. The same child saw Blot 8 as: "A walking-stick"; "a gun"; "a gas"; "a hill"; "an eye"; "a chicken with his beak open"; "a man sitting down"; "two fingers"; "an arch and four pieces of grass."

Blot 8.

To other children the same blot appeared like:

(*a*) "A duck and his shadow."

(*b*) "A lamp."

(*c*) "Two ducks' heads," "a flower pot."

(*d*) "A man with two candlesticks, dropping the grease down, and letting a big ball fall down."

(*e*) "A scales" (balance).

(*f*) "A little Dutch boy with sticks in both his hands," "a bottle," "Punch and his long nose."

(*g*) "Two pegs on the floor."

(*h*) "A man's chest."

(*k*) "A bell."

(*m*) "A cottage loaf," etc. etc.

[1] *Op. cit.* 276.

Dearborn[1] found that in the case of no blot did more than 40 per cent. of his subjects agree on any one suggested object, and that there was at times no agreement whatsoever. In the present experiment the children were considered to agree in the interpretation of any of the blots, provided that the main idea suggested was the same, *e.g.* "Lady with a hat on," "Lady with a long nose."

Blot 6, which gave the highest average of associations per child, for any one blot, viz. 4·09 (S.D., 4·23), was interpreted by 60 per cent. of the children as a woman, among other things: the lowest percentage of agreements in this case was 2·6.

Blot 6.

Blot 2.

Blot 2 gave the lowest average of associations per child, viz. 2·46 (S.D., 2·13) and 26 per cent. of the children interpreted it as a man; the lowest percentage of agreements was again 2·6.

It is noticeable that blots 6 and 2 which gave most and fewest associations respectively, viz. 397 and 239, are strongly suggestive of human beings; and that blot 2, in the writer's opinion, at least as suggestive of a man as blot 6 is of a woman, should yield fewer associations by 158 than blot 6. Possibly the children found the suggestion of a man in the case of blot 2 so strong that they were unable to get away from it and to re-construct the data into an entirely different whole; it may be that the suggestion of a woman in blot 6 was less strong, and that consequently they found wider scope for the use of their imaginative power.

(*b*) In all 54·4 per cent. of the total number of associations were of animals and of human beings. This agrees closely with the results

[1] *Op. cit.* 187.

obtained by Bartlett[1], who found that of 1068 suggestions given by his subjects, 635 were of some animal or of a human being, *i.e.* 59·5 per cent.

According to one of Bartlett's[2] subjects the tendency is due to the fact that "it is living things that are most noticeable and interesting." This correspondence between his results and mine is the more interesting because Bartlett's subjects were mostly adults, mine, children of 7: it suggests that there is no great difference between adults and young children in this respect.

(*c*) It is further interesting to note that there is even at this early age a certain amount of differentiation in interests according to sex. This may be seen from Table I, which gives the average number of associations per child given under each heading by the boys and the girls respectively. The following results can be deduced from this Table:

(i) War Associations.

Among the girls these are conspicuous by their absence, while the boys show an average of 2·6 per child, the difference being 3·99 times the standard error in favour of the boys. Of the boys 80·7 per cent. named some typical war association, *e.g.* "a bomb"; "a German's hat"; "a zeppelin"; while only 23·7 per cent. of the girls mentioned associations which might have arisen from what they had heard concerning the war.

(ii) Domestic Animals.

In the case of domestic animals the girls probably show the higher average, *i.e.* 5·13 per child, as compared with 3·21 per child for the boys. The standard error of the difference between boys and girls is ·78. There is a difference of 2·46 times the standard error in favour of the girls.

(iii) Human Beings.

Here there seems to be a tendency in the same direction as in the case of associations with domestic animals, the difference being 2·77 times the standard error in favour of the girls.

(iv) Landscape.

The average obtained by the boys appears to be higher than that of the girls, for the difference between them is 2·31 times the standard error.

Reference to Table I will show that all other differences are negligible. To sum up: marked differentiation of interest between boys and girls is only shown in the case of War associations; the results suggest that

[1] *Op. cit.* 254. [2] *Op. cit.* 254.

TABLE I. *Primary Classification of Associations.*

	Human beings: Seen as a whole	Human beings: Part only mentioned	Total animal associations given	Animals: Domestic and well known	Animals: Non-domestic but well known	Animals: Probably known to the children by name or picture only	Animals: Domestic and otherwise of which only a part was mentioned	Vegetables	Wearing apparel	Objects such as the children would be likely to have seen frequently in house or street	Associations likely to have been suggested by the War	Architecture	Landscape	Associations which merely described the blots	Reminiscences probably from school or books	Mythological creatures
Averages for girls	7·93	3·08	9·75	5·13	2·13	·62	1·86	2·8	1·4	4·46	·48	1·31	2·06	·44	·26	·22
for boys	6·9	3·59	8·51	3·21	1·96	·9	1·98	3·32	1·17	5·09	2·61	1·71	3·53	·52	·32	·21
Standard deviations for girls	6·68	4·38	85·10	4·78	2·46	—	3·67	3·59	2·98	5·16	·85	2·09	2·72	—	—	—
for boys	4·51	5·24	30·22	2·31	2·11	—	2·57	4·24	1·65	4·81	2·55	4·73	5·68	—	—	—
Standard error of difference between girls and boys	·37	·98	13·36	·78	·47	*	·65	·79	·85	1·02	·53	·73	·64	*	*	*
Excess of boys over girls in terms of standard error	$-2·77\sigma$	$+·52\sigma$	$-·09\sigma$	$-2·46\sigma$	$-·36\sigma$	—	$+·18\sigma$	$+·65\sigma$	$-·27\sigma$	$+·62\sigma$	$+3·99\sigma$	$+·55\sigma$	$+2·31\sigma$	—	—	—

* Such differences throughout the tables are less than the standard error.

σ = Standard error.

there may also be a slight tendency for greater interest in Landscape on the part of the boys; and in Human beings and Domestic animals on the part of the girls. The rest of the Table requires no comment.

(iii) *Active versus Passive Imagination.*

Miss Sharp seems to have used the method of blots as a test of Passive Imagination. She says[1], "the number of objects seen in the blot, their kind and the manner of reporting them, gave information in regard to the Passive Imagination of the individual tested."

The following observation made during the course of the work here described tends to support the view expressed by Whipple[2] that although the ink-blot test is commonly regarded as a test of Passive Imagination, yet in practice, because the subject is quite likely *to search actively* for associations, the mental activity concerned is perhaps more allied to Active than to Passive Imagination.

It was noticed during the course of this experiment that some of the subjects did not view the blot as a whole, but either covered a part of it with the hand, or pointed to one particular portion at a time and described that apart from its surroundings (Table II). This method

TABLE II. *Classification of associations into Whole or Part.*

		Whole	Part
Averages			
	for girls	20·8	13·4
	for boys	20·2	14·4
Standard deviations			
	for girls	26·8	15·6
	for boys	20·9	18·4
Standard error of difference			
	for girls	4·6	
	for boys	3·9	
Difference in terms of standard error			
	for girls	1·6	
	for boys	1·5	

was adopted by 69·07 per cent. of the children. They were allowed to turn the cards in any direction they wished and to move head and arms freely, unlike Dearborn's[3] subjects who were instructed "to look at the blot card, always right side up turning neither the card nor the head: to try to employ the whole character if possible, not allowing it to separate into parts while being observed."

[1] *Op. cit.* 354. [2] *Op. cit.* 430. [3] *Op. cit.* 184–185.

It is possible that the shape of the blots may have accounted for the tendency of some of the children to break them up; Bartlett's[1] view supports this theory for he says: "sometimes it was the general shape of the blot, and sometimes it was an outstanding feature that played the chief part in determining the suggestion." Be this as it may, however, the fact that there was this definite tendency to search actively for the associations themselves, shows clearly that Active Imagination was being used.

As a matter of fact it seems doubtful how far true Imagination can ever be really Passive.

Imaging may consist of two types:

(i) Reproductive Imaging which is equivalent to memory, and

(ii) Productive Imaging which is imagination. This last is capable of two forms —Constructive Imagination and Interpretative Imagination. If this view is accepted it will be seen that such a thing as Passive Imagination pure and simple is not possible, and that as a matter of fact both Constructive and Interpretative Imagination are of necessity active in type: for the one consists in interpreting one's own data in such a way as to form a new whole, the other in interpreting data derived from another source; and this very act of interpreting can hardly be purely passive.

What is often spoken of as Passive Imagination is probably, as a rule, merely sub-conscious working of ordinary Constructive or of Interpretative Imagination: *e.g.* a person sitting before a glowing fire, is suddenly struck by the likeness of a particular cavity to a human face or some other thing, even though he has not been actively searching for 'pictures' in the fire. This is comparable with the case of some one who, perhaps, is unable even with much effort, to remember a name or tune; after some time has elapsed he finds the one or the other on his lips, with no conscious attempt at recall.

This last is attributed to sub-conscious working of memory: possibly the former is similarly due to sub-conscious working of Constructive or of Interpretative Imagination. If this is so, then the phrase 'Passive Imagination' is a contradiction of terms: in any case the fact that 69·07 per cent. of the children interpreted the blots in parts and not as wholes shows that they, at least, were using Active Imagination.

(iv) *Further Classification of Associations into Non-constructive and Constructive.*

During the course of her work Miss Sharp[2] says, "a particular blot may call up in the mind of a subject, through association, a number of objects, similar to this in form, and he enumerates the objects one after another; while to another individual the same blot seems filled with pictures representing some action or situation, which are reported, often with touches of fancy or sentiment. This difference in the reports is sufficiently marked and sufficiently constant to form a basis

[1] *Op. cit.* 253. [2] *Op. cit.* 272–273.

for the classification of the individuals into two classes: one class representing the *Constructive* or imaginative type, characterised by the putting together of concrete details in such a way as to form a significant whole: the other class representing what may be called a *Matter-of-fact* or scientific type, characterised by a process more purely analytic in its nature." She then quotes the following by way of illustration:

I. *Associations Few and Non-constructive.*

An eagle. Stuffed turkey. Head and neck of a musk rat.

II. *Associations Many and Varied but Non-constructive.*

Ghost. Tadpole. Lizard. Ichthyosaurus. Mountains, etc.

III. *Associations Numerous and Constructive.*

Giraffe. Prehistoric bird in flight. Fairy riding on a humble bee. Bit of tropical jungle, with trailing grey mosses and pools of water.

Another Classification in the present experiment was suggested by Stern's[1] four stages of methods of description, viz.:

(i) Till 8 years old 'thing stage' *e.g.* "a bed"; "a table."

(ii) 8 and 9 years, 'action stage' *e.g.* "the man is crying."

(iii) 10–13 years beginning of 'relation stage' *e.g.* "a table in the centre of the room."

(iv) After 13 years beginning of 'quality stage' *e.g.* "a ceiling with beams across it, probably of oak."

The form of classification adopted in this instance was a combination of that of Miss Sharp with that of Stern. The two classes Constructive and Non-constructive associations formed the basis: associations were included in the former class provided that they involved a combination of two or more objects or ideas, which need not inevitably occur together as long as any relationship was expressed between them, and as long as they formed one 'picture,' *e.g.* "a pot with a little plant in it."

Two or more associations mentioned together, though unrelated, were placed in the class Many and Non-constructive associations, *e.g.* "a pot with a tree, and that's the ground." A single association belonged to the class Few and Non-constructive, *e.g.* "a gun"; "a bridge."

An attempt to take account of the classifications used by both Miss Sharp and Stern, gave rise to the following sub-divisions:

(*a*) *Non-constructive Associations* without either action or qualification, *e.g.* "A lady."

[1] W. Stern, *Die Aussage als geistige Leistung u.s.w.*, 1 Teil (1904), also Stern und Lobsien, "Aussage und Wirklichkeit bei Schulkindern," *Beitr. z. Psychol. d. Aussage*, 2 Heft (1903), S. 26 ff.

(*b*) Ditto with action only, *e.g.* "A lady *dancing*."
(*c*) ,, with qualification only, *e.g.* "A *broken* sledge."
(*d*) ,, with action and qualification, *e.g.* "An *old* lady *dancing*."

Constructive Associations with the same sub-divisions:
(*a*) *e.g.* "A man knocked on his head by a rock."
(*b*) *e.g.* "A dog *sitting on a mat*."
(*c*) *e.g.* "A man *with a big mouth* and spikes coming out by his nose."
(*d*) *e.g.* "A *little stick pointing* to a man's head."

Table III summarises the results obtained by this method of classification. In the case of both Non-constructive and Constructive

TABLE III. *Classification of associations as Non-constructive and Constructive.*

		Non-constructive			Constructive			Total	
		Without action or qualification	Action only + action with qualification	Qualification only + action with qualification	Without action or qualification	Action only + action with qualification	Qualification only + action with qualification	Non-constructive	Constructive
Averages	for girls	20·1	2·9	4·8	2·4	3·6	1·7	27·4	6·9
	for boys	23·9	3·1	4·0	3·2	2·5	1·1	30·7	6·4
Standard deviations	for girls	19·9	2·5	6·8	3·5	5·4	2·5	20·4	8·2
	for boys	21·7	2·9	6·1	3·0	3·8	1·4	24·9	5·7
Standard error of difference	for girls	—	1·5		—	·89		3·3	
	for boys	—	*		—	·81		3·5	
Difference in terms of standard error	for girls	—	1·3		—	2·13		6·3	
	for boys	—	—		—	1·73		6·9	

Brackets indicate comparison between the columns bracketed.

associations, the second and third columns respectively, show the result of throwing together all of either type of association which include, in the former case, action whether with or without qualification; and in the latter, qualification, with or without action. The results point to three definite conclusions:

(*a*) There is a marked preponderance of Non-constructive associations: this result agrees with Stern's.

(*b*) There is no significant difference in this respect between boys and girls.

(*c*) There is no tendency for qualification rather than action to be used, though possibly the girls show a slight preference for action in Constructive association, as indicated by the difference in terms of standard error, namely, 2·13 between associations including action with or without qualification, and those including qualification with or without action. The fact that Non-constructive associations predominate over Constructive, in the case of both boys and girls, is supported by Stern's conclusion that the 'thing stage' predominates until 8 years old: for the children in the present instance were between 7 and $7\frac{1}{2}$ years, and the term 'Non-constructive association' as interpreted in this piece of work, is in some ways analogous with Stern's 'thing stage.'

(v) *Methods of Description adopted by subjects.*

One further classification was attempted, based on the method of describing the blots employed by the children: the results are given in Table IV the headings of which require some explanation. By 'Identity' is meant the use of words implying conviction that the blot is really a reproduction of the association suggested, *e.g.* "it *is* so and so." 'Similarity' on the other hand, means that words implying a doubtful attitude on the part of the subject were used, *e.g.* "it is like," or "something like" so and so.

The writer is aware that the inclusion of statements prefixed by "it is like" and "it looks like" among Similarities, involves the assumption that the children who adopted this particular method of description recognised difference between the blot itself and the object it suggested; and that statements in this form are not equivalent to those such as "it is exactly like" so and so. She is aware too, of the fact that children of 7 and $7\frac{1}{2}$ years may have been too young to recognise any difference between such an assertion as "it is like" and one such as "it is": the difficulty is purely one of language. However, she considers the assumption to be a legitimate one on these grounds: (i) the children knew that the shapes were only made by ink-blots; and they were told clearly before the test began that "the blot is not really a picture of anything." Moreover, they were asked to say what they thought it *looked like* and not what they thought it *was*.

(ii) The fact that 68·93 per cent. of the associations were prefixed by the statement "it is like," or "it looks like," suggests an improbability that the children would agree in seeing identical likeness, when they had previously been told that the blots were not "real pictures."

It will be seen from the Table that the average of Similarities is larger than that of Identities, the difference in terms of standard error being 8·5 and 6·1 in the case of boys and girls respectively. Even assuming that all the 'no prefix' associations were meant as 'Identities' by the children who gave them, the difference in terms of standard error between Identity + No Prefix, and Similarity for boys and girls respectively is 4·59 and 3·29 both of which are significant. The inference is that the children preferred a method of description such as "it is like" or even to use no prefix at all, rather than to commit themselves to a definite statement "it *is*" so and so.

In this connection Kirkpatrick[1] remarks that "the younger children seemed to have no doubt whatever of the spot being a picture of the object they named, while the older children simply said 'it is some like' or 'it looks a little like' a 'dog,' 'cloud' or whatever else was suggested." The younger children here referred to were those of grades 1, 2 and 3. Kirkpatrick[1] says "a very few pupils who had just entered the first grade could not count far enough to consume the whole of the ten seconds"; we may assume therefore that these were extremely young, probably 5 or 6 years old. The results quoted from

TABLE IV. *Classification of associations according to Identity with or Similarity to the real thing.*

		Similarity	Identity	No prefix	Identity + No prefix	Similarity
Averages	for girls	24·7	1·7	7·8	8·97	24·7
	for boys	29·5	1·2	6·3	7·53	29·5
Standard deviations						
	for girls	25·04	4·3	19·5	20·19	25·04
	for boys	23·8	2·4	15·8	15·88	23·8
Standard error of difference						
	for girls	3·8		—	4·79	
	for boys	3·3		—	4·78	
Excess in terms of standard error						
	for girls	$+6{\cdot}1\sigma$		—	$+3{\cdot}29\sigma$	
	for boys	$+8{\cdot}5\sigma$		—	$+4{\cdot}59\sigma$	

Table IV appear to differ from those obtained by Kirkpatrick; but probably those of his subjects who thought the spot was really a picture of the object they named were younger and therefore at a lower stage

[1] *Op. cit.* 275.

of development than those in the present experiment, none of whom were less than 7 years of age. The results then suggest that children of 7, at any rate, have passed this primary stage when they accept the blot as really representative of the object suggested.

4. Summary of Results.

Since the children who took part in the experiment were all drawn from the better artisan class, it is not possible to generalise for children in other strata of society. The following points, however, throw some light on the character of Imagination in these particular subjects.

(i) Children like adults[1] are most interested in animals and other living things (54·4 per cent. of the associations were suggested by human beings and animals); they tend therefore to think of these more readily than of other types.

(ii) (*a*) Boys of 7 are more interested in the War than girls of the same age; the excess of boys over girls is 3·99 times the standard error. They are probably more interested in landscape associations than girls, the difference being 2·31 of the standard error in favour of the boys.

(*b*) Possibly the girls betray keener interest in domestic animals than the boys, the excess of girls over boys in terms of the standard error being 2·46.

(*c*) There is no tendency for the girls to give a higher total of animal associations than the boys, since the difference ·09 of the standard error is insignificant.

(iii) There is a tendency to use Active Imagination: 69·07 per cent. of the subjects interpreted the blots in parts and not as wholes.

(iv) There is a marked tendency among both boys and girls to use non-constructive rather than constructive associations, the difference in terms of standard error in the case of boys and girls respectively being 6·9 and 6·3.

[1] Bartlett, *op. cit.* 255.

APPENDIX

It will probably be of interest to quote a few of the children's replies in detail, for the purpose of illustrating individual differences in power of imagination. The following have been chosen as samples of imaginative and of matter-of-fact types, respectively. The reports are quoted verbatim:

H. C. (Boy of 7 years 2 months, son of a barber.) Very talkative and most interested in the work: seemed to be intelligent and to have a keen sense of humour.

Blot 1.

"Looks like a bull running and a dog jumping over him.

"A butcher boy with his apron blowing out, his hands out and a dog behind him."

"Looks like a dragon-fly from here."

"Looks like a Nanny goat from here."

"Like a man on a bull and the man has a stick hitting him."

Blot 2.

"A man."

"Like a bridge all broke."

Blot 3.

"Looks like a harp."

"A dog sitting down on the ground—there's his nose—his ears."

"A little hill coming down and a pole sticking out of the grass."

Blot 4.

"There's the street and all the houses along."

"Like an elephant laying down."

"Like a pump for water to come out, and the place for water to come out."

Blot 5.

"A loaf of bread."

"Two rocks each side and a lady walking through."

"A field."

"Two big rocks up there."

F. D. (Girl of 7 years 3 months, self-contained, direct and abrupt in manner.)

Blot 1.

No response.

Blot 2.

"Like a rock."

Blot 3.

"Half a table."

"Like a chair that way."

Blot 4.

"Like a cattle's teeth."

"That bit's like a stem."

Blot 5.

"That's like a rock."

E. K. (Girl of 7 years. Had some difficulty at times in finding suitable expression for her ideas, and, whenever she was able, made constant use of gesture by way of illustration.)

Blot 6.

"This here looks like a lady looking in a mirror glass."

"Now its like a man with a long stick in his hand, and a dog behind him, and he's dragging him along."

"Now like a lady sitting on the top of a hill."

"It looks like a lady with a plait down her back."

"It looks like a pussy cat."

A. W. (Boy 7 years 1 month. Appeared to be most matter-of-fact.)

Blot 6.

"Looks like a lady"

Blot 7.

"This looks something like a hill with two little children sitting on the top of it."

"Like a lady and a gentleman falling down."

"Like a gentleman picking like flowers."

"Like a little girl sitting like on the top of a hill."

"It looks like as if there was a lot of little flowers there."

"Like a little girl picking flowers."

Blot 7.

"Looks something like a man—that face do."

Blot 8.

"This now looks like a lady with one stick in front of her and one behind."

"Now like a rug with two rats, one running that way and the other this way."

"It looks like a gentleman sitting up in a tree with two sticks in his hands."

"Now it looks as if here like was a big high tree with a ball on top."

"Like a little pussy cat sitting on a high tree."

Blot 8.

"Looks like a man holding up two sticks."

"Like a man between two sticks."

Blot 9.

"This looks as if there was a gentleman standing on one foot."

"Like a lamp post."

"Like a shakey tree."

"As if there was like a lot of children standing on top and a big tree on a very large hill and a lady coming up to catch the children."

"Looks now as if you was catching hold of it (a hammer) and chopping wood."

"Like a lady standing up and going round." (Here subject turned the card round.)

"Like as if you were turning the card round, it would look like a big round ball."

"That looks like as if a lady was standing on the hill to pick flowers."

Blot 9.

"Looks something like a tree."

Blot 10.

"This one looks as if a lady got her thumb out and looking at it."

"Like a man with a horse on top of a hill and a gentleman sitting on the horse."

"Like a lady is sitting—standing up on the top of a hill with her fingers down like this."

"Like a lady looking at her fingers, with the other hand rubbing something—an ointment—on."

"Like a man going down a hill, catching hold of the reins to pull the horse."

"Looks like two lights on top of a hill."

"Looks now as if a man was just only standing on the hill looking at trees."

Blot 10.

"Looks like an arm."

"As if a lady was sitting back like this and putting her fingers down one by one on a table (I can't see a table, only it looks as if she would be)."

"Like a gentleman on a donkey, and going over and over."

"Like a lady sitting on a hill crying, and her hair's coming out here."

"It looks now as if that part there was round—it isn't though—and all dotty specks."

The total number of associations for the ten blots given by these children together with the number per cent. of the former, which were constructive in type, are given below:

	Total Associations	Constructive Associations
H. C.	29	48 %
F. D.	16	—
E. K.	65	53 %
A. W.	13	8 %

Thus there is considerable variation, not only in the fertility of their imagination, but also in their use of Constructive Imagination.

Among the girls some of the replies of two subjects were of special interest in themselves.

E. P. a girl of 7 years 2 months, gave among others the following associations for Blot 5.

"Like a mother pig with a little baby pig in her arms."

"Like a little girl coming home from school with a witch's hat on."

"Like a father going out for a walk, a sheep with him, smoking a pipe and carrying a lamb."

"Like two pig's faces, one looking on the grass for a baby pig, and instead of finding a pig he found a pipe; and then father pig came home from work with a pipe in his hand."

"Like a lady pouring water over some boys, and the boys hit the lady instead of the lady pouring water over them; and as she was walking back she knocked the tree over and there was a policeman near by and she was taken away; and as she was taken away, she was knocked down."

"Like a mother pig looking for something and instead she found the baby pig she lost."

The peculiar circumstances to which these everyday actions such as going for walks, looking for and finding lost things etc., are applied, indicate no little power of constructive imagination in this subject: yet out of a total of 147 associations given by her, but 33 per cent. were constructive: and apart from the examples quoted her replies are no more striking than those of other subjects.

E. W. an exceedingly intelligent girl of 7 years found some difficulty in interpreting Blot 5: she was clearly imaging a feeling of some sort for which she was quite unable to find expression in mere words. Finally she produced this:

"Something between a very very rage—its face." Her facial expression and actions showed plainly that she felt in the shape of the blot, a suggestion of acute anger; it was not an angry person, nor an angry animal, but anger in the abstract which she was trying to express. This was the only case of its kind which occurred.

Several children are described in notes made at the time of the experiment, as being of a 'qualifying' type, *i.e.* they named an association and then proceeded to add to this. The following is a typical example of the tendency:

"This looks like a lady and she got a hat on—she looks to be running; one of her hands looks to be going down; she looks to be looking on the floor: she looks to have a little pussy in her hand. You can see one of her feet. The pussy looks to be looking at the sky: you can see one of his ears."

A remark made by one boy of $7\frac{1}{2}$ years in this connection is enlightening:

"Once you see it as something, you don't see it as anything else, whichever way you turn it (the blot)."

It suggests a possible classification of subjects as (*a*) those who worked synthetically, adding details to one or two associations, and (*b*) those who analysed the blot into several different associations, *e.g.*:

"Looks like a leaf. That part looks like a bear's head. Looks like a couple of roses put together sticking out. A silly old man with a lot of things hanging off him."

The comparatively small number of children taking part in this experiment, however, forbids any generalisation on these lines, as to possible differences in type of subject.

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SOME CONDITIONS AFFECTING THE GROWTH AND PERMANENCE OF DESIRES[1].

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I. Introduction.

Every reformer who has attempted to reduce his theory to practice knows that it is comparatively easy to convince another of the advantages involved in some project, but extraordinarily difficult to obtain the practical support of that person, if this should happen to necessitate action in opposition to some habit or prejudice of long standing. Whenever that is the case the other readily persuades himself that the reform in question, though highly desirable in itself, is quite impracticable under existing conditions and will therefore have to be left to the

[1] Thesis approved for the degree of Doctor of Science in the University of London.

efforts of a future generation. The only effective way to deal with such an opponent is to instil into him so strong a desire for the advantages to be derived from the reform that he becomes willing to risk all else for their sake. It is not always an easy matter to succeed in an attempt of this kind; occasionally the result produced is just the opposite of what was intended. More frequently we seem to be successful at the time, but discover before long that the impression we made was of too temporary a nature to be of any real value.

The more we know about the growth and functioning of desires the less likely are we to fail in attempts of this kind; yet there is at present but little experimental work which has direct bearing on this subject. The two standard investigations are those conducted by Squire and Ruediger respectively. I shall therefore begin by describing these[1].

The object of Squire's experiment was "to determine whether the habit of producing neat papers in Arithmetic, will function with reference to neat written work in other studies." This may appear to have no bearing on the genetic psychology of desire since its primary object was to study the effect of developing a habit. It is however impossible to succeed in this without rousing the corresponding desire in the children, and we may therefore look upon the experiment as an investigation into the effect of developing a desire to produce neat papers in arithmetic. Squire found that "the results were almost startling in their failure to show the slightest improvement in language and spelling papers although the improvement in arithmetic papers was noticeable from the first[2]." It follows that the inculcation of a limited desire of this kind produces improvement in the one direction in which training has been given, but in no other direction.

Ruediger's experiment forms the complement of this. He set out to discover whether "the ideal of neatness brought out in connection with and applied in one school subject functions in other school subjects[3]." What exactly this is intended to imply becomes clear by referring to the first three of the instructions Ruediger gave to the teachers who were responsible for the training.

These are as follows:

"(1) In the written work of one school subject pay all the attention you can both to the habit and to the ideal of neatness. Demand neat papers, having them rewritten when necessary.

[1] See note at end of this section.

[2] *Bagley Educative Process*, 1905, ch. 13, p. 208.

[3] Ruediger, "The Indirect Improvement of Mental Functions through Ideals," *Educational Review*, Nov. 1908.

(2) Talk frequently with the class (not to) [*sic*] on the importance of neatness in dress, business, the home, hospitals, etc. connecting it as far as you can with the subject under experiment.

(3) Do not bring up the subject of neatness in connection with the other studies of the school. If the pupils bring up these studies, quietly substitute something else...[1]."

Hence, expressed in terms of desire, the work the trainer undertook to do was to stimulate the children's desire to become neat with regard to a number of activities of which all but one lay outside the actual school work.

The result obtained in this way is given by the following extract from the same paper:

"Evidently neatness made conscious as an ideal or aim in connection with only one school subject does function in other school subjects. Directing our attention to groups I and III, the most marked improvement of the papers occurred respectively in geography and arithmetic, the subjects in which neatness was emphasised, but there was unquestionable improvement on the average also in other subjects[2]."

In other words the most marked improvement occurred in the activity in which most emphasis had been laid on the value of neatness, but definite improvement was also obtained in other closely allied activities in connection with which no such training had been given.

It will be clear from what has been said that the investigations of Squire and Ruediger give us conditions under which there tends to be transference in the case of desires which have been inculcated by means of direct instruction.

They are however often quoted to prove more than this. It is assumed that the training given by Squire resulted in the formation of a mechanical habit to produce neat papers in arithmetic, whereas that given by Ruediger resulted in the development of a true ideal of neatness. Hence the conclusion is drawn that the inculcation of ideals is necessary, if transference is to be obtained. If this is correct, then an ideal must be the same as a generalised desire; but this is not the sense in which the word is usually employed. An ideal may have any degree of generality; it differs from other desires solely in the nature of the motive force behind it. According to Baldwin's *Dictionary of Philosophy and Psychology* it is (1) "that which we suppose would satisfy our moral nature if we were able to attain it," or (2) "the conception by reference to which man's conduct should be regulated or to which his character

[1] *Op. cit.* p. 366.

[2] *Op. cit.* p. 369.

should be assimilated." The vital part of the phenomenon is therefore the motive to which the desire owes its existence. An individual can only be considered to have acted under the influence of an ideal, in so far as he was conscious that his sense of duty and his self-respect were involved, when he selected his line of action. But assuming that this *was* the case, then the motive remains an ideal, whether there happen to be few or many situations, which appear sufficiently similar to the individual in question to bring it before his consciousness. The improvement in neatness that was observed by Squire was probably due to the children's desire to please the teachers in charge of the training. It is, however, at least theoretically possible that some of the children may have conceived the idea that it is wrong to do untidy work in arithmetic but that there is no real need to be neat in other school-subjects. In so far as improvement was due to such an attitude, it was due to the activity of a true, though very limited ideal.

I shall return to this point at a later stage (§§ VIII and IX). For the present purpose it is sufficient to point out that we have no means of judging how far true ideals of neatness were formed in either case. The only thing we know is that there was more opportunity for it in Ruediger's experiment. The second of his instructions to the teachers shows that a definite attempt was made to inculcate a true ideal of neatness; but this does not in itself show that the improvement obtained was entirely or even mainly due to the development of this ideal. Many other forces may have been at work, such as the desire to win approval and the fear of punishment.

Hence it seems better not to use these two investigations in order to compare the effect of developing a habit with that of developing an ideal. What they do show is that transference is more likely to occur when a desire is stimulated with regard to a number of activities than when it is merely stimulated with regard to some one of the same.

This is, however, only part of the larger problem which will have to be solved if we are to have a genetic psychology of desire. In a complete investigation we should expect to find an account of the forces that make respectively for the birth, the growth and the decay of individual desires, and this both for those cases in which the subject is aware of the fact that the experimenter is trying to make him adopt a certain line of action and for those in which he is not conscious of any such guidance.

It is of course not the intention of the writer to cover anything like this ground. The investigation to be presented in this paper[1] is again limited to the effect of direct instruction on the development of one particular desire. It differs from those to which reference has just been made, in that it is mainly concerned with the conditions that make for growth and permanence of desires, although it is believed to have bearings on the problem of transference. Since both the previous experiments had been concerned with the effect of training in neatness, it seemed preferable to use some other desire for the main part of this investigation.

It was decided that the subjects should again be school children so as to reduce to a minimum the artificiality of the atmosphere during the period of training, which might otherwise have rendered the results valueless. It was therefore necessary to choose an ideal which should be valuable from an educational point of view and which could yet be presented to the class in a new light for the purpose of the experiment. Further it had also to be one in which the rate of improvement could be measured by means of written tests. It seemed to me that these conditions would all be satisfied by the desire to learn more about the common objects of every day life—the desire to become more observant as I shall call it for short. It is a subject in which the rate of improvement evidently lends itself to measurement by means of written tests. Moreover, though the value of good powers of observation is fully realised in most schools, yet training is usually only given with regard to the particular subject in hand and under normal conditions it only forms part of the many things that have to be taught. Thus the desire to become more observant—in the sense just defined—seemed to present just the kind of material that I was wanting. I singled it out for special attention during the greater part of a term, connected it more definitely than is usually done with the children's home-interests and tried to develop it as a conscious ideal in as generalised a form as possible.

The results to be discussed in the following pages are based on the work of 423 children. In all, three experiments were undertaken: in one the method just described was used by itself, in one it was supplemented by a course of special exercises in quick perception, in the third it was again used by itself but this time in order to stimulate a desire to become more neat instead of more observant. The object of this last experiment was to see how the development of an ideal of neatness would react on tests designed to measure the children's habit of observa-

[1] It was conducted under the direction of Professor Spearman, to whom the writer is greatly indebted for the advice and assistance so freely given.

tion. It will be necessary to describe each of these experiments in some detail.

Note. It may be advisable to mention at this point an article which appeared in the January number of this year's *Journal of Educational Psychology* ("An Experiment on the Influence of Training on Memory," by Edwina Abbott Cowan). It is an "account of an attempt to set up in a number of children a habit involving a stimulus which is common to many situations where memory functions, and to test its efficacy in such situations" (*op. cit.* p. 32). For a description of the method employed by the writer, the reader is referred to the original. Here it is sufficient to show what inference the numerical results are thought to justify. This will be most readily seen from the following quotation:

"The subjects were so few and the time available with them so comparatively short that I hesitate to draw conclusions on the basis of these results. But they seem to indicate that at least there still remains the possibility of forming by direct means a habit which may be genuinely general in its effect. To my own infinite astonishment I seemed to have given these children a 'habit of attention' which was useful to them in memorizing. At any rate from the evidence now at hand further experiments along this line need not necessarily be unprofitable" (*op. cit.* p. 38).

Apparently no attempt was made to develop in the children a desire to become more attentive. The conditions of the experiment therefore correspond with those of Squire's, yet the results suggest that transference may have occurred. It is therefore worth while to examine these results more closely.

Two tests were used to estimate the amount of transference:

(1) In one of these—referred to as the 'table test,' the transference is said to have occurred between the second and the third time of testing. Using the numerical results given by the writer (see Table II, *op. cit.* p. 37) we find that the nine trained subjects that were present both times improved 6·22 per cent. or just three times the standard error for gain II–III whereas the four non-trained subjects lost ·75 per cent. But the actual gains per cent. of the four non-trained subjects were − 13, + 10, − 28 and + 28 respectively. They therefore exhibit no general tendency either to gain or to lose marks in the last test and it is therefore impossible to judge what would have been the probable results if a larger number of subjects had been used. So far as the present data is concerned it is quite possible that the gain of 6·22 per

cent. may be merely a practice effect of the tests themselves for the excess of the trained over the non-trained is only 6·97 per cent. and the standard error of this difference is 10·9 (cf. § III, 3 below).

(2) The evidence given by the second test, referred to in the text as the 'prose test,' appears to me to be equally unconvincing. In this case the trained subjects gain ·67 per cent., whereas the non-trained subjects lose 6 per cent. The training is therefore considered to have prevented a deterioration in the quality of the work. But again an examination of the work of the four non-trained subjects shows that this is not a legitimate inference. The actual amounts gained were 0 per cent., + 1 per cent., + 2 per cent. and − 27 per cent. (see *op. cit.* p. 38, Table III). Hence the average loss of 6 per cent. is entirely due to the behaviour of one subject. As a matter of fact the standard error is 6·06, so that the loss is less than the standard error and is therefore probably due to fluctuations of sampling (cf. § III, 3 below). There appears therefore at present to be no reason to believe that a generalised 'habit of attention' can be formed by a course of special exercises which is not supplemented by the development of a generalised desire.

II. METHOD OF TRAINING BY MEANS OF IDEALS AND SPECIAL EXERCISES.

This experiment was carried out in the autumn of 1913, in four of the Willesden elementary schools. 160 children between the ages of 12 and 13 were used for this purpose. No special effort was made to limit the investigation to children of the same social status as it was considered more important to secure teachers, who had sufficient belief in the training, to be willing to try it.

1. *The special exercises.*

Material for the special exercises was ready at hand in a little book called *Methods of Mind Training* by Miss C. Aiken. In her introduction, Miss Aiken says: "It was with a view to arousing mental activities, keeping the mind on the alert and holding the attention steadily, that I formulated certain exercises which placed the mind in the same mental attitude for a short time each day. The result was that a habit of voluntary attention was formed and thus I had secured to a considerable degree the end I had so assiduously sought" (p. 24).

The exercises are then described in some detail. Columns of figures were written on the blackboard and the pupils were required to repeat these figures from memory after a single glance at them. Tables and history dates were treated in the same way. As the children's power improved, the lists were increased in length or complexity. In order to help the children to recall objects in their places, a blackboard was divided into twenty-five large squares, letters or numbers were put into some of them and the pupils were asked to reproduce this from memory after looking at it for a few seconds. Glance drawings and time sketches were used. Stories were read to the class with the request to reproduce sometimes the whole, sometimes only the essential parts of them, etc. Every effort was evidently made to give the children as much variety as possible. The time set apart for these exercises was twenty minutes a day and in Miss Aiken's hands, marvellous results were evidently obtained by these means. She says: "One of the most gratifying results is that it has aroused the dull, slow-moving minds to a degree of activity which has become a new and delightful experience to the possessors. Again the use of the mental training has been seen in the ability to recall with accuracy after many months and often years have passed that which the pupils have read or heard of poetry or prose, facts of history, literature, art, etc....In a considerable degree, the power has been acquired of shutting out from the mind extraneous and irrelevant subjects while pursuing their studies."

The book is very enthusiastic in tone and therefore all the more likely to convince others that the method is worth trying. Copies of this book were put into the hands of the four teachers who had undertaken to train the children. They were asked to keep entirely to the kind of material and the methods of presentation suggested in it, but to give the children as much variety as possible within these limits. Each class was given twelve weeks' special training. The actual time allowed for it was fifteen minutes a day on four days of every week.

2. *The use of ideals.*

In connection with the training, we tried to create in the children a real desire to know more about the things around them and impressed upon them that the special lessons were intended to help them in this. In my written directions to the teachers who kindly undertook this work, I suggested that the children should be allowed to discuss the matter from as many points of view as possible and that illustrations should be taken alike from home and from school. For the sake of uniformity, and for another reason which will appear later, the time for these occasional talks was always to be taken out of the training time.

In order to create as favourable an atmosphere as possible for the growth of an ideal, I pointed out that it was extremely important for the success of the experiment:

(1) that the children should enjoy the special training given;

(2) that they should understand its aim, *i.e.* to make them more observant in the sense defined (see p. 97);

(3) that they should really learn to appreciate the value of good powers of observation.

I have every reason to believe that the children enjoyed the work and understood its object.

3. *The arrangement of the children for the special lessons.*

In order to be able to distinguish the effects of the training from mere practice effects, due to the repetition of the same task, the children that were to be trained were selected in a particular way. Two classes were used in each school, say classes A and B. Both were tested before the training was begun. On the marks obtained in the various parts of this test, they were divided into four groups: A_1 and A_2, B_1 and B_2, such that for each part of the test, the average marks obtained by A_1 were equal to the average marks obtained by A_2 and similarly for B_1 and B_2. For the fifteen minutes training, A_1 and B_1 were put together into the class room of A; A_2 and B_2 into that of B. Beyond this the arrangements of the schools were not interfered with in any way. The teacher of A then gave A_1 and B_1 a course of lessons of the kind described above while the teacher of B gave A_2 and B_2 a lesson in writing or reading or any other subject which did not involve training in quick perception. In every case A was the older class and in one case A was also the less intelligent class.

4. *The method of testing the effect of the training.*

The children were tested four times in all:

(1) just before the beginning of the experiment, as I have already stated;

(2) after they had had six weeks' training;

(3) after they had had twelve weeks' training, *i.e.* at the end of the training course, and

(4) four months after the training had stopped.

The object of this last test was to discover, if possible, the after-effects of the training.

Four similar tests were constructed and it was arranged that each school should begin with a different test and should have the others in rotation, *i.e.* one school would have them in the order *abcd*, the next in the order *bcda*, etc. In this way accidental inequalities in the tests would not affect the average marks from the four schools.

I shall refer to the schools in which this part of the experiment was conducted as the 'demonstration schools.' This term seems better than 'trained schools' because only half of each class was trained directly, though, as will be shown presently, some of the children, who did not actually take part in the special lessons, were yet indirectly affected by the training. The term 'demonstration schools' suggests that these schools were used to demonstrate the effect of the training just described, and this exactly expresses their function in my experiment.

As it seemed possible from the results obtained that the improvement in A_2 and B_2 was not entirely due to practice given by the actual tests, the same series of tests was repeated at another set of four elementary schools in the Willesden area, where no special training was given. On this occasion an attempt was made to select schools of the same type as those used in the first series and to give the tests in the corresponding order, *e.g.*, if the tests had been given in the order *abcd* to children drawn from a very poor neighbourhood in the first series, they were given in the same order to the corresponding school in the second series. Any differences in the two series that are merely due to the difference in the type of school used, should therefore disappear in the average. I shall refer to the second series of schools as the non-trained schools.

III. Method of measuring the effect of the training.

1. *The test material.*

I shall next describe the actual tests used to discover the effect of the training given. Each test consisted of three parts. Of these two were intended to measure the direct effect of the special exercises in quick perception, the third that of stimulating in the children a desire to know more about the common objects of everyday life.

(*a*) The two tests that were intended to measure the direct effect of the special exercises given were taken straight out of Miss Aiken's book. The first consisted in the reproduction of ten numbers, the second in the reproduction of a story containing about sixty ideas. The instructions given in the first case were: "I shall show you a list of ten numbers for half a minute. Learn as much of them as you can in any way you like. As soon as I take away the list, write them out as fast as you can, leaving spaces for those which you have forgotten." The instructions given in the second case were: "I shall read a short story to you once. As soon as I stop reading, write it out as far as you can in the words of the story, but where you have forgotten the words, write out the story in your own words." The children were given fifteen minutes for this piece of reproduction. I shall refer to the former as 'numbers' to the latter as 'composition.'

(*b*) In order to obtain a measure for the extent to which the children had developed an effective desire to know more about every-day objects questions were set involving (*a*) objects familiar and interesting in themselves such as a *motor-bus* or a *piano*, and (*b*) objects which they must frequently have seen or used but to which they were less likely to have paid special attention such as a *hinge* or *straw*. To allow for differences in taste, three objects were given both in (*a*) and in (*b*) and the children were told to choose the two they knew most about. The instructions given were: "make a memory drawing of each of the four objects selected, say what each is used for and in the case of (*a*) how it is used, in the case of (*b*) how it is made." Half-an-hour was allowed for this work. I pointed out to the class that the object was not to make as artistic a drawing as possible, but only to show as many parts as possible and encouraged them to label anything which they thought I might fail to recognise. In marking these papers, marks were as far as possible only given to the generic—*e.g.* flowers painted on the front of the piano received no marks nor did the actual advertisements with which some of the children covered their omnibus, but marks were given if the position of these advertisements was shown correctly.

It may be thought that the element of memory which is undoubtedly involved in this test vitiates it as a measure of the amount the children have actually observed within any given period and this for two reasons: (1) because they will tend to remember most about those things which happen to have attracted their attention shortly before the test, and (2) because they will not be able to remember at the time of the test all they have actually noticed during the preceding weeks.

(1) Of these the first is the more serious; individual children are almost certain to have done exceptionally good work in one or other of the tests, because they happened to have examined the required objects shortly before the day of the test. But it is believed that this cannot have made any significant difference in the average rate of improvement;

seeing that the test material was selected from objects which the children would be likely to see almost every day of their lives and that each average represents the work of at least forty children. (In the experiment under discussion there were 80 in each section of the demonstration schools, 112 in the non-trained schools.) At any rate such individual differences will not affect the value of the result when expressed in terms of the standard error, since the latter is itself a function of these differences (cf. below, § III, 2).

(2) The effect of obliviscence remains to be considered. It is obvious that the children cannot possibly have remembered at the time of the test all they had observed within a period of at least six weeks; but it can be shown that this in no way invalidates the test as a means of estimating the effect of the training under discussion.

The extent to which any given precept is remembered depends mainly on such factors as the kind of associations made with it, the amount of attention paid to it at the time, the tendency to recur to it at intervals, etc. Some of these could no doubt be improved by definite instruction; but since no such instruction was given, any improvement that did occur must have been due to the children's own efforts and was therefore itself one of the results obtained from the training.

The other factor that influences the extent to which a percept is remembered is what is known as 'native retentiveness.' It differs from those just discussed in that it is not educable and can therefore not be affected by any desire to remember more of what has been perceived. Individual differences in native retentiveness are known to be great. They will undoubtedly increase the standard deviation and thus the standard error of the rate of improvement, but it can be shown that they will not affect the numerical results obtained. For suppose that the average amount of observation and thought relevant to questions set in test I was a for a certain set of children and that owing to faulty retentiveness only ax of this could be recalled at the time of the test. Then if the amount of relevant observation and thought in connection with test II was b for the same set of children, the amount remembered at the time of the test was bx; for there is no reason to suppose that the average retentiveness of 40 to 100 children would vary appreciably within the period of the experiment. It follows from this that the difference between the amount remembered in test II and that remembered in test I was $(b - a)\,x$ or $100\,(b - a)\,x/ax$ per cent. of the amount remembered in test I. This is evidently equivalent to $100\,(b - a)/a$ per cent.; *i.e.* to the increase per cent. in the amount of relevant observation and thought in the interval between test I and test II. Hence improvement per cent. is independent of differences in native retentiveness.

In general we may therefore assume that the Observation Test provides a satisfactory method of judging the effective strength of the desire to take an intelligent interest in the immediate environment; in spite of the fact that it is concerned not with actual percepts, but only with what can be recalled of these at the time of the test. As has just been shown any improvement in the amount remembered must under the conditions of the experiment be due to an increase in the desire to become more observant. Individual differences in native retentiveness on the other hand will not affect the numerical value of the rate of improvement. They will of course tend to increase the standard error of the rate of improvement; but this is the only reason for which it would have been desirable to eliminate them, if it had been possible to do so.

2. *Individual differences.*

The way in which individuals respond to training of the kind described in § II and the quality of their work in the actual tests must depend in part on their character and on their general ability. It would therefore have been desirable to have attempted to obtain an analysis of the mental characteristics of each child. Since this was unfortunately impossible owing to the large number of children involved; it seemed worth while to try what results could be obtained by the use of estimates only. Each school was therefore asked to give an estimate of the intelligence, diligence and suggestibility of each of the children examined. Diligence was included in order to prevent any confusion between intelligence and interest in school work. The number of cases in which a high estimate of intelligence accompanied a low estimate of diligence or vice versa show that the teachers were keenly alive to this distinction.

The scale of marking adopted was 5 = much above the average, 4 = above the average, 3 = average, 2 = below the average, 1 = much below the average. For the purpose of the estimates 'average' was defined as 'the average of children at this stage, not necessarily the average of these particular children'; 'suggestibility' as 'readiness to be influenced by members of the staff.' In the elementary schools the estimates were made by the master or mistress in charge of the class; in the high schools they were sometimes made at staff meetings, sometimes by two or three mistresses in consultation. As difficulties were raised in some of the schools about obtaining a second independent estimate, I decided that it would be wiser to do without it in all the schools.

3. *Reliability.*

As stated above the tests in observation etc. were given to three different groups of children: (1) the trained section of the demonstration schools, (2) the non-trained section of these schools, and (3) the non-trained schools. It is evident that any improvement observed in groups (1) and (2) must be due partly to the course of training to which these children were subjected and partly to the practice given by the tests themselves. In order to estimate the effect of the training, it will therefore be necessary to compare the improvement of these groups with that of group (3), to which no training was given. Similarly the difference between the direct and the indirect effect of the training can be obtained by comparing the rate of improvement of group (1) with that of group (2). It is thus a simple matter to determine a series of numerical values which represent the effect of the training under various conditions. It is however not so easy to interpret the results obtained in this way. When we attempt to do this, we find ourselves faced with two difficulties, each of which will have to be discussed in some detail.

(*a*) *General conditions for determining the significance of a numerical result.*

It follows from what has just been said that the effect of the training will have to be deduced from the differences in the rates of improvement of two sets of children. We must therefore make allowance for the fact that these are only samples drawn from all the children that might have been used, and that they would probably not improve at exactly the same rate, even though conditions were made as nearly alike as possible; for the quality of their work is liable to 'fluctuations of sampling,' *i.e.* to the influence of "an extremely complex system of causes of the general nature of which we are aware, but of the detailed operation of which we are ignorant[1]." It is therefore impossible to draw any conclusion from the fact that one group of children has improved more rapidly than another, unless we have means of ascertaining whether the observed difference is or is not due to mere fluctuations of sampling.

Now it can be shown that the great bulk of these fluctuations fall between ± three times the standard error (*i.e.* the standard deviation

[1] Yule, *Introduction to the Theory of Statistics*, 1912, p. 30.

of the mean of the sample from the mean of the universe of which it forms a part) and a large proportion of them between $\pm$ twice the standard error[1]. Hence it may be assumed that a difference of more than three times the standard error is not likely to be due to fluctuations of sampling, and that a difference which is greater than twice, but less than three times the standard error, suggests that there may have been some special cause at work, but is not large enough to prove this conclusively[2]. Since then the significance of the difference between any two rates of improvement depends entirely on the magnitude of the corresponding standard error, I have in most cases discussed it only in relation to the same; but in the tables the percentage rate of improvement has also been given.

In passing it will be convenient to note that the standard error of the mean varies inversely as the number of subjects (standard error = standard deviation/$\sqrt{n}$), and that it is therefore desirable to pool results obtained from different sources, whenever such a course of action is legitimate.

(*b*) *The effect of drawing samples from four independent records.*

In the present instance the interpretation of the results obtained from the experiment is however further complicated by the fact that each difference is itself the average of four differences obtained from four pairs of corresponding schools or sections. In order to enable us to appreciate the effect this may have on the significance of the numerical results, it will be convenient to look upon each section of any particular school (or, in the case of two schools, each of the schools) as a sample drawn from a hypothetical school like the actual school in all essentials, except that it contains an indefinitely large number of children. Such a hypothetical unlimited source for material is termed a 'record.' Hence the difference between the trained and the non-trained section of any school may be considered as a sample obtained from such a hypothetical 'record'; and since four schools were used, we must assume four such records—one for each school. We have however no reason for assuming that these four records are not perfectly independent of each other. Further the average difference in rate of improvement is the average of the differences observed in the four schools; it can therefore be looked upon as the average of four samples taken each

[1] Yule, *op. cit.* p. 311.

[2] For an approximately normal distribution the odds are about 370 to 1 against a given difference being due to mere fluctuations of sampling if it is just 3σ (σ = standard error), and about 22 : 1 if it is just 2σ (Yule, *op. cit.* p. 311).

from an indefinitely large record. This raises the question under what circumstances it is legitimate to pool these four records.

Now it can be shown that when samples are drawn from four different records the standard error of the mean is in general greater than it would have been if they had all been drawn from one and the same record, and this obviously affects the interpretation of any difference in rate of improvement that may be observed. For a full discussion of the problem the reader is referred to Yule[1]. It is there shown that when samples are drawn from different records the relation of the standard error of the means for these records (σ_m) to that of all the individuals composing the records ($\sigma_0/\sqrt{n}$) is given by the formula

$$\sigma_m{}^2 = \frac{\sigma_0{}^2}{n} + \frac{n-1}{n} s_m{}^2,^{2}$$

where s_m is a quantity, the magnitude of which depends on the deviation of the mean of each of the records from the mean of all the records and which becomes zero only when each of these deviations is zero.

It is evident from this that σ_m is in general greater than $\sigma_0/\sqrt{n}$ and that the two standard errors will be equal only if s_m is zero, that is, if the mean of each of the n hypothetical records coincides with the mean of all the records, or in other words, if the samples have been drawn from essentially homogeneous records. Since the four schools under discussion must be considered to provide samples from four independent records, it follows from this, that the average difference in rate of improvement obtained by pooling the schools can *not* be expressed in terms of the standard error as calculated from the individuals composing the schools, unless the four records happen to be homogeneous within the limits of fluctuations of sampling. It will therefore be necessary to examine the schools from this point of view, before attempting to discuss the significance of the results obtained from them.

The method I adopted for this purpose will be most readily explained by considering a particular case. Suppose therefore that it is required to find whether the four differences in rate of improvement in observation, as given by the trained and non-trained sections of the four demonstration schools, may be taken to have been obtained from records homogeneous within the limits of fluctuations of sampling.

Let the four schools be P, Q, R, and S; the trained sections P_t, Q_t, etc.; and the non-trained sections P_{nt}, Q_{nt}, etc. Then the difference

[1] Yule, *Introduction to the Theory of Statistics* (1912, pp. 347 and 348).

[2] *Op. cit.* p. 348 (9).

in the rate of improvement of the four schools is the average of the differences observed in $P_t \sim P_{nt}$, $Q_t \sim Q_{nt}$, etc. It is therefore necessary to show that these four differences were taken from records homogeneous within the limits of errors of sampling. The most satisfactory way of doing this would be to compare the work of the sections under identical conditions as regards test material and position of test in series. This it is impossible to do, since the test material was purposely given in cyclic order, so as to make it possible to compare the rate of improvement in different periods of the experiment for each group separately.

Yet it seems undesirable to ignore differences in test material entirely, for it is at least possible that the differences in rates of improvement in corresponding sections (*e.g.* $P_t \sim P_{nt}$) are in part due to the nature of the test material. Owing to local conditions, test II may, for instance, have appealed more to P_t than to P_{nt} and the difference between P_t and P_{nt} may consequently be large for gain I–II as compared with gain I–III, etc. The effect of such a chance difference is obviously much reduced in the average. It will therefore be more satisfactory to select as a measure of differences in the rate of improvement, one which will reduce the effect of differences in test material to a minimum.

Such a measure is given by the average deviation of the three rates of improvement gain I–II, gain II–III, and gain III–IV, for this reduces the effect of one test in one section to one-third of its total value. In order to show that the four differences $P_t \sim P_{nt}$, etc. have been taken from records which are homogeneous within the limits of fluctuations of sampling, it will be necessary to show that every two of them may be taken to consist of samples from such records. There are evidently six ways of pairing the four differences, and each of these will have to be considered separately. I proceed to give the working in full for one such pair, *i.e.* $P_t \sim P_{nt}$ and $Q_t \sim Q_{nt}$. The numerical differences obtained from these are as follows:

Excess of	Gain I–II	Gain II–III	Gain III–IV
P_t over P_{nt}	$+6{\cdot}4$	$-2{\cdot}0$	$-1{\cdot}3$
Q_t over Q_{nt}	$+0{\cdot}6$	$+1{\cdot}9$	$-0{\cdot}9$
Difference	$+5{\cdot}8$	$-3{\cdot}9$	$-0{\cdot}4$

Therefore average deviation $= \frac{1}{3}(5{\cdot}8 + 3{\cdot}9 + 0{\cdot}4) = 3{\cdot}4$ ......(1).

For P_t the standard error of the average deviation (*i.e.* av. devn./$\sqrt{n}$, where n = number of children) is $1{\cdot}32$.

For P_{nt} the corresponding standard error is $1{\cdot}36$.

Therefore the standard error of the difference

$$P_t \sim P_{nt} = \sqrt{(1{\cdot}32)^2 + (1{\cdot}36)^2} = 1{\cdot}89.$$

Similarly the standard error of the difference $Q_t \sim Q_{nt} = 1{\cdot}60$, and hence the standard error of the difference $P \sim Q$

$$= \sqrt{(1{\cdot}89)^2 + (1{\cdot}60)^2} = 2{\cdot}48 \qquad \ldots\ldots\ldots(2).$$

But from (1), the difference between the average deviations in schools P and Q is 3·4; from (2) the standard error of this difference is 2·48;

therefore the difference between the average deviations in schools P and Q is 1·37 of the standard error;

therefore the two schools may be considered to have been taken from records homogeneous within the limits of fluctuations of sampling.

Similar calculations were made for each of the other pairs (*e.g.* $P \sim R$) and it was found that the differences all lay between $1{\cdot}2\sigma$ and $1{\cdot}9\sigma$, where σ is the standard error of the difference for the schools under consideration. In comparing the average rate at which the trained and non-trained sections improved in observation, the differences obtained in the four schools may therefore be considered to have been taken from essentially homogeneous records. Hence the standard error of the mean (σ_m) does not differ significantly from the standard error for the individuals $\sigma_0/\sqrt{n}$ (cf. Yule's formula quoted above), and it is therefore permissible to use this latter standard error as the unit in determining the significance of any mean difference in rate of improvement (cf. (*a*)).

The same method can obviously be applied in the case of any two of the three groups—the trained section, the non-trained section, and the non-trained schools, and for each of the three tests—observation, composition, and numbers. When this was done, it was found that in *observation* the differences in the average rate of improvement of any two pairs of schools or sections (*e.g.* $\overline{P_t - P_{nt}} \sim \overline{Q_t - Q_{nt}}$) all lie between $0{\cdot}8\sigma$ and $2{\cdot}1\sigma$; the results obtained from them may therefore all be expressed in terms of the standard error calculated from the individuals. The same holds for the *numbers* test, the maximum difference being only $1{\cdot}8\sigma$ in this case.

In *composition* the differences between the trained and the non-trained sections are homogeneous within the limits of the fluctuations of sampling, the maximum difference being $2{\cdot}0\sigma$. But, owing to the behaviour of the non-trained school, S, only three of the four differences can be considered to have been taken from homogeneous records for any comparison between demonstration schools and non-trained schools. For schools P, Q, and R, differences between pairs

$$(e.g.\ \overline{P_{\text{trained section}} - P_{\text{non-trained school}}} \sim \overline{Q_{\text{trained section}} - Q_{\text{non-trained school}}})$$

all lie between $1{\cdot}4\sigma$ and $1{\cdot}9\sigma$; for differences involving schools S the corresponding range is $3{\cdot}2\sigma$ to $5{\cdot}5\sigma$. In measuring the total effect of the training on the work in composition it will therefore be necessary to exclude schools S. Since each pair of corresponding schools (*e.g.* demonstration school P and non-trained school P) had the tests in the same order, this will in no way invalidate the results obtained, so long as we do not attempt to compare the rates of improvement of one group of schools or sections in different periods of the experiment, *e.g.* gain I–II with gain I–III for non-trained schools P, Q, and R.

IV. THE VALUE OF THE SPECIAL EXERCISES IN QUICK PERCEPTION.

I shall begin by considering the direct effect of the special exercises in quick observation. As stated above these covered a fairly wide range and any particular exercise was only given once a week but if they had really increased the children's power of 'quick perception' then an improvement should at least have been observable in tests based directly on them, such as the Numbers Test and the Composition Test.

TABLE I. *Number Test.* (Schools P, Q, R and S.)

	Average gain per cent.			Average gain in terms of standard error		
Excess of	I–II	I–III	I–IV	I–II	I–III	I–IV
Trained section over non-trained schools	$-9{\cdot}8\pm6{\cdot}6$	$-8{\cdot}0\pm7{\cdot}0$	$+8{\cdot}4\pm6{\cdot}6$	$-1{\cdot}5\sigma$	$-1{\cdot}1\sigma$	$+1{\cdot}3\sigma$
Non-trained section over non-trained schools	$-2{\cdot}8\pm5{\cdot}9$	$-6{\cdot}2\pm6{\cdot}8$	$-3{\cdot}1\pm6{\cdot}6$	$-0{\cdot}5\sigma$	$-0{\cdot}9\sigma$	$-0{\cdot}5\sigma$
Trained section over non-trained section	$-7{\cdot}0\pm6{\cdot}8$	$-1{\cdot}8\pm8{\cdot}1$	$+11{\cdot}5\pm6{\cdot}9$	$-1{\cdot}0\sigma$	$-0{\cdot}2\sigma$	$+1{\cdot}7\sigma$

(*a*) The results obtained in the Numbers Test are given in Table I. It is evident from this that the difference between corresponding sections and schools is in every case less than twice the standard error. The special exercises have therefore not increased the children's power to take in lists of numbers at a glance.

This agrees with the results obtained by Whipple in two experiments "conducted to determine the effect of practice upon quick visual perception with special reference to the usefulness of specific exercises in 'mind-training' such as have been recommended by Miss Aiken[1]." He found that the effect of practice was very slight and that it was "entirely explicable in terms of habituation to the experimental con-

[1] See "The Effect of Practice upon the Range of Visual Attention and of Visua Apprehension," *Journal of Educational Psychology*, I.

ditions and of the development of grasping schemes or other devices of observation[1]." A similar result was obtained in an experiment for which Professor Foxley and four post-graduate students of the Education Department of University College, Cardiff, kindly acted as subjects. In this, the task was to memorise lists of numbers and geometrical figures; the length of each learning period was thirty seconds and the method of learning was left entirely to the choice of the subject. Both the numbers and the drawings were practised eight times, within a fortnight, *i.e.* twice on each of four occasions. Under these circumstances it was found that every marked gain or loss was accompanied by a change in the method of learning employed by the individual and that the average rate of improvement was negligible.

TABLE II. *Composition Test.* (Schools P, Q and R.)

	Average gain per cent.			Average gain in terms of standard error		
Excess of	I–II	II–III	I–IV	I–II	II–III	I–IV
Trained section over non-trained schools	$+8{\cdot}4 \pm 3{\cdot}7$	$-13{\cdot}0 \pm 3{\cdot}5$	$+1{\cdot}4 \pm 3{\cdot}6$	$+2{\cdot}3\sigma$	$-3{\cdot}7\sigma$	$+0{\cdot}4\sigma$
Non-trained section over non-trained schools	$+7{\cdot}0 \pm 3{\cdot}3$	$-17{\cdot}2 \pm 3{\cdot}8$	$-2{\cdot}2 \pm 3{\cdot}1$	$+2{\cdot}1\sigma$	$-4{\cdot}5\sigma$	$-0{\cdot}7\sigma$
Trained section over non-trained section	$+1{\cdot}3 \pm 4{\cdot}1$	$-4{\cdot}4 \pm 3{\cdot}9$	$+3{\cdot}5 \pm 3{\cdot}8$	$+0{\cdot}3\sigma$	$-1{\cdot}1\sigma$	$+0{\cdot}9\sigma$

(*b*) The effect of the training on the Composition Tests is shown in Table II. This is not so obvious and will have to be considered in more detail. As was shown in § III (3) the difference in rate of improvement of schools S was found not to lie in the same universe as the differences observed in the other pairs of schools. The marks given in Table II were therefore calculated from schools P, Q and R only.

The following results can be deduced from these marks:

(1) The difference between the two sections of the demonstration schools is in every case negligible. Its largest value is $1{\cdot}1\,\sigma$.

(2) For gain I–II the excess of the trained section over the non-trained schools is $+\,2{\cdot}3\sigma_{12}$: for the non-trained section the corresponding difference is $+2{\cdot}1\,\sigma_{12}$. This shows that there is probably a slight tendency for the demonstration schools to improve more rapidly than the non-trained schools during the first six weeks of the period of training.

(3) For gain II–III the excess of the trained section over the non-trained schools is $-\,3{\cdot}7\,\sigma_{23}$ and the corresponding value for the non-trained section is $-\,4{\cdot}5\sigma_{23}$. There is therefore a definite check in the

[1] *Op. cit.* p. 260.

rate of improvement of the demonstration schools during the second half of the period of training.

(4) For gain I–IV the difference between section and non-trained schools is in both cases negligible. Hence the impression made by the training was not strong enough to show its effect four months after the end of the period of training.

One point stands out clearly from this analysis, namely that the exercises themselves were not very effective. If they had really helped the children to develop a "habit of quick perception[1]" then this should certainly have shown itself in tests based on their daily practice. But this is not the case; both in the Numbers and in the Composition Tests the difference between the rates of improvement of the trained and of the non-trained section is in every case less than twice the standard error. The slight advantage the demonstration schools had over the non-trained schools is the same for both sections of the former and suggests that, if due to more than chance, it was probably due to increase in interest in the work or in the amount of effort made at the time of the test. I shall have occasion to show presently with regard to another set of schools (see neatness, § VII, 3 *d*) that the children in the non-trained schools probably experienced a definite loss of interest after the first time and that this did not occur in the schools which were receiving the special course of training. Such a loss of interest would be likely to occur in a test which differed so little from the ordinary school work as the Composition Tests and it is therefore probably quite sufficient to account for the fact that the demonstration schools improved somewhat more quickly than the non-trained schools in Composition gain I–II.

It is much more difficult to account for the check in this improvement observed in the work of the demonstration schools in Composition gain II–III. Dr Sleight obtained similar results from a group of children who were trained in reproducing from memory the substance of passages of prose, that were read to them[2]. In this case the total training period was six weeks and the time allotted to the practice was thirty minutes a day on four days of every week. After three weeks of practice the improvement in the work of this group was found to exceed that of the corresponding non-trained group by over five times the probable error, after six weeks by not quite three times the probable error[3]. Dr Sleight shows that the decrease during the latter half of the period of training is due to the fact that the non-trained group

[1] Aiken, *op. cit.* p. 26. [2] "Memory and Formal Training," This *Journal*, IV.

[3] *Op. cit.* Table IV, p. 417.

improved steadily throughout the period of training whereas the trained group reached its maximum at the end of the first three weeks of the same[1], and he concludes that "the question seems to find its only true answer in the fact that the tests themselves formed for the other groups the training necessary to bring them to their maximum power; that 'saturation' point was sufficiently near for them to need but very little exercise to reach it[2]."

In my experiment the conditions were somewhat different, for the task of the trainer was to vary the exercises as much as possible and the records kept by the teachers show that 'composition' was practised once or at the most twice a week. Yet the result was similar to that obtained by Dr Sleight. The percentage of marks gained by the three groups of children was as follows:

	Gain I–II	Gain I–III
Trained section of demonstration schools	+11	+ 8
Non-trained section ,, ,,	+ 9·6	+ 5
Non-trained schools	+ 2·6	+13

It is evident from this that the demonstration schools reached their maximum by the end of the first six weeks of training, whereas the non-trained schools improved steadily from test to test. Thus the effect of the training is similar to that observed by Dr Sleight; but the explanation he suggests is not sufficient in this case. Table II shows that the check in the rate of improvement is $4{\cdot}5\sigma$ for the non-trained section, $3{\cdot}7\sigma$ for the trained section of the demonstration schools. It is thus more marked in the non-trained section than in the trained section and can therefore not be due to the practice given in the special lessons. Neither is it due to abnormal behaviour on the part of one or other of the schools, for it occurs equally in each of the three pairs of schools under consideration. What actually is the cause at work, it would however appear to be impossible to say from the material at our disposal.

V. The effect of the training in the demonstration schools on the work in the observation tests.

We can now turn to the effect of the training on the rate of improvement in the Observation Tests. As stated above these tests were designed to measure the extent to which the training given made the children take a more intelligent interest in the common objects of their everyday

[1] *Op. cit.* Table III, p. 415.

[2] *Op. cit.* p. 422

life. Comparison of the work of the trained with that of the non-trained groups will therefore show how far the training given was successful in developing an effective desire to become more observant.

It is evident that the direct and indirect effect of the training can be estimated by comparing the rate of improvement of the trained and of the non-trained section respectively with that of the non-trained schools, and that the excess of the direct over the indirect effect of the training can be deduced from differences in the rates of improvement of these two sections.

TABLE III A. *Observation Test.* (Schools *P*, *Q*, *R* and *S*.)

	Average gain per cent.			Average gain in terms of standard error		
Excess of	I–II	I–III	I–IV	I–II	I–III	I–IV
Trained section over non-trained schools	$+13{\cdot}8 \pm 3{\cdot}7$	$+17{\cdot}6 \pm 3{\cdot}8$	$+6{\cdot}2 \pm 3{\cdot}3$	$\mathbf{+3{\cdot}7}\sigma$	$\mathbf{+4{\cdot}6}\sigma$	$+1{\cdot}9\sigma$
Non-trained section over non-trained schools	$+5{\cdot}0 \pm 3{\cdot}6$	$+8{\cdot}0 \pm 3{\cdot}5$	$+0{\cdot}4 \pm 4{\cdot}0$	$+1{\cdot}4\sigma$	$+2{\cdot}3\sigma$	$+0{\cdot}1\sigma$
Trained section over non-trained section	$+8{\cdot}8 \pm 3{\cdot}8$	$+9{\cdot}5 \pm 4{\cdot}3$	$+5{\cdot}9 \pm 3{\cdot}7$	$+2{\cdot}3\sigma$	$+2{\cdot}2\sigma$	$+1{\cdot}6\sigma$

TABLE III B. *Observation Test.* (Schools *P*, *Q* and *R*.)

	Average gain per cent.			Average gain in terms of standard error		
Excess of	I–II	I–III	I–IV	I–II	I–III	I–IV
Trained section over non-trained schools	$+14{\cdot}0 \pm 4{\cdot}2$	$+17{\cdot}9 \pm 3{\cdot}9$	$+15{\cdot}2 \pm 4{\cdot}2$	$\mathbf{+3{\cdot}3}\sigma$	$\mathbf{+4{\cdot}6}\sigma$	$\mathbf{+3{\cdot}7}\sigma$
Non-trained section over non-trained schools	$+6{\cdot}9 \pm 4{\cdot}3$	$+9{\cdot}5 \pm 4{\cdot}0$	$+7{\cdot}2 \pm 3{\cdot}6$	$+1{\cdot}6\sigma$	$+2{\cdot}4\sigma$	$+2{\cdot}0\sigma$
Trained section over non-trained section	$+7{\cdot}2 \pm 4{\cdot}1$	$+8{\cdot}4 \pm 4{\cdot}3$	$+8{\cdot}1 \pm 3{\cdot}9$	$+1{\cdot}8\sigma$	$+2{\cdot}0\sigma$	$+2{\cdot}1\sigma$

(*a*) *The direct effect of the training.*

The direct effect of the training is given by the excess of the rate of improvement of the trained section over that of the non-trained schools. Table III A shows that this is $3{\cdot}7\,\sigma_{12}$ for gain I–II, $4{\cdot}6\,\sigma_{13}$ for gain I–III and $1{\cdot}9\sigma_{14}$ for gain I–IV.

Hence there was a significant improvement in the work at the end of the first six weeks and this improvement was more than maintained during the next six weeks. It is therefore surprising to find that there is no trace of it left four months after the end of the period of training (*i.e.* for gain I–IV). As a matter of fact this result is due to the behaviour of one of the four demonstration schools. The percentages of marks gained by the trained sections of the four schools in gain I–IV are as follows:

School	P	+ 49·0,
,,	Q	+ 18·5,
,,	R	+ 11·2,
,,	S	− 12·0.

This shows that school S, alone did not improve in test IV. The values for the corresponding non-trained schools are + 4·7, +10·8, +10·6 and + 7·9 so that the behaviour of school S is not likely to be due to differences in the difficulty of the test-material. It was therefore worth while to see what is the average excess of the trained sections P, Q, R over their corresponding non-trained schools for gain I–IV. This was found to be +15·2. The standard error of the difference is 4·2 for these three pairs of schools. The excess of the trained over the non-trained is therefore $3{\cdot}7\sigma$. This shows that the training did make a permanent impression in three out of the four schools and suggests that there must be special reasons why only a temporary improvement was produced in school S (see Table III B).

(*b*) *The indirect effect of the training.*

If we compare the four non-trained sections with the four non-trained schools (see Table III A) we find that the excess of the former over the latter is + $1{\cdot}4\sigma$ for gain I–II, + $2{\cdot}3\sigma$ for gain I–III and $0{\cdot}1\sigma$ for gain I–IV. These are all less than three times the standard error of the difference. Hence the indirect effect of the training is almost negligible; but that the non-trained section may have derived some benefit from it during the latter half of the period of training. It will however be shown presently that this does not really represent the true state of affairs (see § VI).

If we examine the work of the separate schools for gain I–IV we find that the amount gained by the non-trained sections are as follows:

School	P	+ 33·5	per cent.
,,	Q	+ 13·1	,,
,,	R	+ 5·5	,,
,,	S	− 12·8	,,

As before school S is the only one which loses marks in test IV.

Omitting this school, the average excess of the non-trained sections P, Q, R, over the non-trained schools P, Q, R, is found to be + $2{\cdot}0\sigma$ (see Table III B). It follows that such impression as was made by the training given was probably fairly permanent in three schools, P, Q and R, but was rapidly lost in school S.

It is difficult to say why demonstration school S behaved differently from the other schools in the after period. For direct training effect (as measured by the excess of the trained section over the corresponding non-trained school) school S is third of the four schools for gain I–II, second for gain I–III. The difference is therefore not likely to be due to the quality of the training given.

It may be that this difference was due to a cause which lay quite outside the intended scope of the experiment. It so happens that the children in schools P, Q and R remained in the same class throughout the seven months over which the tests were distributed, whereas the children in school S were moved into different classes within a week after they had done test III. What is more, the members of each class were not moved all into the same class but were distributed among a number of different classes. It is easy to see that such a general upheaval, causing the destruction of old associations and the formation of new ones, might well check the development of any newly formed ideals. If it could be shown that this is really the case, it would form another strong argument against the system of two or even three removals a year, that is so frequent at present.

(c) *The difference between the direct and the indirect effect of the training.*

The difference between the direct and the indirect effect of the training can be obtained by comparing the work of the trained section of the demonstration schools with that of the non-trained section. Table III A shows that the excess of the former over the latter is $+ 2{\cdot}3\sigma_{12}$ for gain I–II, $+ 2{\cdot}2\sigma_{13}$ for gain I–III, and $+ 1{\cdot}6\sigma$ for gain I–IV. This suggests that the direct effect of the training may have been somewhat greater than its indirect effect, but the difference is evidently very slight in every case.

The fact that the training made any impression whatsoever on the non-trained section is worth a moment's consideration. This improvement cannot have been due to the special exercises, because the children did not take part in them; neither can it have been due to the influence of the teachers, for they were specially asked to limit their discussion of the training and its object entirely to the special lesson time. It must therefore have been due to the children themselves. I have reason to believe that the lessons in quick perception were very popular.

They were probably described at length in the playground. Pride at having been selected and disgust at having been left out would be equally successful in stimulating the children to do their best. It seems to be an example of the diffusion of what Fouillée would call an idée-force and shows that such an impulse may have a measurable effect on those who only receive it at second-hand.

(*d*) *Individual differences.*

The effect of individual differences in intelligence, diligence and suggestibility remains to be considered. As stated in § III (2) each school was asked to divide the children into five classes under each of these headings. These estimates made it possible to select the children who were in each case (*a*) above, and (*b*) below the average. Thus six groups were obtained, two for intelligence, two for diligence and two for suggestibility. The average rate of improvement of each of these groups was then calculated both for the trained section of the demonstration schools and for the non-trained schools. From these the effect of the training on *e.g.* those above the average in diligence is readily obtained by subtracting the improvement of the non-trained from that of the trained members of this group. Finally, any difference in the way in which the training effected (*a*) those above, and (*b*) those below the average, in *e.g.* diligence, will be indicated by the difference between the excess of the trained over the non-trained in the former and the corresponding excess in the latter group.

Table IV gives (*a*) the excess of each group of the trained section over the corresponding group of the non-trained section for each of the rates of improvement under discussion, and (*b*) the excess of each group consisting of children above the average in diligence, intelligence or suggestibility over the corresponding group of children below the average as calculated from the facts given in (*a*).

Suppose then that it is required to find whether the effectiveness of the training during the first six weeks of the course depends on differences in *diligence*. The procedure would be as follows:

From Table IV the excess of the trained over the non-trained is given by gain I–II = – 2·6 for those *above* the average in diligence and by gain I–II = + 13·2 for those *below* the average, giving a difference of + 15·8 or + $2{\cdot}6\sigma_{12}$ in favour of those *below* the average in diligence (where σ_{12} is the standard error of the difference between those above and those below the average for gain I–II). Hence it would appear that those *below* the average probably derived more benefit from the first six weeks of the training than those *above* the average.

TABLE IV. *Excess of improvement of trained section over that of non-trained schools in Observation for groups of children differing in diligence, intelligence or suggestibility.*

A. *Effect of differences in diligence.*

	Average gain per cent.			Average gain in terms of standard error		
	I–II	I–III	I–IV	I–II	I–III	I–IV
Children above average	− 2·6	+ 12·2	− 1·5	—	—	—
,, below ,,	+ 13·2	+ 10·9	+ 9·1	—	—	—
Difference	− 15·8 ± 6·1	+ 1·3 ± 6·3	− 10·6 ± 5·8	$-2\cdot6\sigma$	$+0\cdot2\sigma$	$-1\cdot8\sigma$

B. *Effect of differences in intelligence.*

	Average gain per cent.			Average gain in terms of standard error		
	I–II	I–III	I–IV	I–II	I–III	I–IV
Children above average	+ 13·1	+ 15·8	+ 3·1	—	—	—
,, below ,,	+ 14·4	+ 12·8	+ 7·0	—	—	—
Difference	− 1·3 ± 6·5	+ 3·0 ± 6·2	− 3·9 ± 5·6	$-0\cdot2\sigma$	$+0\cdot5\sigma$	$-0\cdot7\sigma$

C. *Effect of differences in suggestibility.*

	Average gain per cent.			Average gain in terms of standard error		
	I–II	I–III	I–IV	I–II	I–III	I–IV
Children above average	− 13·2	− 2·1	− 5·0	—	—	—
,, below ,,	+ 5·5	+ 1·1	+ 1·9	—	—	—
Difference	− 18·7 ± 7·5	− 3·2 ± 6·9	− 6·9 ± 7·1	$-2\cdot5\sigma$	$-0\cdot5\sigma$	$-1\cdot0\sigma$

Referring again to Table IV it will be seen that most of the differences between those above and those below the average are less than twice the standard error and that none of them are as much as three times the standard error. The two that exceed twice the standard error both occur in gain I–II: (1) those *below* the average in diligence exceed those *above* the average by $2\cdot6\sigma_{12}$, and (2) those *below* the average in suggestibility exceed those *above* the average by $2\cdot5\sigma_{12}$. It follows that the effect of the training is on the whole independent of differences in intelligence, diligence and suggestibility, but that there may be a slight tendency for the less suggestible and the less diligent to improve more rapidly at first.

At first sight it appears difficult to account for the behaviour of the less *diligent*. If anything one would expect them to improve less not more than the more diligent; but this difficulty disappears on reference to the actual estimates. As stated above the teachers were asked to divide their pupils into five classes for intelligence, diligence and suggestibility respectively (*i.e.* (1) much below the average, (2) below the average, (3) average, (4) above the average, (5) much above the average). An examination of the results obtained in this way shows that there was a strong tendency to put children into the same or at least into two adjacent classes for diligence and suggestibility. But since I grouped classes 4 and 5 together as 'above the average,' classes 1 and 2 as 'below the average' it is evident that the difference would have to be one of two classes in order to become evident in my method of grouping. Such a difference actually only occurred in 15 per cent. of all the estimates obtained. Hence in dealing with exceptionally diligent children, we are on the whole also dealing with exceptionally suggestible children and an explanation that will account for the one will therefore also account for the other. It was stated above that for the purpose of the estimates suggestibility was defined as "readiness to be influenced by members of the staff." The group of highly suggestible children would therefore include those who, through their very anxiety to please, fail to do themselves justice in tests. This may account for the fact that these children did not improve quite as much in test II as the children of low suggestibility, although they had probably responded more effectively to the training given. If this is so, the fact that the difference between the two groups for gain I–III is no longer significant, suggests that twelve weeks had been long enough to enable the highly suggestible children to secure a sufficient advantage over the others in the amount of knowledge at their disposal to balance their inability to do themselves justice at the time of the test.

The conclusion to be drawn is therefore that the highly suggestible probably did derive more benefit from the training given than those of low suggestibility. There is on the other hand no reason to believe that the impression made by the course of lessons was more permanent in their case, for the difference between the two groups is negligible both for gain I–III and for gain I–IV. The proportion of interest lost during the after period must therefore have been about the same for those of high and for those of low suggestibility. It follows that a desire to please those in authority is not necessarily accompanied by a corresponding readiness to form lasting ideals. I shall return to this point towards the end of this paper.

(e) *Summary.*

The results described so far give the effect of the training on the work in the Observation Tests, that is to say the extent to which the training was successful in making the children take more intelligent interest in the everyday objects of their lives. The conclusions derived from them may be summarised as follows:

(1) The training period of twelve weeks was sufficient to produce a decided improvement in the observation work of the children directly under the influence of the trainer.

(2) It probably also had some effect on the work of the children who belonged to the same classes as those trained but did not take part in the special lessons given.

(3) In all schools but one the impression made was strong enough to remain effective for at least four months after the end of the period of training.

(4) The school in which there was no after effect is the only one in which the children were distributed among the different classes immediately after the end of the period of training. This suggests that an influx of new interests during the after period tends to check the growth of the ideal.

(5) The effectiveness of the training depends very slightly if at all on individual differences in intelligence, diligence and suggestibility. In gain I–II the children below the average in suggestibility improved slightly more than those above the average, but this difference is probably due to frequent inability on the part of the latter to do themselves justice in tests; it is unlikely that it should mean that they really had less knowledge at their disposal.

(6) For gain I–IV the difference between the work of the children above and that of those below the average in suggestibility is only $0{\cdot}5\sigma_{14}$. It would appear from this that the permanence of the impression made by the training does not depend on the children's desire to please those in authority.

VI. THE EFFECT OF THE TRAINING ON CERTAIN GROUPS OF CHILDREN.

The effect of the training can be considered from another point of view. So far we have only examined the work of the sections as a whole; but since each section itself consisted of children from two different classes, it in turn falls naturally into two sub-sections: (1) the class of the teacher who was responsible for the training, and (2) the class of the teacher who took no part in it. It will therefore be interesting to see to what extent the effect of the training varied from sub-section to sub-section. For convenience of reference, I shall refer to (1) as the 'trainers' classes,' to (2) as the 'other classes.'

TABLE V. *Observation Test.* (Schools *P, Q, R* and *S.*)

A. *Excess of trainers' classes over other classes.*

	Average gain per cent.			Average gain in terms of standard error		
	I–II	I–III	I–IV*	I–II	I–III	I–IV*
(1) Trained section	$-4{\cdot}7 \pm 5{\cdot}9$	$+12{\cdot}4 \pm 4{\cdot}8$	$+1{\cdot}1 \pm 5{\cdot}5$	$-0{\cdot}8\sigma$	$+2{\cdot}6\sigma$	$+0{\cdot}2\sigma$
(2) Non-trained section	$-14{\cdot}7 \pm 6{\cdot}4$	$-6{\cdot}9 \pm 6{\cdot}2$	$-9{\cdot}2 \pm 5{\cdot}9$	$-2{\cdot}3\sigma$	$-1{\cdot}1\sigma$	$-1{\cdot}6\sigma$

B. *Excess of non-trained section over non-trained schools.*

	Average gain per cent.			Average gain in terms of standard error		
	I–II	I–III	I–IV*	I–II	I–III	I–IV*
(1) Trainers' classes	$-3{\cdot}7 \pm 4{\cdot}3$	$+3{\cdot}9 \pm 4{\cdot}0$	$+0{\cdot}4 \pm 4{\cdot}6$	$-0{\cdot}9\sigma$	$+1{\cdot}0\sigma$	$+0{\cdot}1\sigma$
(2) Other ,,	$+10{\cdot}8 \pm 4{\cdot}9$	$+9{\cdot}7 \pm 4{\cdot}4$	$+9{\cdot}6 \pm 4{\cdot}8$	$+2{\cdot}2\sigma$	$+2{\cdot}2\sigma$	$+2{\cdot}0\sigma$

C. *Excess of trained section over non-trained section.*

	Average gain per cent.			Average gain in terms of standard error		
	I–II	I–III	I–IV*	I–II	I–III	I–IV*
(1) Trainers' classes	$+12{\cdot}2 \pm 6{\cdot}8$	$+17{\cdot}5 \pm 7{\cdot}0$	$+13{\cdot}0 \pm 10{\cdot}8$	$+1{\cdot}8\sigma$	$+2{\cdot}5\sigma$	$+1{\cdot}2\sigma$
(2) Other ,,	$+1{\cdot}2 \pm 7{\cdot}2$	$-1{\cdot}8 \pm 8{\cdot}1$	$+2{\cdot}7 \pm 7{\cdot}5$	$-0{\cdot}2\sigma$	$-0{\cdot}2\sigma$	$+0{\cdot}4\sigma$

* Excluding school *S*.

The rates of improvement of these groups are given in Table V. The following results can be obtained from them:

(*a*) *The work of the trained section.*

In the trained section the difference between the rates of improvement of the trainers' classes and that of the other classes is negligible for gain I–II. In gain I–III the trainers' classes gain $+ 2{\cdot}6\sigma_{13}$ more than the other classes, in gain II–III the corresponding difference is $+ 3{\cdot}0\sigma_{23}$. This shows that the trainers' classes improved more rapidly than the other classes in the latter half of the period of training. The advantage gained in this way was however only temporary for in gain I–IV the difference between the two classes is once more negligible.

(*b*) *Comparison of the non-trained sections with the non-trained schools.*

In the non-trained section it will be more satisfactory to compare the rate of improvement of each group with that of the non-trained schools. As before school S will have to be excluded for gain I–IV in order to obtain a true impression of the after effect of the training under favourable circumstances (see § V (*b*)). The results obtained in this way are as follows:

(1) The rate of improvement of the non-trained sections of the *trainers' classes* exceeded that of the non-trained schools by $-0{\cdot}9\sigma$ for gain I–II, by $+1{\cdot}0\sigma$ for gain I–III and by $+0{\cdot}1\sigma$ for gain I–IV. We are therefore justified in assuming that the training made no appreciable impression on these children.

(2) The rate of improvement of the non-trained section of the *other classes* exceeded that of the non-trained schools by $+2{\cdot}2\sigma_{12}$ for gain I–II, by $+2{\cdot}2\sigma_{13}$ for gain I–III and by $+2{\cdot}0\sigma_{14}$ for gain I–IV. This suggests that there was a slight tendency for the non-trained sections of the other classes to improve more rapidly than the non-trained schools.

(*c*) *Comparison of the trained with the non-trained section.*

(1) Comparing the work of the two sections of the '*trainers' classes*' we find that the excess of the trained over the non-trained is $+1{\cdot}8\sigma_{12}$ for gain I–II, $+2{\cdot}5\sigma_{13}$ for gain I–III and $+1{\cdot}2\sigma_{14}$ for gain I–IV. It would appear from this that there is a slight tendency for the trained section to derive more benefit from the training during the latter half of the period of training.

(2) A similar comparison with regard to the two sections of the 'other classes' shows that their rates of improvement are almost identical. The difference is in every case less than ·4 of the standard error. Hence the non-trained section improved as rapidly as the trained section, although it did not take part in any of the lessons in quick perception. If further proof be needed, this shows once more that the intrinsic value of the special exercises must have been nil.

It is evident from (*a*) that in the only instance in which the results show any difference in the effect of the training on the trained section of the trainers' classes as compared with its effect on the same section of the other classes, this difference is in favour of the trainers' classes. Yet we are forced to conclude from (*c*) that the trained section of the trainers' classes had only quite a slight advantage over the non-trained section of these classes and from (*b*) that these non-trained sections derived no benefit whatsoever from the training given. The obvious conclusion to draw from this, is that the training can only have had a very slight effect on the rate of improvement of the trained section. But this we know to be untrue; for the actual effect of the training on the trained section as a whole is presented by $+ 3\cdot7\,\sigma_{12}$, $+ 4\cdot6\,\sigma_{13}$ and $+ 3\cdot7\,\sigma_{14}$ for gain I–II, gain I–III and gain I–IV respectively (see § V (*a*)).

This apparent contradiction is due to the fact that the standard deviation of the smaller groups (*e.g.* trained section of trainers' classes) were in each case of about the same magnitude as the corresponding standard deviation of the whole section (*e.g.* trained section). Since these groups were each roughly half the size of the section of which they formed part, it follows that they had much larger standard errors. Hence differences which would have been three to four times the standard error as a whole are only two to three times the standard error for the smaller groups.

Before leaving this part of the subject, it will be worth while to discuss what was the origin of the differences in the behaviour of the two classes as observed (1) in the trained section, and (2) in the non-trained section.

(1) During the latter half of the period of training the trained section of the trainers' classes derived more benefit from the training than the same section of the other classes, the difference being $3\,\sigma$ (see (*a*)). It is true that the former were on the average a year older than the latter and it may be thought that they were consequently more able to benefit by the training given; but since the correlation with estimated intelligence was found to be approximately zero in every case, this is not a very likely explanation.

Further since the other classes were in every case the younger children who were allowed to work with the older ones for a short time every day, this difference is not likely to be due to any lack of good-will on the part of the children. It may be that the trainer, in the ordinary school work, unconsciously laid more emphasis on the value of good powers of observation than he would have done under normal circumstances. But if this had been the case to any marked degree, the non-trained section of the trainer's class should at least have done better work than the non-trained section of the other class, and exactly the reverse has been found to be the case. The explanation must therefore be sought in some difference between the children. The most obvious one is that members of the trainer's class profited more than the others, because his mere presence would frequently serve as a reminder to his own children and would thus tend to keep his desires in this respect more vividly before them.

(2) The difference between the trainer's class and the other class in the non-trained section if due to more than chance suggests that the former may have felt no interest in the experiment and therefore made no effort to become more observant (see (*b*) and (*e*)). Possibly a sense of injury at not being allowed to partake in the special lessons prevented some of them from even trying to do good work at the time of the tests, although this was not evident from the attitude of the classes during the tests. The net result is that their marks keep more closely to those of the non-trained schools than those of the other sections and occasionally even fall slightly below them.

In the 'other classes' the children would not mind so much that they had not been selected for the special lessons. The non-trained sections of the trainers' classes had been turned out of their own class-rooms and had been sent to join younger children in an ordinary reading or writing lesson, whilst their comrades were having a new sort of lesson from their own teacher. The non-trained sections of the other classes simply had not had the good fortune to be allowed to join in the work of the older class. There is a vast difference between being denied a right and being denied a privilege. Contrary suggestion was probably rife among the excluded members of the trainers' classes, but those of the other classes would provide excellent soil for the growth of a desire. Their self-subjective instinct would be stimulated and they would be anxious to find out all they could about the special lessons. How good a soil they did provide is seen by the fact that they improved as rapidly as the trained section of their class without being present at any of the special lessons.

In conclusion it will be worth while to return once more to the effect of the training on the non-trained section. It was pointed out in § V (*b*) that this effect appeared to be very slight, but the results obtained in this paragraph suggest that it is unsatisfactory to consider the work of this section as a whole, since the trainers' classes did not improve at all under the influence of the training. It will therefore be of interest to see what results would be obtained under the assumption that the non-trained section consisted (1) entirely of other classes, and (2) entirely of trainers' classes, the standard errors being assumed to be those given in Table II (cf. (*e*)).

(1) If the whole of the non-trained section had consisted of 'other classes' and the standard error had remained that given in Table II, then the excess of the rate of improvement of this section over that of the non-trained schools would have been $+ 3{\cdot}0\,\sigma_{12}$ for gain I–II, $+ 2{\cdot}75\,\sigma_{13}$ for gain I–III and $+ 2{\cdot}65\,\sigma_{14}$ for gain I–IV.

(2) If similar calculations were made on the basis of the rate of improvement of the 'trainers' classes,' it would be found that the excess of the improvement of the non-trained sections over that of the non-trained schools was $- 1{\cdot}1\,\sigma_{12}$ for gain I–II, $+ 1{\cdot}3\,\sigma_{13}$ for gain I–III and zero for gain I–IV. These differences are one and all negligible.

Comparing (1) with (2) it becomes evident that the improvement observed in the work of the non-trained section as a whole is merely the result obtained by compounding two opposing tendencies. In gain I–II the work of the other classes improved rapidly whereas that of the trainers' classes fell below that of the non-trained schools with the result that the gain of the section as a whole is only 1·4 of the standard error (see Table III A). In gain I–III the excess of the sections over the schools is slightly less for the other classes ($+ 2{\cdot}75\,\sigma_{13}$ as compared with $3{\cdot}0\,\sigma_{12}$) but somewhat greater for the trainers' classes ($+ 1{\cdot}3\,\sigma_{13}$ as compared with $- 1{\cdot}1\,\sigma_{12}$), and in consequence of this the effect of the training as a whole is represented by $2{\cdot}3\,\sigma$ (see Table III A). Finally in gain I–IV the improvement due to training was zero for the trainers' classes and $+ 2{\cdot}65\,\sigma$ for the other classes. It is evident from this that it is useless to base any inferences on the work of the non-trained section as a whole; each class must be considered separately in any discussion of the indirect effect of the training. From what has just been said the actual effect produced was as follows:

(1) The non-trained section of the trainers' classes were not affected at all by the training given.

(2) The non-trained section of the other classes probably did derive some benefit from it at any rate during the first six weeks of the training. The decrease observed during the latter half of the training period may have been due to loss of novelty. It is interesting to note that there is in this case no further loss during the after-period.

VII. TRAINING IN OBSERVATION WITHOUT THE USE OF SPECIAL EXERCISES.

Though the special exercises were apparently valueless in themselves their suggestive force may yet have had an important influence on the amount of improvement actually obtained. The beginner may be able to ride his bicycle quite steadily so long as he imagines that someone is supporting him, but he will most likely come to grief if he unexpectedly discovers that he has been left to his own resources. Similarly the belief that the exercises were making them more observant may have been a great help to the trained section by giving them self-confidence. Possible also the non-trained sections would have been less interested in the training if no concrete exercises had been given. It therefore seemed worth while to conduct another series of experiments in which it would be possible to observe the effect of using the ideal by itself without such help.

1. *Method of training by means of ideals.*

The new conditions made it impossible to use two classes in each school as was done in the first experiment. It is in any case a very delicate piece of work to stimulate the growth of an ideal in children by means of direct instruction without producing contrary suggestion. I feel sure that it could not but have been a failure if an attempt had been made to do this at stated times when children had been brought together from different classes foı that purpose. It might perhaps be thought that I could have used parallel classes which were rearranged among themselves for certain subjects, but I think it would have been impossible to obtain the requisite amount of uniformity in the different schools. I therefore decided that it would in this case be better not to attempt to measure any indirect effects on children not directly under the influence of the trainer.

The experiment was carried out in the autumn of 1914 in six High Schools for girls. In three the tests only were given, in the other three we tried to develop an ideal of observation as well. 106 girls were examined in all, of whom 49 belonged to the former, 57 to the latter set of schools.

The following instructions were given to the form-mistresses who kindly undertook to train their girls:

(1) Make the girls want to keep their eyes open—at home and at school, at work and at play.

(2) Encourage them to ask questions about anything that is puzzling them and if possible show them how to find their own answers.

(3) Encourage them to notice essentials rather than details both in their reading and in other occupations.

2. *Method of testing effect of training given.*

The period of training was in this case reduced to nine weeks because I wished to avoid the disturbing effect of end of term examinations. Tests were given at the beginning and at the end of the period of training and again nine weeks after the training had stopped. The tests were given in cyclic order as in the elementary schools. As only three tests were used, only three schools were needed, but as just stated the tests were also given at another set of three schools where no special training was being attempted, so that it might be possible to distinguish the effects of the training from the improvement due to the mere repetition of the task.

I shall refer to the former of these two sets of schools as the observation trained schools, to the latter as the non-trained schools. Of the tests used in the first experiment the Observation Tests alone were retained on this occasion. As the training had had no effect on the Composition and Numbers Tests it did not seem worth while to repeat these. In their place two other tests were given. One of these was chosen in the hope of obtaining some evidence of transfer. It seemed possible that the girls in their desire to become more observant might develop the habit of looking more carefully at whatsoever attracted their attention and if so I thought it might also make them more accurate in copying drawings. I therefore gave them what I shall call the '*drawing test.*' It consisted of a large painted flower which was put on the blackboard for the girls to copy. The only instructions given were:

"Make your copy as large as possible and do not hurry to finish it. Do as much as you can do properly in the time." The time allowed was twenty minutes.

In correcting these paintings, marks were given for correct observation, not for skill. For instance, if a girl had tried to show that a certain leaf was turning brown, she was given the same amount of credit for it whether her reproduction of the fact was or was not skilful, but if she made too large a proportion of the leaf brown, then she lost marks for lack of observation. My results show a very high standard deviation. Possibly this would have been avoided if the girls had been given a smaller piece of work, so that they would all have had plenty of time to finish it.

The other test that was introduced for the first time in this experiment was intended to test the girls' power of grasping the essentials of what they read. For this purpose I used descriptions of well-known historical events such as Frobisher's discovery of North Canada. Each passage consisted of about 250 words and was constructed to contain (1) the answer to a definite question; (2) material connected with the answer but giving it in a less definite form; (3) material which had nothing whatever to do with the question. In the case of Frobisher's discovery of North Canada the passage given described what he and his men did and saw during their stay in North Canada. The question given was "What kind of men did Frobisher find in North Canada?"

After a blackboard demonstration with another passage, each girl was given a printed copy of the passage, the question was written on the blackboard and the girls were told to underline those phrases which gave most of the answer to the question in the fewest possible words and to cross out those that had nothing to do with the question. In marking, two marks were given for every idea correctly underlined or crossed out, one mark if it had been left when it should have been underlined or crossed out, no marks if it had been crossed out when it should have been underlined or vice versa. The girls were given enough time to finish without having to hurry.

My object in giving the test this particular form was to obtain a measure of the children's power of intelligent reading, which would involve neither their range of vocabulary nor their power of self-expression. The form of the tests obviously excludes the latter of these; to ensure the former, I was careful not to use any uncommon words and offered to tell the girls the meaning of any words they did not know. As a matter of fact, I was very rarely asked for help.

Referring to the instructions given to the form-mistresses it will be seen that the last of these was: "Encourage the girls to notice essentials rather than details both in their reading and in their other occupations." It was the effect of the first half of this instruction that the test just described was intended to measure. Though superficially quite different from the rest of the training, it will be seen that this at bottom also implied practice in 'purposeful looking.' Its object was to stimulate in the girls a desire to get at the real meaning of all they read, whereas the rest of the training was intended to make them take more intelligent interest in the events of everyday life. In other words, the former concentrated on concrete objects, the latter on ideas expressed in words, but both were intended to develop in the girls the wish to acquire enough knowledge and power to enable them to take an intelligent interest in their environment (including therein books as well as things).

The test differed from the Observation Test in that it measured increase of power instead of development of desire, but it seemed impossible to construct a test which would avoid this difficulty and would yet be applicable in a number of different schools. As a matter of fact increase of power would in the main be the result of practice, and the intensity, if not the amount of practice, would depend to a large extent on the strength of the desire to learn to read intelligently. In spite of this drawback, the test should therefore give a fairly reliable measure of any increase in this desire. In another direction this '*intelligent reading test*' may be considered to be more satisfactory than the Observation Test. Owing to the form in which it was presented, it was possible to reduce the effect of differences in memory to a minimum. There would therefore be practically no reduction in the amount of gain owing to inability to remember relevant facts, so that the actual amount gained may in this case be taken to show the total effect of the training instead of only showing its effect in so far as this was not obscured by inability to remember the right thing at the right time (cf. § III (1)).

3. *The effect of the training.*

Before discussing the results obtained in these tests, it will again be necessary to see, whether the three differences between the progress of the trained and that of the non-trained (as deduced from the work of three pairs of schools) are sufficiently alike to allow us to express the average obtained from them in terms of the standard error calculated from the work of the individuals; for, if this is not the case, it will not be possible to assign any meaning to this average (see § III (3)).

I shall refer to the schools of this series as schools *A*, *B* and *C*. As before corresponding schools will be schools which had the tests in the same order so that the differences between the rate of improvement of observation trained school *A* and that of the corresponding non-trained school *A* will be due to the training given, whether or no the two sets of the test material used respectively at the beginning and at the end of the period under consideration were of exactly the same difficulty. Using the method described in connection with differences in rate of improvement in the elementary schools, we find that drawing is the only test for which the three pairs of schools can be considered to form homogeneous records within the limits of errors of sampling. Both for the observation tests and for the intelligent reading tests school *B* had to be excluded in order to obtain records in which it was permissible to express the average rate of improvement in terms of the standard error of the individuals. It was therefore necessary to consider school *B* separately in constructing Table VI. As was pointed out in connection with the Composition Tests, this in no way affects the value of the results obtained, so long as we do not attempt to compare the rate of improvement in one part of the experiment with that in another part of it (*e.g.* gain I–II with gain I–III) for any one set of schools.

TABLE VI. *Excess of improvement in observation trained schools over that in non-trained schools.* (Schools *A*, *B* and *C*.)

Test	Average gain per cent. I–II	Average gain per cent. I–III	Average gain in terms of standard error I–II	Average gain in terms of standard error I–III
Observation (schools *A* and *C*) ...	$+32·5 \pm 5·2$	$+17·7 \pm 6·4$	$+$ **6·3**σ	$+2·75\sigma$
Intelligent reading (schools *A* and *C*)	$-16·8 \pm 6·2$	$+10·3 \pm 5·0$	$-\ 2·7\sigma$	$+2·05\sigma$
,, ,, (school *B*) ...	$+78·8 \pm 7·9$	$+23·9 \pm 7·5$	$+$ **10·0**σ	$+$ **3·2**σ
Drawing (schools *A*, *B* and *C*) ...	$-\ 4·6 \pm 6·6$	$-\ 1·3 \pm 6·9$	$-\ 0·7\sigma$	$-0·2\sigma$

(*a*) *Observation Tests.* I shall begin with the results obtained in the Observation Tests as calculated from schools *A* and *C*.

Table VI shows that the excess of the rate of improvement of the observation trained over the non-trained was $+\ 6·3\sigma_{12}$ for gain I–II. This compares favourably with an excess of $+\ 4·6\sigma$ observed in the trained section of the demonstration schools at the end of its period of training especially as the latter was twelve weeks, the former only nine weeks in length[1]. But the course of training, though so successful for the time being, was evidently not long enough to make a lasting impression, for in gain I–III, the excess of the trained over the non-trained is only $2·75\sigma_{13}$. In fact the decrease from $6·3\sigma$ to $2·75\sigma$ in an after period of only nine weeks makes it doubtful whether it is safe to conclude that there was even a slight effective desire left at the end of the after period; for the improvement represented by $2·75\sigma$ may well be due to nothing more than an increase of knowledge produced by the work of the first nine weeks. If so it merely shows that the girls remembered a certain amount of what they had learnt during the first nine weeks, not that they had retained a slight though still effective

[1] It should however be remembered that in the demonstration schools only part of the section belonged to the class of the trainer. If the whole section had improved at the rate of this part there would have been an excess of $5·9\sigma$ instead of one of $4·6\sigma$.

desire to become more observant. This argument does not apply to the work of the demonstration schools, partly because the decrease during the after period was in their case only from $4{\cdot}6\sigma$ to $3{\cdot}7\sigma$ (excluding school S in both cases) partly because the after period was four months instead of nine weeks, so that it is very unlikely that much would have been remembered from the training period if all interest had been lost during the interval.

In schools B the excess of the trained over the non-trained is -40 per cent. for gain I–II and -23 per cent. for gain I–III. This loss of marks is explained by the report given by the mistress in charge of this class. She definitely felt her training to be a failure and apparently did not attempt to continue it after the first few weeks of the period of training.

(*b*) *Intelligent reading test.* Table VI shows that for schools A and C the excess of the trained over the non-trained in this test is $-2{\cdot}7\sigma_{12}$ for gain I–II, $+2{\cdot}05\sigma_{13}$ for gain I–III. This suggests that the training was disadvantageous whilst it lasted, though it is possible that a slight benefit was derived from it during the after period. If the excess of $2{\cdot}05\sigma$ is due to more than mere chance, then it is just possible that the difference $-2{\cdot}7\sigma$ was due to nervousness on the part of the observation trained girls in the second test. They knew that the experiment was being carried out in a number of schools, they were very anxious that their own school should do best and they must have realised that some of the teaching given during the first period of nine weeks was intended to help them in this test. It may of course be that it was the training itself that was unsuccessful, but there is nothing in the reports obtained from the schools that would induce one to think that it was felt to be unsatisfactory by the person responsible for it. On the contrary it seems to have been considered the easier part of the work in each of the three schools.

If we now turn to school B we find that just the reverse has happened. In this school the training was apparently highly successful, for the excess of its rate of improvement over that of the corresponding non-trained school is $+10{\cdot}0\sigma_{12}$ for gain I–II and $+3{\cdot}2\sigma_{13}$ for gain I–III. Some of the improvement represented by $10{\cdot}0\sigma_{12}$ is no doubt due to the fact that observation trained school B scored very low marks in test I (25 as compared with 37 in school A and 34 in school C), but if this were due to special conditions which only applied to test I, gain I–III would have been more than $3{\cdot}2\sigma_{13}$. We may therefore conclude that the training given did produce a definite improvement in school B

but that this advantage was quickly lost after the end of the period of training.

Comparing the result obtained in the Observation Test with that obtained in this test we find that the effect of the training was in each case only temporary but that the part of the training tested by the former was for the time being markedly successful in schools A and C, that tested by the latter in school B. This suggests that there may be an inverse correlation between rates of improvement in these two tests This is however not the case; the correlation is found to be low but positive in every instance, the average for the three observation trained schools being given by $r = + \cdot 26$. We must therefore conclude that the observed differences in the rates of improvement in the two tests are due to differences in the amount of emphasis laid on the two parts of the training by the person responsible for it or in her manner of presenting the two ideals.

(*c*) *Drawing.* The last test that has to be considered is the Drawing Test. It was stated above, that this was selected in the hope of obtaining some sign of transference, but Table IV shows that this is not the case. For gain I–II the excess of the trained over the non-trained is $\cdot 7\,\sigma_{12}$, for gain I–III the difference is even smaller. It follows that the training given had no effect whatsoever on the accuracy with which the girls copied the painting that was given them for this purpose.

(*d*) *Neatness.* If sufficient time is allowed for a piece of written work, and if the work is not familiar enough to cause a rush of ideas, those who are most anxious to do good work will on the whole tend to work most slowly; for they will tend to criticise such thoughts as rise to consciousness before they write them down and will do their utmost to make their memory drawings really correct. In consequence of this they will be the most likely to produce a comparatively neat and well-arranged piece of work, although this was by no means the special object they had in view. Thus the amount of thought and care bestowed in a piece of work can be judged from the neatness of the final product, if there has been no need to hurry.

On the whole the girls found that the half hour allowed for the Observation Test was more than sufficient. Observation trained school A was the only one in which several girls found that they had not enough time to finish their work. It will therefore be worth while to see how the observation papers of the observation trained schools compare with those of the non-trained schools in regard to neatness, in order to obtain a measure of the amount of effort made at the time of the test.

In marking the observation papers for this purpose I allowed 5 marks for the drawing, 5 marks for the handwriting and 5 marks for method giving a maximum of 15 marks. The two terms 'drawing' and 'handwriting' explain themselves; by method I mean such things as making suitable headings, beginning each answer on a new line, numbering answers, etc. In each case credit was only given for the care with which the work had been done; thus full marks might be given for a very unskilful drawing or an equally awkward handwriting if the work gave the impression that sufficient pains had been taken with it. On the same principle, no marks were deducted if the contents of the paragraph did not correspond with the heading, so long as the whole gave the impression that the writer thought they corresponded.

In marking the papers from this point of view, there will be no point in excluding school *B*, since there may well have been an increase in effort at the time of the test although the training was a failure in other respects. Altogether the whole method of pairing the schools according to the order in which they had the test material becomes meaningless, when the papers are being marked for neatness and not for subject matter; it will be more satisfactory to pair them according to their average rate of improvement in neatness; for it is only logical to suppose that the non-trained school which improved most in neatness is either the one in which most stress was being laid on neatness in the ordinary school work or the one in which most interest was being taken in the actual tests.

When the schools were paired in this way and calculations were made on the lines described in § III (3) it was found that the greatest difference between any two pairs of schools was 1·4 of the standard error of the difference. The average difference as determined from the three pairs of schools can therefore be taken as a measure of the effect of the training on the neatness of the papers. Table VII gives both the amount of improvement in the individual schools and the average calculated from them. It will be seen that for schools *B* and *C* the tendency of the non-trained schools is to do the first test more neatly than the others, whereas that of the observation trained schools is in the opposite direction. In schools *A* just the reverse appears to happen. The behaviour of the non-trained school *A* must be due to local causes of some kind; that of the observation trained school is no doubt due to the fact that this was the school in which the girls ought to have had more time for their work.

This explanation does not however apply to the non-trained schools

B and C; the children there had plenty of time to finish their work on each occasion. My impression is that the loss of marks was in these schools due to a gradual decrease in the amount of effort made at the time of the test. The first time the novelty of the test material and the fact that they were going to be used for an experiment both greatly appealed to the girls. We may take it for granted that few if any failed to do their best on that occasion. But when the same experimenter came for a second and a third time and each time brought the same kind of test material some of this interest would necessarily wear off. In support of this it is worth while to mention that one of the non-trained classes frankly owned to being disappointed with the second test. Such a loss of interest would necessarily carry with it a decrease in the amount of effort made at the time of the test and this, as suggested above, would tend to have an adverse effect on the neatness and arrangement of the girls' work and would thus account for the loss of neatness marks in two of the three schools.

TABLE VII. *Neatness.* (Schools A, B and C.)

	Average gain per cent.				Excess per cent. of observation trained schools over non-trained schools	
	Observation trained schools		Non-trained schools			
	I–II	I–III	I–II	I–III	I–II	I–III
School A	$-1\cdot8$	$-2\cdot9$	$+4\cdot4$	$+2\cdot2$	$-6\cdot2$	$-5\cdot1$
„ B	$+4\cdot5$	$+4\cdot5$	$-5\cdot6$	$-3\cdot5$	$+10\cdot0$	$+8\cdot0$
„ C	$+5\cdot7$	$+14\cdot8$	$-16\cdot6$	$-15\cdot7$	$+22\cdot3$	$+30\cdot5$
Average	...	...	...	...	$+8\cdot7\pm3\cdot0$	$+11\cdot1\pm3\cdot3$
Average in terms of standard error			...	...	$+2\cdot9\sigma$	$+3\cdot4\sigma$

Turning to the averages given in Table VII, we find that the excess of the trained over the non-trained is $2\cdot9\sigma_{12}$ for gain I–II and $3\cdot4\sigma_{13}$ for gain I–III. If we may take these differences as measures of the amount of effort made at the time of the test, it follows that the girls in the observation trained schools were on the whole more anxious to do good work than those in the non-trained schools. This tendency is most evident in the work done at the end of the after-period. As stated above, the amount of improvement on subject matter observed in schools A and C was only $2\cdot75\sigma_{13}$ for gain I–III as compared with $6\cdot3\sigma_{12}$ for gain I–II. It follows that there was an increase in the amount of effort made to do the actual test well at a time when the motive force of the desire to become more observant was already greatly on the decrease. This was not due to any fear of unpleasant consequences, since the girls were definitely told that the work was only being done for the sake of finding out what girls of their age knew and that the marks they obtained in the tests would in no way affect their school records. We may therefore assume that they retained a belief that this was the kind of thing about which they ought to know something, for some weeks after the desire to become more observant had grown too weak to induce them to make any very serious effort to act up to their belief. If this is so, it is a good illustration of the fact that the belief that a certain line of action is right may exist without the corresponding desire to pursue that line of action.

(*e*) *Individual difference in intelligence, diligence, and suggestibility*. We have finally to consider whether the amount of impression made by the training was in any way dependent upon individual differences in intelligence, diligence, or suggestibility. The method adopted for this purpose was that described in § IV (*d*) in the course of the discussion of the effect of these differences on the demonstration schools. It was there shown that the less suggestible and the less diligent probably improved more than the others for gain I–II but that the effect produced by the training was otherwise independent of the differences under discussion. The results obtained in the observation trained high schools suggest that the individual differences were even less important in these schools, for the rates of improvement of any two complementary groups were in every case found to be within twice the standard error of each other. It may be, that the effect of the training was independent of suggestibility in gain I–II because the interval between the first and the second test of this series was nine weeks instead of six and thus gave the highly suggestible children more opportunity to acquire an amount of knowledge sufficient to balance their inability to do themselves justice at the time of the test.

(*f*) *The results obtained from this set of schools* may be summarised as follows:

(1) Under favourable circumstances training by means of ideals only may be successful both in observation and in intelligent reading, but most of the improvement secured by a training period of nine weeks is lost within the next nine weeks.

(2) The training seems to produce an improvement in the amount of effort made at the time of the test, this tendency is more marked in the third test than in the second.

(3) There is no improvement in the accuracy with which a given painting is copied.

(4) The effect produced by the training does not depend on individual differences in intelligence, diligence or suggestibility.

It was pointed out above that the schools *A* and *C* derived more benefit from nine weeks' training by means of ideals only than the trained section of the demonstration schools derived from twelve weeks' training by means of ideals and special exercises. Evidently therefore the suggestive value of these exercises cannot be an important factor in the improvement produced. Yet Miss Aiken believes that it is due to these exercises that the pupils developed a "habit of quick perception" and a "habit of accuracy in seeing and hearing[1]." My results lead me to think that it was rather her enthusiasm which stimulated them to form ideals of quick perception and of accuracy in seeing and hearing and that the observed improvement is really due to the effort made by her pupils in response to her teaching. In other words my impression is that the results obtained by her would have been just as striking, if the actual exercises had been omitted.

This does not imply that it would not at times be better to have some course of special lessons in connection with the cultivation of an ideal. It is worth while to note that the training failed in none of the four elementary schools, in each of which it was based on a definite course of lessons, whereas it did fail absolutely in one of the three high schools; also that the mistresses who undertook this second training all felt it to be a difficult piece of work, whereas no special objections to it were raised by the first set of trainers.

Probably the conclusion to be drawn from the results as a whole is that it is better to use some course of lesson as a basis for this kind of training, although it is of no importance what the subject matter of the course happens to be, so long as the training can be made to appear to be a natural outgrowth from it. Skilfully managed such a course may even be conducted so as to stimulate the growth of the desired ideal in the pupils without any direct instructions on the part of the teacher, and ideals, inculcated by means of indirect suggestions, are, as is well known, much the most likely to remain effective throughout life. If this is not possible, the lessons will at any rate create a natural atmosphere for such direct instructions as has to be given, with the result that there is under these conditions less risk of rousing contrary suggestion by anything which may appear to the children to savour of 'preaching.' It is only in this sense that the lessons themselves can be considered of value n the cultivation of ideals.

[1] *Op. cit.* p. 26.

VIII. The effect of developing an ideal of neatness.

The experiments I have described so far were undertaken in order to see whether the desire to become more observant could be developed as a generalised ideal. We did this by stimulating the desire in a number of school children and then testing the effect this had on the quality of their work in certain tasks selected specially for this purpose. I wished to see how the work in the same tests would be affected if the ideal of neatness were developed instead of that observation. I therefore gave the tests in the same way to another set of three high schools, *i.e.* each school began with a different test and each had one test at the beginning and one at the end of a nine weeks' period of training as well as a third test nine weeks after the end of the course of training. The only difference between this experiment and the last, was that these children were trained in neatness and not in observation. The instructions given were: "Make the girls want to grow more neat and insist on neat work both in class and at home." I shall refer to the schools in which this training was given as the 'neatness trained' schools in order to distinguish them from the observation trained schools on the one hand and from the non-trained schools on the other. My reasons for choosing this particular ideal were (1) that I had reason to expect from Ruediger's experiment that I should obtain a measurable improvement in the neatness of the work, and (2) that I was able to measure this improvement in the observation papers and was therefore not under the necessity of constructing further special tests for this purpose. The fact that the children had no idea that the tests I gave them would be marked for neatness, made the marks all the more valuable as a measure of the extent to which an ideal of neatness had been formed.

As stated above there were three neatness trained schools corresponding to the three non-trained schools *A, B, C.* Calculations on the lines explained in § III (3) show that the three values obtained by comparing the rate of improvement of each neatness trained school with the corresponding non-trained school are such, that the three pairs of schools may be considered to form essentially homogeneous records; *i.e.* that it is permissible to express the average difference in rate of improvement, as calculated from them, in terms of the standard error of the individuals. This applies equally to the Observation, the Intelligent Reading and the Drawing tests. The average of the three pairs of differences may therefore in each case be used as a measure of the effect of the training in neatness.

The results obtained are given in Table VIII.

TABLE VIII. *Excess of improvement in neatness trained schools over that in non-trained schools.* (Schools *A*, *B* and *C*.)

Test	Average gain per cent. I–II	Average gain per cent. I–III	Average gain in terms of standard error I–II	Average gain in terms of standard error I–III
Observation	$+9 \cdot 4 \pm 5 \cdot 5$	$+4 \cdot 4 \pm 4 \cdot 9$	$+1 \cdot 7\sigma$	$+0 \cdot 9\sigma$
Intelligent reading	$+9 \cdot 4 \pm 5 \cdot 6$	$+13 \cdot 4 \pm 5 \cdot 8$	$+1 \cdot 7\sigma$	$+2 \cdot 3\sigma$
Drawing	$+17 \cdot 8 \pm 6 \cdot 6$	$+1 \cdot 6 \pm 8 \cdot 0$	$+2 \cdot 7\sigma$	$+0 \cdot 2\sigma$
Neatness	$+13 \cdot 0 \pm 3 \cdot 9$	$+11 \cdot 2 \pm 3 \cdot 5$	$+\mathbf{3 \cdot 3}\sigma$	$+\mathbf{3 \cdot 2}\sigma$

(*a*) *Observation test.* In observation the excess of the neatness trained over the non-trained is $+ 1 \cdot 7\,\sigma_{12}$ for gain I–II and $+ 0 \cdot 9\,\sigma_{13}$ for gain I–III. It follows that the training in neatness did not affect the quality of the observation work either advantageously or disadvantageously.

(*b*) *Intelligent reading test.* In intelligent reading the corresponding values are $+ 1 \cdot 7\,\sigma_{12}$ for gain I–II and $+ 2 \cdot 3\,\sigma_{13}$ for gain I–III. It would appear therefore that the quality of the work in intelligent reading was affected very slightly if at all by the training in neatness.

(*c*) *Drawing test.* In drawing the excess of the neatness trained over the non-trained is $+ 2 \cdot 7\,\sigma_{12}$ for gain I–II, but only $+ 0 \cdot 2\,\sigma_{13}$ for gain I–III. This suggests that the training may have made the girls temporarily more accurate in their drawing, but that the period of nine weeks was again not long enough to produce an improvement of any permanence.

(*d*) *Individual differences in intelligence, diligence and suggestibility.* If the papers are marked for neatness (see V (4)) and the children are grouped according to these individual differences in the way described in § IV (*d*), it is found that the amount of improvement in neatness is independent of them, a result which corresponds with that obtained in the observation work of the observation trained schools. We are therefore justified in assuming that the amount of impression made by training such as that under discussion does not depend on the individual differences for which estimates were obtained.

(*e*) *Neatness.* The last point to consider is the effect of the training on the neatness of the papers. It is in this that one would expect the most marked improvement to be observable since it was the value of neatness that was being emphasised during the period of training.

In marking the papers for neatness the method adopted was that described in connection with the observation trained schools

(§ V (4)). Also for reasons given there the trained and the non-trained schools were paired according to their rate of improvement so that the neatness trained school which improved most in neatness was paired with the non-trained school that improved most, etc. When this was done it was found that the differences in the excess of the trained over the non-trained for any two pairs of schools was in each case less than 1·6 of the standard error. We are therefore justified in using the difference in the averages obtained from the two sets of schools as a measure of the effect of the training on the neatness trained schools. The results obtained are given in Table VIII. It will be seen that the excess of the neatness trained over the non-trained is $+ 3{\cdot}3\sigma_{12}$ for gain I–II and $+ 3{\cdot}2\sigma_{13}$ for gain I–III. It is evident from this that the training made sufficient impression to produce a significant improvement in the neatness of the work of the children; but though significant the magnitude of the improvement observable at the end of the period of training (*i.e.* for gain I–II) is disappointing when compared with that obtained in observation by the observation trained schools. Referring to Tables III B and VI, it will be seen that this was $4{\cdot}6\sigma_{13}$ and $6{\cdot}3\sigma_{12}$ for the schools in which the training was successful. It will therefore be worth while to compare my results with those obtained by Ruediger in his paper on the "Improvement of Mental Function through Ideals[1]." It will be convenient to begin by summarising both the method he adopted and the results he obtained.

Method. The children were set weekly written papers in three school subjects. These papers were collected and marked for neatness. After three or four papers had been collected in each subject, the value of neatness was emphasised once or twice a week in connection with one only of the three subjects selected, but an attempt was made to develop a true ideal of neatness (cf. *op. cit.* p. 366). This training was continued for a period of eight weeks. Three sets of marks were then obtained by taking the average of each child's marks (1) in the month before the beginning of the training, (2) in the first, and (3) in the second month of the training. Ruediger's gain I–III should therefore correspond roughly with my gain I–II.

Results. The results obtained fall into two classes: (1) the direct effect of the training on the subject in connection with which the importance of neatness was being emphasised, (2) its indirect effect on other school subjects. If we limit our discussion to the two schools in which the training was considered to have been satisfactory, we obtain two

[1] *Educational Review*, November 1908.

values for the direct, four for the indirect effect of the training given. In order to make these comparable with the results I obtained it will be necessary to express them in terms of the standard error of the amount gained (this is easily done since the actual marks scored by each child on each occasion are given in the paper from which these facts are taken[1]). The values for gain I–III obtained in this way are as follows:

	Direct effect	Indirect effect
School I	$10{\cdot}9\sigma$	$6{\cdot}6\sigma$ and $5{\cdot}1\sigma$
School III	$5{\cdot}3\sigma$	$6{\cdot}7\sigma$ and $3{\cdot}9\sigma$

where σ is in each case the standard error for the rate of improvement under discussion.

It is only the indirect effect with which we are immediately concerned, since it was only this that my experiment was intended to measure; but even so every one of the values obtained by Ruediger is greater than mine ($3{\cdot}3\sigma$), three of them markedly so. Yet there is no reason to believe that the training was not satisfactory in schools used for my experiment. Those responsible for it were in each case under the impression that there was a marked improvement in the neatness of the children's work and in one school at any rate the exceptional neatness of the class that had been trained was commented upon nearly twelve months after the end of the period of training, and this by a mistress who had not heard anything about the experiment. The cause of the difference between Ruediger's experiment and mine is probably to be found in the technique of the experiments themselves. Ruediger tested the indirect effect of the development of the ideal on other school subjects, that is to say on work of which the children knew that it would be corrected by a member of the school staff, possible even by the very teacher who was insisting on neatness in another school subject. In contrast with this, my experiment tested the effect of the same ideal on work done under the supervision of an outsider, work too of which the children had every reason to believe that it would not be seen by any member of the school staff and of which they certainly did not suspect, that it was going to be marked for neatness. As a result Ruediger's children improved very markedly in neatness whereas the improvement I obtained was only just significant.

[1] *Op. cit.* p. 368.

IX. The extent to which true ideals were developed by the training.

This puts the whole of the results obtained so far into a new light; for it suggests that both the improvement obtained by Ruediger in response to special training in neatness and that obtained by me in response to special training in observation may merely have had their source in an effort on the part of the children to please the teacher in charge of them. In so far as this was the case the training cannot be said to have developed a true ideal at all; for it simply made the children act in certain definite ways in response to a previously established ideal, namely that of satisfying the demands of their teachers.

It will therefore be necessary to consider separately the cases in which the improvement observed is not likely to have been due to this tendency. There appear to be two such cases. The more obvious of these is the improvement in neatness that was observed in the neatness trained schools, since the children there had no reason to suspect that the neatness of their papers would be taken into consideration at all. The other is the improvement in observation maintained by the observation trained schools during the after period, since the trainer was specially requested to make no further effort to keep the children up to the mark after the end of the period of training. It may be thought that the latter was even so due to a desire to please the trainer since the children were not removed from his influence; but this is rendered very unlikely by the way in which the experiment was conducted. The trainer had little or no means of judging whether the children were maintaining an intelligent interest in the common objects of everyday life, and the children cannot have been actuated by a fear that he would ask to see the last set of papers because they had not been told that I should be giving them a further test in the following term.

In general we may therefore assume that the influence the training had on the work done in the after period cannot be explained in terms of a mere desire to please those in authority. There seem to be two possible explanations for it: it may have been due to the development of a true ideal, *i.e.* a desire to become neat or observant because it is right to do so and it may have been due to the development of an automatic habit of choice during the period of training. These are of course not mutually exclusive.

By a habit of choice I mean the tendency always to make the same choice when faced with the same alternatives. So long as this is done in response to a conscious desire to do the one rather than the other, it is of course a sign that the resulting action is being controlled by a principle of some kind; but when the same choice has been made a sufficient number of times, the alternative which has been rejected on each occasion ultimately ceases to come to consciousness and the action follows immediately on the appearance of the favoured alternative. The choice is then said to have become automatic. That a training period of nine to twelve weeks should have been sufficient to establish the habit on as firm a basis as this, seems unlikely on the face of it and this impression is strengthened by the results obtained by Boyd Barrett in his work on *Motives and Motivation-Tracks*. In his experiment the conditions were much more favourable for the automatization of the act of choice; the choices were always made in the same surroundings, the value of each alternative was well known and the instruction was repeated before each reaction[1]. Yet in spite of all this, the extent to which automatism was actually developed was as follows:

	Experiments			
	1–20	20–40	40–60	60–80
Percentage of automatic choices (calculated from table given on p. 127)	3	13	34	57

This shows that over sixty experiments had to be performed before half the acts of choice had become automatic.

In my experiment none of the conditions were maintained; the children did not even have the benefit of being reminded to 'look purposefully' when there was something to look at. There is therefore every reason to believe that the act of choice had not become automatic during the period of training; the improvement observed during the after period can therefore not be explained as an automatic reaction of the training given. Since it is neither at all likely to be due to a desire to please those in authority, it seems to be fair to assume that it may be taken as a measure of the extent to which a true ideal was developed by the training given.

In examining the extent to which this did occur, it will be simpler to omit those schools in which conditions were for some reason abnormal. This involves (1) demonstration school *S* because the children in this school were distributed among different classes before the newly roused desires had had time to become firmly established (see § IV (*b*)), and (2) one of the observation trained high schools for the Observation Test

[1] *Op. cit.* p. 144.

and the other two for the Intelligent Reading Test, because the training did not have any effect on the work of these schools during the actual period of training. If we exclude these schools, the results obtained are as follows:

Schools	Length of training	Length of after period	Measure of permanent effect of training
Trained section of three demonstration schools (tested in observation)	12 weeks	4 months	$+\mathbf{3{\cdot}7}\sigma$
Two observation trained high schools (tested in observation)	9 „	9 „	$+2{\cdot}75\sigma$
One observation trained high school (tested in intelligent reading)	9 „	9 „	$+\mathbf{3{\cdot}2}\sigma$
Three neatness trained high schools (tested in neatness)	9 „	9 „	$+\mathbf{3{\cdot}2}\sigma$

Of these all but the second are significant. We may therefore assume that true ideals were developed in most cases, though they were not ideals with a very strong motive force behind them. Such as it was, this motive force was evidently more effective in the demonstration schools than in the high schools ($3{\cdot}7\,\sigma$ as compared with $2{\cdot}75\sigma$ and $3{\cdot}2\,\sigma$) but this in itself does not constitute an argument in favour of basing instruction of this kind on some definite course of lessons. If the difference in permanence were really due to the difference in method, we should expect to find a tendency in the same direction at the end of the training period and this is just the reverse of what actually happened. Referring to Tables III and VI we find that the excess of the trained over the non-trained at this stage was $4{\cdot}6\,\sigma$ for the demonstration schools and $6{\cdot}3\,\sigma$ for the observation trained high schools. For the same reason it seems unlikely that the cause of the difference in permanence is to be sought in the attitude *of* the girls towards the ideal we were trying to inculcate. On the whole the most probable conclusion is that the training made a more lasting impression in the demonstration schools because the actual period of training was so much longer.

X. Summary.

Before attempting to discuss the extent to which the experiments described in the course of this paper can be said to throw light on the functioning of ideals it will be convenient to summarise the results obtained from them.

These may be stated as follows:

1. A training period of six to nine weeks is sufficient to develop a temporary desire to take more intelligent interest in the objects of every-day life (see Tables III and VI, gain I–II) but twelve weeks seem to be necessary to make a permanent improvement of any value (see Table III, gain I–IV and Table VI, gain I–III).

2. A course of twelve weeks of special exercises in quick perception on the lines laid down by Miss Aiken in *Methods of Mind-training* does not appear to improve the children's power of taking in things at a glance (see Table I). Neither does it seem to have any particular suggestive value when used as a basis for training by means of ideals; for the schools in which no such exercises were given do not appear to have been under any disadvantage on that account (see Table V and Table VI). On the other hand the fact that the training in observation failed in one of the latter schools (Table VI) and in none of the former suggests that it is easier to make instruction by means of ideals successful, when it is given in a form in which it appears to be the natural outcome of some definite course of lessons.

3. The effect of the training on the non-trained section of the demonstration schools (*i.e.* on those children who did not take part in the special lessons although they belonged to the classes in which the training by means of special exercises and ideals was being given) was two-fold:

(*a*) Those who belonged to the trainer's class did not derive any benefit from the training given probably because they were the older and therefore resented being sent to work with younger children while their companions had the course of special lessons referred to in (2) (see § VI (*b*)).

(*b*) Those who belonged to the other, the younger, class improved as much under the influence of the training as their companions who were taking part in the special lessons. This is equally true of the actual improvement during the period of training and of the extent to which this was maintained during the after period (§ VI (*c*)).

4. In the trained section of the demonstration schools (*i.e.* among those children who took part in the course of special lessons) the permanent effect was the same for both classes but the trainer's class improved more rapidly than the other class during the latter half of the training period. It therefore seems likely that the mere presence of the trainer often acted as a reminder of his wish and thus helped to keep the

idea vividly before the children, after the first novelty of it had worn off (see § VI (*a*)).

5. School *S* was the only one of the demonstration schools in which the benefit derived from the training was entirely temporary. Since it was also the only one of the schools in which the children were distributed among different classes for the after period, it seems probable that the observed check in the growth of the ideal was due to the influence of new interests consequent upon the rearrangement of the classes (see § V (*b*)).

6. The amount of improvement the training effected in the work of the children does not appear to have depended on individual differences in intelligence, diligence or suggestibility, the last of these being defined for the present purpose as readiness to be influenced by members of the staff. It was however noticed that those above the average in suggestibility at first showed a tendency to improve less than those below the average. It may be that this was due to inability on the part of these children to do themselves justice at the time of the test—a frequent accompaniment of high suggestibility in the sense defined. If so they probably derived more benefit from the training during the actual course of training, but there is nothing to suggest that the impression made was at all more permanent in their case.

7. No sign of transference was observed; girls who under the influence of the training improved in 'observation' failed to improve in the accuracy with which they copied a given painting (§ VIII (*c*)).

8. In the observation trained schools improvement in the subject matter of the observation tests was found to be accompanied by improvement in the neatness with which the work was done. Further, comparison of the work done at the end of the after period with that done at the end of the training period showed that there was a marked decrease in the quality of the work as compared with a slight increase in its neatness. It was pointed out that this probably meant that the girls retained the belief that one should try to take an intelligent interest in the things around for some time after the desire to do so had ceased to be effective (§ VII (*d*)).

9. In the neatness trained schools there seems to have been a slight temporary improvement in the drawing test, showing that the training probably made the girls somewhat more accurate in this respect, but that a training period of nine weeks was not sufficient to effect an improvement of any permanence (§ VIII (*c*)).

10. It was found necessary to distinguish carefully between the

improvement due to a desire to please those in authority and the much smaller improvement due to the growth of a true ideal, *i.e.* a desire to become neater or more observant for its own sake. It was shown that the effect of the latter could be seen in two instances: (*a*) in the improvement in subject matter in the observation trained schools at the end of the after period, and (*b*) in the improvement in neatness in the neatness trained schools both at the end of the training period and at the end of the after period (§ IX).

11. The results obtained with regard to 10 (*a*) showed that a training period of twelve weeks was sufficient to produce an ideal with a certain amount of real motive force behind it (§ IX).

XI. CONCLUSIONS WITH REGARD TO THE GROWTH OF IDEALS.

As stated in the introduction the main purpose of the experiment described in the preceding pages was to obtain some insight into the laws underlying the growth of ideals under the influence of direct instruction. It is probable that the impression made by an idea tends to be more permanent when it has been presented in such a form that the subject believes it to be his own discovery that he should do this or omit to do that. There are however many cases in which it is impossible or at least very difficult to proceed on these lines and in which it is none the less necessary to stimulate right desires. It is with these alone that the present investigation is concerned. The actual results obtained in the course of the work were summarised in the last paragraph. I shall now attempt to show to what extent these results help us in solving our original problem.

1. In the experiment care was taken that the children should have every opportunity of realising the value of taking an intelligent interest in their surroundings; the trainer was specially asked to discuss the matter with them and to invite them to express their own opinions. Yet the improvement produced during the training period does not appear to have been primarily the result of conviction, especially in the high schools in which there was such a marked difference between the work done at the end of the training period and that done at the end of the after period (see Table V). Probably the new desire was cultivated at first in order to satisfy a strong previously established desire—due to love of approbation in the trained sections, to rivalry in the other classes of the non-trained sections and as likely as not supplemented in both cases during the first few weeks by that love of novelty which is so

characteristic of all children (§ X 3 and 4). Sooner or later this desire must have acquired motive force of its own (§ X 10) but this seems to have been a matter of slow growth, if we may judge from the fact that nine weeks' training caused an improvement represented by $6{\cdot}3\sigma$ of which only $2{\cdot}75\sigma$ was left at the end of an after period of nine weeks, whereas the corresponding values were $4{\cdot}6\sigma$ and $3{\cdot}7\sigma$ for a training period of twelve weeks and an after period of four months respectively.

2. The desire created in this way appears to have been a true ideal, in that it was based on a conviction that it was a matter of duty to try to become more observant in the sense defined; but there is reason to believe that the conviction outlived the motive force that is necessary to make it effective. In conducting the tests the experimenter did her utmost to make the children realise that there were neither penalties nor rewards attached to the task and that the papers would not be shown to members of the school staff. In spite of this the work of the observation trained high schools at the end of the after period showed a definite loss in the quality of the subject matter accompanied by a slight increase in the amount of effort made at the time of the test (§ X 8). Under the circumstances the most probable explanation of this would seem to be that the girls retained the conviction that it was their duty to become more 'observant' for some time after the motive force behind the ideal had become very weak.

3. Finally the behaviour of the non-trained section gives us an opportunity to study the way in which an idea may spread from individual to individual. We have seen that the two sections of the trainers' classes showed no sign of transfer from trained to non-trained, whereas there was complete transfer in the other classes (§ X 3). It was suggested at the time that the cause of this difference lay in the fact that the former had reason to resent the way in which they were being treated for the purposes of the experiment; whereas the latter not only had no such cause of complaint, but were on the contrary probably thrown into a state of mind, in which they were highly suggestible for anything that occurred in the course of special lessons, on which the training was based. This leads us to believe that training of the kind described is liable to affect not only the actual individuals trained but also such other individuals as live in close contact with them and have the requisite interests in common with them. The only condition for transfer of this kind appears to be that the training must be conducted in such a way as to render the latter suggestible for what occurs in the course of training.

In conclusion it will be worth while to re-state some of the results obtained in the preceding discussion in a form which will show their bearing on the problem of direct moral instruction. This may be done as follows:

(1) Definite improvement usually follows immediately upon direct instruction if it has been presented in a suitable form; but it would be misleading to imagine that such improvement can be taken as a measure of the extent to which a true ideal has been developed. Much of it is merely due to various other motive forces such as love of approbation, love of novelty, rivalry, etc.

(2) Knowledge of what is right is not sufficient to ensure right action.

(3) When direct instruction is used, weeks of careful training are needed to develop a true ideal with any effective driving force behind it and even at the end of such a course of training, a sudden influx of new interests may undo all the good the training has done (§ X 1 and 5).

Any system of direct moral instruction which does not allow sufficient time for this slow process of growth and mistakes knowledge for desire is therefore unlikely to achieve results of any great value.

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PUBLICATIONS RECEIVED.

The Philosophical Review. Vol. XXVI. Nos. 3, 4 and 5. New York: Longmans, Green & Co. 1917. $3.00 per annum.

Mind. No. 103. London: Macmillan & Co. 1917. 4s.

Science Progress. No. 46. London: Murray. 1917.

Bulletin de l'Institut Général Psychologique. Nos. 1–3. Paris: Au siége de la Société. 1917. Abonnement annuel, 20 f.

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THE MIND OF THE WIZARD.

BY CARVETH READ.

1. *The Rise, Fall and Pretensions of Wizardry.*
2. *Characteristics of the Wizard:* (1) *Intelligence;* (2) *Force of Will;* (3) *Motives;* (4) *Costume;* (5) *Jealousy;* (6) *Histrionic temperament;* (7) *Hysteria and Power of Suggestion.*
3. *The Wizard and the Sceptic.*
4. *The Wizard's Persuasion.*

1. THE RISE, FALL AND PRETENSIONS OF WIZARDRY.

IN describing the occult arts and those who practise them, terms are so loosely used that it may be convenient to premise that by 'Wizard' (or Medicine-man) is here meant either a magician or a sorcerer; that is, either one who puts into operation impersonal magical forces (or so far as he does so), or one who relies upon the aid (or so far as he does so) of ghosts or spirits under magical control. It is an objection to this use of the word 'sorcerer,' that it is often applied to those chiefly whose practice is maleficent: but there seems to be no word used only in the more general sense; and the difference between maleficent and beneficent wizards, whether magicians or sorcerers, will here be marked, when necessary, by the familiar epithets 'black' and 'white.'

In backward societies, wizardry is, or may be, practised by every man or woman[1]; and, indeed, in its simpler operations or observances this is true at every stage of culture. But in every kind of task it appears that some men can do it better than others, and they attract the attention of the rest; and probably this is the beginning of the differentiation of the professional wizard: he is at first merely one whom others ask to help them in certain matters, because they believe that he, more than themselves, has the knack of it. As the occult arts become complicated and dangerous the superiority of the master-mind is more manifest. We are told that amongst the Tasmanians there were no professional wizards or medicine-men, but that some people practised more than others.

[1] Throughout this article, only the practices of men are discussed; but the part of women in the occult arts has been general and persistent, no doubt presents a special mentality, and deserves separate treatment.

From the beginning the art excites wonder, and wonder credulity; and an old fellow, who was subject to fits of contraction in the muscles of one breast, used this mysterious affection to impose upon his neighbours[1]. Wonder and the deference it brings with it, with the self-delusion of power it generates, are at first the wizard's sole recompense; and to the end they remain his chief recompense. In Australia a wizard is initiated (in fact or by repute), and is in some ways a man apart from others; yet in several cases it is reported that he receives no fees. For magical services amongst the Arunta "no reward of any kind is given or expected[2]." Generally, the wizard earns his living like other men, and merely supplements it by fees and presents. He rarely attains the professional dignity of living solely by his art and mystery.

Nevertheless in simple societies the wizard is a leader or a chief. The predominance of old men in council depends upon their occult powers rather than upon their worldly wisdom: even hereditary chiefs may have greater prestige through magic than through royal descent. But the rise and spread of the social and political power of wizards has been fully illustrated by Sir J. G. Frazer in the sixth chapter of *The Magic Art*. I conceive that after the organisation of the primitive hunting-pack had, by various causes, been weakened or destroyed, it was through belief in magic that some sort of leadership and subordination were re-established: perhaps in many experimental social forms, of which some specimens may be found in Australia and survivals of others in all parts of the world.

As animistic interpretation prevails in any society, so that the marvels of magic come to be attributed to spiritual causes, magicians tend to become sorcerers; and, being associated with spirits, they may not be easily distinguishable from priests: but we may say, generally, that an officiant is a sorcerer, so far as he depends upon coercing spirits by magic; a priest, so far as he relies upon propitiating them by prayer and sacrifice. The sorcerer is aggressive and domineering toward supernatural powers; the priest professes humility. Conflict arises between them; and with the development of ancestor-worship and its derivatives, the priest, who is often the chief or one of the royal family, conducting the worship of gods who are his own ancestors, absorbs into their rites the socially beneficent practices of wizardry, or white magic, outlaws the wizard, and leaves him to the secret performance of black magic in dark and unwholesome circumstances. Persecuted with hatred and derision

[1] Ling Roth, *The Aborigines of Tasmania*, 65.

[2] Spencer and Gillen, *Across Australia*, 336.

not unmixed with fear, his line persists even to our own day; the wizard still has countless followers, more or less ashamed of themselves, and so discreditable to him, that those who have not studied his earlier history may wonder what interest he can have for us, or why we should make any inquiry into "what he is pleased to call his mind."

Among backward peoples, wizards, being a marked class, and well aware of it, are naturally drawn into mutual understanding, and often form clubs or secret societies; not apparently in Australia, but certainly in Melanesia, Africa and America. Every profession, organised or unorganised, provided there be an understanding amongst its members, is prone to acquire anti-social interests and to establish a secret tradition; and the antagonism of a profession to the public may be a virulent evil; of which wizardry offers the first example. The profession and its tradition begin with practices common to all members of a tribe; and the tradition grows by accumulating the discoveries and inventions of the professionals. The cumulative tradition becomes more voluminous, the spells more intricate, the rites more elaborate, because the possible membership of the profession is thereby narrowed, the self-valuation of the initiated is heightened, the wonder and credulity of the laity is enhanced; so much of the doctrine and discipline being allowed to transpire as may make the last effect a maximum. Whilst tribal belief in magic is the necessary ground of the wizard's existence, he—being once recognised—thenceforth confirms, sways and guides the tribal belief.

Where wizardry has become a comprehensive art controlling every province of nature, its practitioners may become specialists—"departmental experts," as Prof. Seligman calls them. On the lower Congo Mr Weeks says there are fifty branches of the profession. Amongst them they cause rain or drought, favourable or adverse winds; they prosper the crops or blight them, control the game of the hunter and the cattle of the herdsman. They cause and cure love and other forms of sickness; slay, and recall the soul to its accustomed habitation. They have the power of metamorphosis. To fly is one of the most persistent claims of the profession: it is common in Australia and in Greenland; European witches flew upon broomsticks (equivalent of Siberian horse-staves); Dr Faustus upon his magical cloak; and 'levitation' has been exhibited by recent 'mediums.' They also discover thieves and murderers, preside at births and marriages, administer the ordeal, interpret dreams, prophesy by the flight of birds, the fall of dice, the posture of the stars and so forth.

The sorcerer, communicating with the ghosts of the dead or other spirits, accomplishes with their aid whatever can be done by magic. He is possessed by them and operates or speaks by their power or inspiration; he drives them out of others whom they possess, or sends them on errands; controlling them either by aid of other spirits or by his profound knowledge of enchantments.

Good observers assure us that the wizard often believes in his own pretensions, and that they are accepted as genuine by the majority of his tribesmen. For them his performances are so wonderful as to put to shame the achievements of scientific invention; and this may explain the frequent reports that savages are deficient in wonder. "It takes a good deal to astonish a savage. He is brought up on magic, and things that strike us with astonishment he regards as simply the exhibition of magic of greater power than any possessed by himself[1]."

2. Characteristics of the Wizard.

(1) Observers generally agree that the wizard, or medicine-man, is distinguished in his tribe for intelligence and penetration, or at least for cunning. He is apt to impress an unsympathetic witness as "some fellow with more brains and less industry than his fellows." Amongst the Bakongo, the witch-doctors, according to Mr Weeks, have sharp eyes, acute knowledge of human nature, and tact[2]. The Samoyed shamans "are, as a rule, the most intelligent and cunning of the whole race[3]." Cunning is plainly necessary to the wizard's life, and, for some of his functions, much more than cunning. In many tribes his advice is asked in every difficulty and upon every undertaking. Sir E. im Thurn reports that the *peaiman* of the Arawaks learns and hands down the traditions of his tribe and is the depository of its medical and hunting lore[4]. The surgical skill of Cherokee medicine-men in the treatment of wounds was considerable[5]. Still greater seems to have been that of the Fijians, with no mean knowledge of anatomy learnt during their incessant wars[6]. Medicine-men of the Amerinds discovered the virtues of coca, jalap, sarsaparilla, chinchona, and guiacum[7], implying on their part superior curiosity and observation. The Bantu doctors of S. Africa employ aloes,

[1] Spencer and Gillen, *Across Australia*, 51.

[2] *The Primitive Bakongo*, 216.

[3] *J. A. I.* xxiv. "Shamanism," 144.

[4] *Among the Indians of Guiana*, 335.

[5] *Am. B. of Ethn.* "Sacred Formulas of the Cherokees," by J. Mooney, 323.

[6] W. Mariner's *Account of the Tonga Islands*, c. xxi.

[7] *Am. B. of Ethn.* ix. "Medicine Men of the Apache," by J. E. Bourke, 471.

nux vomica, castor-oil, fern, rhubarb and other drugs[1]. In all parts of the world some knowledge of drugs and of certain methods of treatment, such as sweat-baths and massage, ligatures, cauterisation and fomentation, seems to have been possessed by the magical profession. As weather-doctors and crop-guardians, they laid out the first rudiments of astronomy and meteorology. All their knowledge of this sort is, of course, no magic, but experience and common sense; though science is not derived from magic, the scientist does descend from the magician: not, however, in so far as the magician operates by magic, but in so far as he operates by common sense. Among the Lushai tribes the name for sorcerer, *puithiam*, means 'great knower[2],' the equivalent of our 'wizard.' But sorcery degrades the magic art of medicine by discouraging with its theory of 'possession' every impulse of rational curiosity, and by substituting for empirical treatment (however crude) its rites of exorcism and propitiation.

In parts of the world so widely separated as Siberia, Greenland and the remote back-woods of Brazil, wizards have discovered the secret of ventriloquism. Everywhere they have learned the art of conjuring, without which (especially the trick of 'palming') many of their performances, and notably the sucking-cure, could not be accomplished. Their practice is generally clumsy and easily detected by sophisticated whites, but imposes upon their patients and the native bystanders[3]. In India it is carried to a much higher degree of illusion. Wizards often have a practical knowledge of some obscure regions of psychology; such as the force of suggestion and various means of conveying it, and the effect of continuous rhythmic movements and noises in inducing a state of exaltation or of dissociation.

A wizard's tribesmen, of course, believe him to possess knowledge absurdly in excess of the reality. He boasts of it as the foundation of his power over nature or over spirits; often as a supernatural gift of spirits whom he has visited, or who have visited him, and who have initiated him; or else as secret traditionary lore. It is by knowledge of human nature that he rules his fellows; and he asserts that knowledge of the names and origins of things and of spirits gives him the same control over them. In Mr Skeat's *Malay Magic*, incantations addressed by a miner to spirits or to metals, adjuring the spirits to withdraw from his 'claim,'

[1] Dudley Kidd, *The Essential Kafir*, 135.

[2] *The Lushai Kuki Clans*, by J. Shakespeare, 80.

[3] Hose and McDougall, *Pagan Tribes of Borneo*, 120, and *Am. B. of Ethn.* XI. 417; XIV. 97, 148.

or the grains of metal to assemble there, contain the intimidating and subduing verse:

I know the origin from which you sprang!

The same compelling power was employed—if we may trust the *Kalavala*—by Finnish wizards; for "every thing or being loses its ability for evil, as soon as someone is found who knows, who proclaims its essence, its origin, its genealogy." "Tietaja, which etymologically signifies wise, or learned, is ordinarily used for magician[1]." It was by profound science that mediaeval magicians were believed to control demons; and anybody celebrated for science was suspected of sorcery: such as Grosseteste, Albertus Magnus, Roger Bacon, Aquinas, and Raymond Lulli; whose reputation supported the credit of such men as Paracelsus and John Dee.

The fine arts in their rudiments owe much to the wizards. Incantations in verse often reach a high pitch of lyric fervour. The words *rune, carmen, laulaa* bear witness to the magic of poetry. Vergil and Taliessin have been famous for more than natural gifts; that one by superstitious repute, and this by his own vaunting[2]. Dancing and pantomime were cultivated for their magical virtues. Primitive carving and painting are in many cases undertaken in order to influence the spirits, or the animals, or the natural powers they represent; and if magic was the motive of the recently discovered animal paintings dating from the old stone age, its efficacy in encouraging art at that remote period rivalled that of religious patronage in some later ages.

(2) In most tribes the wizard needs great force of will and persistency of purpose—whether from deliberate choice or from infatuation with the profession—to carry him through the severe training that is often exacted from candidates for the office; great audacity and courage to impel and sustain him in the practice of his art, pestered by taboos and (the sorcerer especially) always beset by supernatural terrors and often by more real dangers; and unusual presence of mind to extricate himself from very embarrassing situations. It is true that, in some cases, where the office of wizard is hereditary, or may be assumed by alleging the favour of spirits, or some other underhand device—perhaps upon the evidence of visions, or by mere fraud, or by a mixture of both—we hear little of really serious initiatory rites; but often these formalities are very painful or very expensive. Among the Arunta, there are three classes of wizards: the first and second, made by spirits, undergo no severe trial—except the boring of a hole in their own tongues and the keeping of it open,

[1] D. Comparetti, *The Traditional Poetry of the Finns*, 27 and 25.

[2] Rhys, *Celtic Heathendom*, 548–50.

as evidence of a professional story about spirits who slew them with spears, cut out their entrails and replaced them with a new set and certain magic stones. But the third class are initiated by two wizards of the first and second class, who pretend to force crystal stones[1] into their bleeding bodies from the front of the leg up to the breast bone, into the crown of their heads and under the nail of the right forefinger. This they must suffer in perfect silence, three times a day on three consecutive days, with other tortures, followed by various taboos; and, after all they do not stand as well with the tribe as those whom the spirits have initiated[2]. Can there be any doubt that the initiatory rites of the third class represent the older magical custom, and that the fabulous initiation by spirits is an overgrowth of animism? There are clear motives for the change; since the latter method is easily carried out by oneself, is far less painful, and is more stimulating to the imaginative belief of the laity, and therefore more imposing. For the inverse change there are no motives. And, therefore, wizards of the third class are probably really less competent than the others; for the man who, after the new spirit-path has been opened up, still prefers the old road through pain and privation must be (comparatively) an unimaginative, dull, honest, inferior fellow.

Old wizards of the Warramunga, receiving a new candidate for the profession, allow him during the process no rest; he must stand or walk until quite worn out, when he scarcely knows what is happening to him; deprived for a long time of water and food, he becomes dazed and stupefied[3]. In the western islands of Torres Straits, a novice was taken into the bush by his instructor, who defaecated into a shell full of water and made him drink it with his eyes open; next he must chew certain fruits and plants, which made his inside bad and his skin itch; then

[1] The crystals forced into a wizard's body, whether by spirits or by other wizards, are essential to his profession, and if they leave him his power is lost. John Mathew says that, according to the belief of the Kabi (Queensland), "A man's power in the occult art would appear to be proportioned to his vitality, and the degree of vitality which he possessed depended upon the number of sacred pebbles and the quantity of *yurru* (rope) which he carried within him" (*Eagle-hawk and Crow*, 143). 'Rope' was the property of the higher grade of medicine-men (substitute for snakes?), who had obtained it from the Rainbow in exchange for some of their pebbles. Certain pebbles, especially crystals, are independent magic-powers throughout Australasia and elsewhere, probably of much older repute than the profession of wizardry; and the wizard gets his personal power by having them inside him: whereas we are often told that the occult art begins with the extraordinary personality of the wizard.

[2] *Native Tribes of Central Aust.* 522–9.

[3] Spencer and Gillen, *Northern Tribes of Central Aust.* 485.

shark's flesh, and, finally, the decomposing flesh of a dead man full of maggots. He became very ill and half frantic. Few cared to undergo these rites; some gave up the undertaking; some died of it[1]. In British Guiana, an aspirant to wizardry undergoes long fasts, wanders alone in the bush (full of terrors to the timid Indian), and accustoms himself to take large draughts of tobacco-juice mixed with water, which cause temporary insanity[2]. Across the watershed to the S.W., the office of medicine-man is hereditary; yet Waterton reports that probationers have to endure exhausting ordeals and tortures[3]. The severe training of the Bantu witch-doctor kills many novices[4]. Under such conditions, only men of unusual force of will, or constancy of infatuation (qualities not always easy to discriminate), can become wizards. Preparatory ritual for the office of shaman among the Buryats of Siberia is elaborate, expensive and intimidating: a candidate of poor family is helped by the community to get animals for sacrifice and objects necessary for the rites; but many shrink from the trial, "dreading the vast responsibility it brings; for the gods deal severely with those who have undergone consecration, and punish with death any serious mistake." There are nine degrees in the profession, each requiring a special initiation[5]. Thus, in many cases, the ordeal of initiation turns away the weak and incompetent, and keeps up the wizardly profession at a high level of resolution and endurance. In more sophisticated societies a similar result is obtained by the belief that the attainment of magical powers depends upon the undergoing of prolonged austerity in study, or in privations and tortures, which give a mystical right to supernatural power: the superstition upon which Southey raised *The Curse of Kehama*—least unreadable of his romances.

As for the courage that may be requisite for carrying on the wizard's practices, when he is the terror of his neighbours, their attitude toward him varies, in different tribes, from the tamest toleration to murderous antagonism. Thus, in the western islands of Torres Straits, Professor Haddon never knew the sorcerers mobbed or violently put to death on account of their magical practices[6]. In New Caledonia, when a sorcerer causes a general famine, the people merely make him presents to procure

[1] Haddon, *Camb. Exp. to Torres Straits,* v. 321.
[2] E. im Thurn, *The Indians of Guiana,* 334.
[3] Th. Whiffen, *The N.W. Amazons,* 181.
[4] Dudley Kidd, *The Essential Kafir,* 156.
[5] *J. Anthrop. Inst.* XXIV. "Shamanism," 87–90
[6] *Camb. Exp.* v. 322.

a return of plenty[1]. Among the Todas, a man who is the victim of a sorcerer pays him to have the curse removed[2]. In such cases, effrontery is all the sorcerer needs. On the other hand, near Finsch Harbour in New Guinea, a dangerous sorcerer is often put to death; and so he is amongst the neighbouring Kais[3]. Such in fact is the more general practice[4]; and the wizard, carrying on his profession at the risk of his life, must be supported by the sort of fearlessness that criminals often show.

Confronted with supernatural dangers, the sorcerer's need of courage must depend upon the sincerity of his own belief in them: a matter to be discussed in the fourth section of this article. If his professions are veracious, the attitude of such a man toward spiritual powers cannot be sustained by any ordinary daring. In the N.W. Amazons the shaman is the only one of his tribe who dares go alone into the haunted forest. Everywhere the sorcerer fights the demons of disease with reckless valour. On the Congo he drives them into some animal, and then cuts its head off. In North America he intimidates, quells and exorcises them with furious boasting. In Siberia, to capture the fleeting soul of a patient, he follows it over land and sea and into the regions of the dead. The Innuit of Greenland acknowledge Sedna as the supreme Being and the creatress of all living things; yet their angakoq subdue even her: one lures her from Adlivun with a magic song; whilst another, as she emerges, harpoons her with a seal-spear, which is then found to be smeared with blood[5]. To obtain assistance from even the highest spirits the wizard deceives them; or to slay an enemy he usurps their powers. The Malay avenges himself by making an image of his victim in a shroud, and praying over it as over the dead; then he buries it in the path to his victim's house, and says:

Peace be to you, ho prophet Tap, in whose charge the earth is!
Lo I am burying the corpse of (name of victim)
I am bidden by the prophet Mohammed,
Because he (the victim) was a rebel against God.
Do you assist in killing him.

[1] J. G. Frazer, *Belief in Immortality*, 334.

[2] W. H. R. Rivers, *The Todas*, 256–7.

[3] *Belief in Immortality*, 249, 269.

[4] For examples see Weeks, *The Primitive Bakongo*, 204; Dudley Kidd, *The Essential Kafir*, 149; Hose and McDougall, *The Pagan Tribes of Borneo*, II. 115; *Shamanism*, 130; Carl Lumholtz, *New Trails in Mexico*, 24; T. A. Joyce, *South American Archaeology*, 245; E. Westermark, *Origin and Dev. of Moral Ideas*, II. 650–2.

[5] Franz Boas, "The Central Esquimo," *Am. B. of Ethn.* VI. (1884–5), 603.

If you do not kill him,
You shall be a rebel against God,
A rebel against Mohammed.
It is not I who am burying him.
It is Gabriel who is burying him.
Do you grant my prayer this day:
Grant it by the grace of my petition within the fold of the creed La ilaha, etc.[1]

One might suppose that the audacity of blaspheming could rise no higher than this; but an Egyptian woman, when in labour, was taught to declare herself to be Isis, and to summon the gods to her help. "Should they refuse to come, 'Then shall ye be destroyed ye nine gods; the heaven shall no longer exist, the earth shall no longer exist, the five days over and above the year shall cease to be; offerings shall no more be made to the gods, the lords of Heliopolis, etc.[2]'" If the facts were not before us, it would be incredible that a fixed purpose of obtaining supernatural aid should thus exclude from the mind all thoughts of the divine attributes and of one's own insignificance.

(3) What motives impel a man to adopt this strange and hazardous profession, or sustain him amidst all the dangers and disappointments of exercising it? In the first place, some men are oppressed by a vocation toward wizardry; just as amongst ourselves some men have an irresistible vocation to be poets, though that way poverty stares them in the face. To ordinary people these seem to be cases for the asylum. Yet we may understand the "votary of the Muses" by considering that the poet, as the master of rhythm, the treasurer of tradition, the arbiter of fame, has had a necessary place in the ancient culture of the tribes, and greatest in the noblest tribes. A tribe that produces poets has an advantage in the struggle of life; and, accordingly, a strain of poet-blood is bred in the tribe, and shows itself in a certain number of youths in each generation. I think the same must be true of wizards. They are often 'called'; the Altaians believe that no man of his free will becomes a shaman[3]. Like poets, they are sacred and possessed. They are also very useful: their functions in several ways overlap those of the poet, as in cherishing traditions; and often they themselves are poets. They give confidence to their fellows amidst the awful imaginary dangers of savage life. Their teaching is sometimes favourable to sanitation; they sometimes detect thieves and compose domestic quarrels; they make some discoveries and remember them. Their nervous temperament may raise the vital level

[1] W. W. Skeat, *Malay Magic*, 571.
[2] A. Wiedemann, *The Religion of the Egyptians*, 273–4.
[3] A. M. Czaplicka, *Aboriginal Siberia*, 178.

of a tribe. They keep alive the beliefs in taboo and the like mysterious dangers, on which savage order and morality depend; and in many cases they become leaders and chieftains. In this way, belief in Magic and Animism seems to have been the necessary scaffolding of social life[1]. And were this all, the utility of wizards would be clear. But they often do so much and such horrible mischief, prohibiting every improvement and spreading general terror, that it is difficult to judge, in such cases, whether their activities leave a balance of good or of evil. Perhaps sometimes the evil may exceed, and a tribe may degenerate and perish of it. On the whole, however, there is certainly a balance of good, at least in many tribes and at some stages of social development; and this accounts for the flourishing from age to age of the wizardly profession, and for the attraction it has for those of wizardly blood who enter it, because it promises to satisfy an innate disposition. Even in a civilised country, this disposition still, in a few people, manifests itself in the old way; but for the most part has been 'sublimated' into other professions. Of course that which attracts the neophyte of wizardry is not the utility of the profession, any more than the youthful poet is allured by the utility of poetry. That which appeals to the wizard of inbred genius is (besides the indulgence of his personal powers) the mystery of wizardry; which excites in his soul a complex, consisting chiefly of curiosity as to the unknown powers that control nature and spirit, the fascination of fear in approaching them and an exaltation of self-consciousness at the prospect of attaining superhuman wisdom and authority. The article on Shamanism[2], which I have cited so often, describes the shaman as sometimes profoundly convinced that he was chosen for the service of the spirits; and says that some feel a compulsive vocation, and endure persecution for their faith; they cannot help shamanising. One is mentioned as having been gifted with a sensitive nature and an ardent imagination: he had a strong belief in the spirits and in his own mysterious intercourse with them.

Men of such a temperament, I take it, distinguishing themselves above others when every man practised magic or sorcery, founded the profession, and are always its vital nucleus, though in time they may become but a small proportion of its members. Under sincere infatuation they established its observances—the fastings, sufferings, austerities, visions and frenzies of initiation, whether into magical knowledge or spiritual possession; the working of themselves up, whilst officiating, into the orgiastic intoxication in which they felt their own greatness and

[1] J. G. Frazer, *Psyche's Task*.

[2] Pp. 138–9.

dominated their audience; and they discovered some of the modes of operating by drugs and suggestion and some real remedies. But the profession, once formed, soon had attractions for a very different sort of man, impelled by very different motives; who saw in it the road sometimes to wealth, always to reputation and power. Since, amongst very primitive people there is little wealth to collect, and sometimes (as we have seen) remuneration of magical services is neither given nor expected, the earlier of these motives must have been the love of causing wonder and fear, of the power which their fellows' fears conferred and of the reputation which consequently spread far and wide. At first they seem to have had no privileges, but power acquires privilege: so that, among the Warramunga, they are free from sexual taboos[1]; in Guiana no tribesman dares refuse the sorcerer anything, not even his wife[2]; among the Boloki, the sorcerer is never charged with injurious witchcraft and, therefore, is never in danger of the ordeal[3].

Sometimes, indeed, the wizard may not be respected in private life, but only in the exercise of his office. Fear and wonder, in fact, do not always entirely blind the eyes of neighbours to his shortcomings. Spencer and Gillen tell us that the magic of distant places, being the less known, is the more feared[4]. The Rev. J. H. Weeks even says that on the Congo the village medicine-man is seldom engaged at home; for the people know that his fetich cannot protect him or his from harm and, therefore, hire someone from another village of whom they know less[5]. Similarly, a tribe is apt to fear its own adepts less than those of another tribe of lower culture, whose ways are less known and more mysterious: as Malays fear especially the Jakuns[6]; formerly the Swedes the Finns, and the Finns the Lapps; the Todas the Kurumba[7]: and in Macedonia Mohammedan monks enjoy a far higher reputation for magic than the Christian[8].

Still, the wizard, whether of home or foreign growth, becomes necessary in every crisis of life, at birth and marriage, in misfortune, sickness and death; in every undertaking—hunting, agriculture or commerce; and by his omens and auguries may determine war and peace.

1 Spencer and Gillen, *Northern Tribes of Cent. Aust.*
2 im Thurn, *op. cit.* 339.
3 J. H. Weeks, *Among Congo Cannibals*, 145.
4 *Across Australia*, 350.
5 *Primitive Bakongo*, 285.
6 Skeat, *Pagan Races of the Malay Pen.* I. 563.
7 Rivers, *The Todas*, 263.
8 Abbot, *Macedonian Folk-Lore*, 225.

After his own death, he may sometimes look forward to being deified. With such powers, surrounded by intimidated and dependent crowds, and often enjoying a long career of conscious or unconscious imposture, he seems to himself, as to others, a 'superman'; his *Selbstgefühl* rises to megalomania, and his boasting becomes monstrous and stupefying.

(4) The more to impress the imagination of all spectators and enhance his reputation, the wizard usually affects a costume, or behaviour, or strange companionship of animals, that distinguishes him from the rest of his tribe. Not always; for Miss Czaplicka[1] says that in Siberia the shaman is in everyday life not distinguished from others, except occasionally by a haughty demeanour; but the rule is otherwise. A. W. Howitt[2] relates that one Australian medicine-man obtained influence by always carrying about with him a lace-lizard four feet long; another cherished a tame brown snake; and an old woman kept a native cat (*Daysurus*): "familiars" that correspond to the black cats and goats of our own witches. The Arunta medicine-man bores a hole in his tongue, appears with a broad band of powdered charcoal and fat across the bridge of his nose and learns to look preternaturally solemn, as one possessing knowledge hidden from ordinary men[3]. In the forest of the upper Amazons, the medicine-man does not depilate, though the rest of his tribe do so to distinguish themselves from monkeys; and he attempts to present in his costume something original and striking[4]. In S. Africa, too, the witch-doctor's dress is often very conspicuous. So it is throughout history: the thaumaturgist, by his wand, robes, austerities, demeanour, advertises himself as a man apart from the crowd. Rites of initiation mark this superiority: as tribal initiation separates a man from women and boys, so the wizard's initiation makes him a 'superman.'

(5) Such a temper cannot endure opposition, and is jealous of rivalry; the man whom it actuates lives in constant fear of failure and discredit and is, therefore, full of suspicion and cruelty. The savage sorcerer looks with no kindly eye upon the European; who, too plainly, possesses extraordinary magic. Captain Whiffen says[5] of the sorcerer amongst the South Amerinds, who has much knowledge of poisons, that to maintain his reputation, if he has declared that he cannot cure a patient, he poisons him. Of the sorcerer of the lower Congo, Mr Weeks says, that "his face becomes ugly, repulsive, the canvas on which

[1] *Op. cit.* 203. [2] *Native Tribes of S.E. Australia*, 387–8.

[3] Spencer and Gillen, *Across Australia*, 334–5.

[4] Whiffen, *op. cit.* 182–3.

[5] Whiffen, *op. cit.* 64 and 168.

cruelty, chicanery, hatred and all devilish passions are portrayed with repellant accuracy[1]." An extreme case perhaps; but, leading up to it, there are all degrees of rancour and malignity toward those who hinder our ambitions; and it can only be in exceptional magicians that megalomania is reconcilable with considerateness and magnanimity.

(6) Since the greater part of the wizard's practice is imposture (whether he believes in himself or not), he must be an actor; and his success must greatly depend on the degree in which he possesses the actor's special gifts and temperament: an audience must be a stimulus to him and not a check; he exhibits himself not unwillingly. To the shaman, Miss Czaplicka tells us[2], an audience is useful: though the presence of an European is depressing. A Chuckehee shaman without a sort of chorus considers himself unable to discharge his office: novices in training usually get a brother or sister to respond to their exercises. And, indeed, a wizard's exhibitions often provide without design the same sort of social entertainment as those of an actor. One cannot read of the shaman's performance of a pantomimic journey on horseback to the South, over frozen mountains, burning deserts, and along the bridge of a single hair, stretched across a chasm over foaming whirlpools, to Erlik Kahn's abode, and then home again on the back of a flying goose, without perceiving that in the wilds of Siberia such an entertainment, whatever other virtues it may have, supplies the want of theatres and music-halls, and that in such displays a dramatic profession might originate.

But there are two kinds of actors; and they correspond well enough with the two kinds of wizards already described—the vocational and the exploitative. Some actors are said to identify themselves with their assumed character and situation so profoundly as to substantiate the fiction: concentration of imagination, amounting to dissociation, makes the part they play, whilst it lasts, more real than anything else; and raises them, for that time, in energy of thought, feeling and action, much above their ordinary powers; so that they compel the attention, sympathy and belief of the audience. But others study their part and determine beforehand exactly what tones, gestures, expressions are the most effective reinforcement of every word, and thereafter carry out upon the stage in cold blood the whole dramatic lesson which they have taught themselves. Perhaps more common than either of these extreme types is the actor who begins by studying under a sort of inspiration, and after experimenting for a few nights, repeats what he has found to

[1] *Primitive Bakongo*, 216.

[2] *Aboriginal Siberia*, 230, 240.

answer best. We need not consider those who can do nothing but what they have been taught by others. Well, the wizard by vocation probably behaves like the first kind of actor: enters upon any office to which he may be called in exclusive devotion to his task; works himself into a frenzy, groans, writhes and sweats in the possession of his demon; chants incantations in an archaic tongue; drums and dances by the hour, and falls into speechless trance—according to the professional pattern—all in what may be described as dramatic good faith. By native disposition and by practised self-suggestion he obtains a temporary dissociation. The opposite sort of wizard, exploiting the profession, sees all this, and imitates it with such improvements as he may be able to devise. Of the intervening crowd of charlatans, who mingle self-delusion with deceit in all possible proportions, no definite account can be given.

According to Diderot[1], the greatest actors belong to the second class, to the deliberate and disciplined artists. We may be sure the gods in the gallery, if they understood what was going on under their eyes, would always prefer the inspired performers. Probably these are, in fact, the greatest in their best hours, but less to be depended on, less sure of being always equal to themselves. And the same may be true of the corresponding sorts of wizards. The lucid impostor, at any rate, is less likely to be abashed by unforeseen difficulties and by the awkwardness of failure, less likely to be mobbed and murdered and pegged down in his grave with aspen stakes.

(7) Wizards are very often people who manifest an hysterical or epileptoid diathesis. According to Miss Czaplicka, hysteria (common in Siberia) is at the bottom of the shaman's vocation: but it is not merely a matter of climate; for the Rev. E. T. Bryant writes of the Zulus—"the great majority of diviners being clearly of neurotic type[2]"; and on all sides we obtain from descriptions of wizards the same impression. The word 'shaman' comes not, as usually supposed, from the Sanskrit (*śramana* = work, a religious mendicant), but from *saman*, which is Manchu for 'one who is excited, moved, raised[3].' The effect on the audience of shamanising depends in great measure upon the fits, ecstasy, convulsions introduced at some stage of the performance and attributed

[1] *Paradoxe sur le comédien.* Mr William Archer, some years ago, published *Masks and Faces*, an entertaining and very instructive book, in which he (as a genuine though unprofessional psychologist) discusses this paradox in the light of evidence obtained, by *questionnaire* and otherwise, from actors then living.

[2] *Man*, Sept. 1817, 144.

[3] *Aboriginal Siberia*, 197.

to possession by the spirits. Similar exhibitions have been reported by all observers of wizardry. Professor Otto Stoll of Zurich has described[1] the phenomena as they occur in all countries and all ages; and he attributes them, as well as hallucinations and analgesia (as in the fire-walk), which wizards also have at command, to the power of self-suggestion acquired by practice and training, on the basis (of course) of a natural disposition. Cagliostro declared that he could smell atheists and blasphemers; "the vapour from such throws him into epileptic fits; into which sacred disorder he, like a true juggler, has the art of falling when he likes[2]." The wizard's fits are voluntarily induced; but from the moment the attack takes place the development of its symptoms becomes automatic. Between the fits, however, says Miss Czaplicka, he must be able to master himself, or else he becomes incapable of his profession: nervous and excitable often to the verge of insanity, if he passes that verge he must retire[3]. In short, his ecstasy is the climacterical scene of his dramatic performance, the whole of which must be rendered with the disciplined accuracy of an artist. We are not to think of the shaman as an hysterical patient: if he were, there would be greater, not less, reason to suspect him of deceit. No doubt the training which gives control of the nervous attack protects the subject from its unwholesome consequences; there seems to be no special liability to disease or to a shortening of life. Formerly in Siberia, before the power of the profession was broken by immigrant beliefs and practices, it was necessary that the shaman should be well-developed mentally and physically (as has often been required of priests); and such a constitution is by no means incompatible with an intense histrionic temperament.

A necessary complement to the suggestive devices of a wizard is the suggestibility of his clients. There is such a thing as an assenting or a dissenting disposition—suggestibility or contrasuggestibility; but the latter is, like the former, a tendency to react without reflection, and may be as well controlled by appropriate suggestions. The power of any given suggestion to control the course of a man's thought and action depends upon the resistance (apart from contrasuggestibility) which it meets with in his mind; and this depends upon the extent, quality and integration of his apperceptive masses, and upon the facility with which they come into action. Upon the perceptual plane a savage's mind is well organised, and accordingly his suggestibility is low[4]; but upon the

[1] *Suggestion und Hypnotismus in der Völkerpsychologie.*

[2] Meiners, *Briefe über die Schweiz* (quoted by Carlyle in essay on *Count Cagliostro*).

[3] *Aboriginal Siberia*, 169, 172.

[4] This *Journal*, Oct. 1905, 393.

ideational plane it is in most cases ill organised, poor in analysis, classification, generalisation, poor in knowledge, abounding in imagination-beliefs about magic and animism; so that, except in natural sceptics (who, as we shall see, exist among savages), the suggestions of the wizard meet with little resistance from common sense and with ready acceptance by magical and animistic prejudice. But even with a man of common sense, a suggestion, however absurd, may for a time prevail, if his mental reaction is slow; although, with time for reflection, he will certainly reject it. The art of the wizard consists in getting such hold of his client's attention that, as in hypnosis, the power of reflective comparison is suspended and criticism abolished. There are many masters of this art. The client's state of mind is very common in the effects of oratory, the theatre, ghost-stories and generally in the propagation of opinion, suspicion and prejudice.

3. THE WIZARD AND THE SCEPTIC.

Inasmuch as the wizard's boasting, conjuring, ventriloquising, dramatising and practising of all the arts of suggestion, seem incompatible with sincerity, whilst nevertheless he is devoted to his calling, some observers have declared him a calculating impostor, whilst others maintain that he, in various degrees, believes in himself and shares the delusions which he propagates. Examples may be found in support of either position.

Some say that the wizard believes in himself because all others believe in him; that at a certain level of culture, there is an universal social obsession by certain ideas, from which the individual cannot escape, and for whose consequences, therefore, he is not responsible. According to this theory, a sort of tribal insanity prevails. Dr Mercier says that, in the individual, paranoia is characterised by systematised delusion: "there is an organised body of (false) knowledge, and it differs from other delusions in the fact that it colours the whole life of the patient; it regulates his daily conduct; it provides him with an explanation of all his experiences; it is his theory of the cosmos." And, again, "as long as the highest level of thought is intact, so that we can and do recognise that our mistakes are mistakes and our disorders, whether of mind or conduct, are disorders, so long sanity is unaffected, and our mistakes and disorders are sane. As soon, however, as we become incompetent to make this adjustment...insanity is established[1]." These

[1] *Text-book of Insanity*, 281 and 79.

passages exactly describe the condition of a wizard and his tribe afflicted with social paranoia: their theories of magic and animism and of the wizard's relations with invisible powers, may truly be said to form an organised body of (false) knowledge, to colour all their lives, to explain everything that happens to them; and their mistakes and disorders to be incorrigible by reflection or experience. Such conditions of the social mind have, I believe, existed (and may still exist), tending toward, and sometimes ending in, a tribe's destruction. That, in such a case, there should be unanimity is not necessary; it is enough that the current of belief, in certain directions, be overwhelming. But such extreme cases are rare. Normally there may be found, even in backward tribes, a good deal of incredulity and of what may be called primitive rationalism or positivism.

Considerable sections of a tribe sometimes co-operate in imposture, the men against the women and children, or the old against the young; and it cannot be supposed that, where this occurs, anything like universal delusion prevails. The Arunta, who teach their women and children that Twanyiriki is a spirit living in wild regions, who attends initiations, and that the noise of the bull-roarer is his voice; whilst they reveal to the youths when initiated that the bundle of churinga is the true Twanyiriki[1]; cannot be blind to the existence of social fictions. The discovery by initiated youths in some parts of New Guinea, that Balum, the monster that is believed to swallow them during initiation, is nothing but a bug-a-boo, and that his growl is only the bull-roarer, must be a shock to their credulity[2]. Indeed, disillusionment as to some popular superstition is a common characteristic of initiation ceremonies. On the mainland of New Caledonia, a spirit-night is held every five months; when the people assemble around a cave and call upon the ghosts, supposed to be inside it, to sing; and they do sing—the nasal squeak of old men and women predominating[3]. There is not in such cases any natural growth of social delusion, subduing the individual mind, but prearranged cozenage; and a wizard with such surroundings, instead of being confirmed in the genuineness of his art, only reads there the method of his own imposture in large type.

Home-bred wizards, who are less trusted than those who live further off, or than those of an inferior tribe, do not derive self-confidence from the unanimous approval of their neighbours.

[1] Spencer and Gillen, *Northern Tribes of Central Australia*, 497.

[2] J. G. Frazer, *Belief in Immortality*, 250–60.

[3] G. Turner, *Samoa*, 346.

Again, the general prevalence of delusions in a tribe does not suppress the scepticism of individuals. It is reasonable to expect such scepticism to be most prevalent amongst men of rank, who are comparatively exempt from the oppression of popular sanctions; and probably this is the fact. Such exemption, by preserving a nucleus of relatively sane people, is one of the great social utilities of rank. The Basuto chief, Mokatchané, surrounded by people grossly superstitious, lent himself to their practices; but in paying his diviners he did not hesitate to say "that he regarded them as the biggest impostors in the world[1]." In Fiji, it is doubtful whether the high chiefs believed in the inspiration of the priests, though it suited their policy to appear to do so. There was an understanding between the two orders: one got sacrifices (food), the other good oracles. A chieftain, on receiving an unfavourable oracle, said to the priest: "Who are you? Who is your god? If you make a stir, I will eat you[2]"—not metaphorically. In Tonga, "even seventeen years before the arrival of the first missionaries, the chiefs did not care to conceal their scepticism." In Vavou, Taufaahan, having long been sceptical of his ancestral faith, on learning of Christianity, hanged five idols by the neck, beat the priestess, and burned the spirit-houses[3]. The Vikings, like the Homeric heroes, are said to have fought their gods; and at other times to have declared entire disbelief in them. Hence the easy conversion of the North to Christianity. Scepticism may have been fashionable at court much earlier: "It is scarcely possible to doubt," says Mr Chadwick, "that familiarity, not to say levity, in the treatment of the gods characterised the Heroic Age [350–550 A.D.] just as much as that of the Vikings[4]." The burlesque representations of the gods in some passages of the *Iliad* are a sort of atheism: in astonishing contrast with the sublime piety elsewhere expressed. And the inadequacy of such a literary religion (like that of Valhalla) may explain the facile reception (or revival) of the Mysteries in the sixth century B.C. History abounds with examples of rulers and priests who, in collusion, have used religion for political convenience, in a way that implied their own disbelief and opened unintentionally the doors of disbelief to others.

But it is not only chiefs and heroes whose minds are sometimes emancipated from popular superstitions. An old Australian whose duty it was to watch the bones of a dead man and to keep alight a fire near

[1] Casalis, *My Life in Basuto Land*, 185.

[2] Basil Thompson, *The Fijians*, 158.

[3] Basil Thompson, *Diversions of a Prime Minister*, 201 and 346.

[4] *The Heroic Age*, 413.

them, sold them for some tobacco and a tomahawk, in great fear lest it should be known to his tribesmen; but he "evidently suffered from no qualms of conscience[1]": that is to say, he feared the living, but not the dead. W. Stanbridge says of the aborigines of Victoria that "there are doctors or priests of several vocations; of the rain, of rivers and of human diseases...but there are natives who refuse to become doctors and disbelieve altogether the pretensions of those persons[2]." John Matthew writes of the tribe with which he was best acquainted—"whilst the blacks had a term for ghosts and behind these were departed spirits, ... individual men would tell you upon inquiry that they believed that death was the last of them[3]." Near Cape King William in New Guinea, there is a general belief in spirits and ghosts and also in one Mate; but some whisper that there is no such being[4]. The Bakongo villagers do not believe in all witchcraft; but respect some sorcerers, and regard others with more or less contempt: every man, however, must profess belief, or else "his life will be made wretched by accusations of witchcraft[5]." Before missionaries came amongst the Baloki, many people had no faith in the medicine-men, but would not oppose them for fear of being charged with witchcraft[6]. Capt. Whiffen reports of the tribes of S. Amerinds whom he visited that, "among individuals there are sceptics of every grade[7]." Whilst all Dakotas reverence the great, intangible, mysterious power Takoo-Wahkon, as to particular divinities, any man may worship some and despise others. "One speaks of the medicine-dance with respect; another smiles at the name." The Assiniboin generally believe that good ghosts migrate to the south where game is abundant; whilst the wicked go northward; but some think that death ends all[8]. At Ureparapara (Banks Islands) food is buried with a corpse; and "if there be too much, some is hung above the grave, whence the bolder people take it secretly and eat it[9]." Disbelief is expressed in actions more emphatically than in words; the plundering of tombs has been universal. An idol-maker of Maeva assured Ellis that, "although at times he thought it was all deception and only practised his trade for gain, yet

[1] Spencer and Gillen, *Across Australia*, 374–5.

[2] "Aborigines of Victoria," *Trans. of the Ethn. Soc.* New Series, I. 300.

[3] *Eagle-hawk and Crow*, 146.

[4] J. G. Frazer, *Belief in Immortality*, 239.

[5] J. H. Weeks, *The Primitive Bakongo*, 284 and 285.

[6] J. H. Weeks, *Among Congo Cannibals*, 293.

[7] *The North West Amazons*, 218.

[8] J. O. Dorsay, *Sionan Cults*, 431–2 and 485; *Am. B. of Ethn.* XI.

[9] Codrington, *The Melanesians*, p. 270.

at other times he really thought the gods he himself had made were powerful beings[1]." In Tonga it was orthodox that chiefs and their retainers were immortal, doubtful whether men of the third rank were so, certain that those of the lowest rank, Tooas, were not; their souls died with the body; yet some of these Tooas ventured to think that they too would live again[2]. A sceptical Kayan could hardly believe that men continue to exist after death; for then they would return to visit those they love. "But," he concluded, "who knows?" The traditionary lore of the Kayans answers many deep questions; but the keener intelligences inquire further—"why do the dead become visible only in dreams?" etc.[3] A Tanghul told me, says Mr T. C. Hodson, that no one had ever seen a Lai (Deity): when things happened men said a Lai had done it. "In his view clearly a Lai was a mere hypothesis[4]."

These examples of the occurrence of 'free-thought' in all parts of the world have been taken from my notes without any special search for them: no doubt, a little investigation would discover many more. Everywhere some savages think for themselves; though, like civilised folk, they cannot always venture to avow their conclusions. We must not suppose that belief is as uniform as custom or conventional doctrine: custom and convention hinder thought in dull people, but do not enslave it in the "keener intelligence." Without the enviable advantage of personal intimacy with savages, I have, by reading about them, gained the impression that they enjoy a considerable measure of individuality—as much as the less educated Europeans—and are not mere creatures of a social environment.

The most backward savages have a large stock of common sense concerning the properties of bodies, of wind and water and fire, of plants and animals and human nature; for this is the necessary ground of their life. This common sense has certain characters which are in conflict with their superstitions; the facts known to common sense are regular, proportional, the same for all; not often failing and needing excuses, not extravagant and disconnected, not depending on the presence of some fantastic mountebank. Savages do not draw explicitly the comparisons that make this conflict apparent; but it may be felt without being defined. Some of them, especially, are (as amongst ourselves) naturally inclined to a positive way of thinking; their common sense predominates

1 *Polynesian Researches*, II. 204.

2 John Martin, *W. Mariner's Account of the Tonga Islands*, II. 105 and 137.

3 Hose and McDougall, *The Pagan Tribes of Borneo*, II. 48 and 214.

4 *The Naga Tribes of Manipur*, 126.

over the suggestions of magic and animism, and they, more than others aware of the conflict, become the proto-sceptics or rationalists. Lecky observes that beliefs and changes of belief depend not upon definite arguments, but upon habits of thought. In the seventeenth century a new habit of thought overcame the belief in witchcraft and miracles[1]; and in many other centuries it has everywhere done the same for those in whom the "apperceptive mass" of common sense became more or less clearly and steadily a standard of belief, repelling the apperceptive masses of Magic and Animism with all their contents and alliances. They had more definite ideas than others, little love of the marvellous, little subjection to fear, desire, imagination.

Sometimes the dictates of common sense are imposed by necessity. The Motu (Papuasian) were accustomed to rub spears with ginger to make them fly straight. It had, however, been discovered that no amount of magic would turn a poor spearman into an accurate thrower[2]. Spear-throwing is too serious a concern not to be judged of upon its merits, however interesting the properties of ginger. A whole tribe may in some vital matter, whilst practising a superstitious rite, disregard its significance: like the Kalims, who hold a crop-festival in January, and afterwards take the omens as to what ground shall be cultivated for next harvest; "but this seems a relic of old times, for the circle of cultivation is never broken, let the omens be what they may[3]." That is a tacit triumph of common sense.

The bearing of all this on the character of the wizard is as follows: since everywhere sceptics occur, and some individuals go further than others in openly or secretly rejecting superstitions, why should not the wizard or sorcerer, who is amongst the most intelligent and daring of his tribe, be himself a rationalist and, therefore, a conscious impostor? That much of his art is imposture no one disputes; and so far as it is so, he sees it as part of the ordinary course of experience. The sorcerer on the Congo who drove a spirit into the dark corner of a hut, stabbed it there, and showed the blood upon his spear, having produced the blood (as his son confessed to Mr Weeks) by scratching his own gums, was by that action himself instructed in common sense[4]. He saw in it quite plainly an ordinary course of events, the "routine of experience"; whilst the spectators were mystified. Must he not, then, have more common sense than other people?

[1] Introduction to the *Rise of the Spirit of Rationalism in Europe*.

[2] C. G. Seligman, *Melanesians of B. New Guinea*. 179.

[3] T. C. Hodson, *op. cit.* 171.

[4] *Among Congo Cannibals*, 284.

In fact, he recognises the course of nature and his own impotence, whenever an attempt to conjure would endanger his reputation. The Arunta medicine-men exhibit great dramatic action in curing various diseases; but waste no antics on recognisable senile decay[1]. The medicine-man of Torres Straits admits that he cannot make rain when the wind is N.W.[2] The Polynesian sorcerers confessed their practices harmless to Europeans[3];—who were not suggestible on that plane of ideas. Shamans amongst the Yakuts would not try to cure diarrhoea, small-pox, syphilis, scrofula or leprosy, and would not shamanise in a house where small-pox had been[4]. These miracle-mongers sometimes know a hawk from a handsaw. It seems reasonable, then, to assume that wizards have more common sense than other people; since, besides the instruction of common experience, they know in their professional practice (at least in a superficial way) the real course of events, which is concealed from the laity. And, no doubt, of those whose art is deliberate imposture, this is true. But the infatuation of those who are wizards by vocation may be incorrigible by any kind of evidence: especially as to the genuineness of another's performance. Dr Rivers speaks of the blindness of the man of rude culture to deceitful proceedings on the part of others with which he is familiar in his own actions[5]. For the wizard there are established prejudices, professional and personal interest, fear of trusting his own judgment, supernatural responsibility, desire of superhuman power, sometimes even a passionate desire to alleviate the sufferings of his tribesmen, and everything else that confirms the will to believe.

4. THE WIZARD'S PERSUASION.

Many practices of wizards involve a representation of the course of events as something very different from the reality; and there is no doubt that frequently, or even in most cases, wizards are aware of this, as in performing the sucking cure or visiting the man in the moon. But some anthropologists dislike to hear such practices described as 'fraud,' 'imposture' or 'deceit'; and for certain classes of wizards it seems (as I shall show) unjust to speak in these terms of their profession. Still, taking the profession and its actions on the whole, it is difficult to find in popular language terms more fairly descriptive. We meet here the inconvenience that other social or moral sciences find when they try to

[1] Spencer and Gillen, *Native Tribes of Cent. Aust.* 531–2.

[2] Haddon, *Camb. Exp.* VI. 201.

[3] Ellis, *op. cit.* II. 232.

[4] *Shamanism*, 92.

[5] *History of Melanesian Society*, II. 107.

use common words in a specially restricted sense. In Economics, *e.g.* 'rent,' 'wages,' 'profits' have definite meanings very different from their popular acceptance. In Ethics, what controversy, what confusion in the defining of 'virtue' and 'the good'! In Metaphysics, what is the meaning of 'cause'; what is the meaning of 'god'? The terms must be defined in each system. If then, in these pages, the conduct of a man who, on his way to cure a sufferer of 'stitch in the side,' conceals in his mouth a piece of bone or pebble, and after dancing, and sucking hard enough at the patient's belly, produces that bone or pebble as the cause of pain—is described as 'fraud,' or 'imposture,' or 'deceit,' the words are used to describe the fact only, without any such imputation upon the man's character as they convey in popular usage. Scientifically considered, the man and his circumstances being such as they are, his actions are a necessary consequence; in this limited region of thought moral ideas are irrelevant.

There are some wizards, as it were in minor orders, such as Professor Seligman calls "departmental experts," especially the man who blesses gardens, so earnest and harmless that no one will abuse them. They visit the garden at the owner's request, practise a little hocus-pocus, mutter a few spells, take a small fee and go peaceably home. The owner indeed supposes himself to buy fertility, and obtains only peace of mind—a greater good say the moralists. The wizard has done what he learnt of his father, what respectable neighbours approve of; there is always some crop to justify his ministry, and many an evil power to excuse occasional blight or drought. If he is convinced of being a really indispensable man, it is easily intelligible.

As to the profession in general, a small number of wizards—wizards by vocation—may be strongly persuaded of the genuineness of their art; a much larger body mingles credulity in various proportions with fraud; and not a few are deliberate cheats. Some of the greatest masters of wizardry are dissatisfied with their own colleagues: that eminent shaman Scratching Woman said to Bogoras the traveller, "There are many liars in our calling[1]." On the other hand, the wizard who demonstrated the 'pointing-stick' to Messrs Spencer and Gillen, and having no object upon which to discharge its magic, thought it had entered his own head and thereupon fell ill, was certainly a believer[2]; and so was the wizard who felt that he had lost his power after drinking a cup of tea, because hot drinks were taboo to him[3]. Now a cheat needs no explana-

[1] A. M. Czaplicka, *Aboriginal Siberia*, 180.

[2] *Across Australia*, 326.

[3] Spencer and Gillen, *Northern Tribes of Cent. Aust.* 481.

tion—at least no more on the Yenisei than on the Thames; and the variety of those who mingle credulity with fraud is too great to be dealt with. What chiefly needs to be accounted for is the persuasion of those who—in spite of so many circumstances that seem to make disillusion inevitable—are in some manner true believers—in the manner, that is to say, of imaginative belief, founded on tradition and desire, unlike the perceptual belief of common sense.

(*a*) Men under a vocation to wizardry, of course, begin with full belief in it. Probably they are possessed by the imaginative and histrionic temperament, which we have seen to be favourable to eminence as a wizard. Their vocation consists in the warm sympathy and emulation with which, before their own initiation, they witness the feats of great practitioners, and which generally imply a stirring of their own latent powers; just as many an actor has begun by being 'stage-struck.' A man of such temperament is prone to self-delusion not only as to his own powers, but in other ways. The wizard often begins by fasting and having visions; thereby weakening his apprehension of the difference between fact and phantasy. The fixed idea of his calling begets in his imagination a story of what happened at his initiation, manifestly false, but not a falsehood—a 'rationalisation' according to wizardly ideas of what he can remember of that time. If initiated with torture, which he endures for the sake of his vocation, he is confirmed in it. When called upon to perform, he 'works himself up' by music, dancing and whatever arts he may have learned or discovered, into a state of dissociation, during which his judgment of everything extraneous to his task is suspended; and his dramatic demeanour uninhibited by fear or shame, unembarrassed by any second thoughts, makes, by vivid gestures and contortions and thrilling tones, a profound impression upon patients, clients and witnesses. His performances may derive some original traits from his own genius; but must generally conform to a traditionary pattern and to the consequent expectations of the audience. By practice he acquires—as for their own tasks all artists, poets and actors must—a facility in inducing the state of dissociation (more or less strict), in which work goes smoothly forward under the exclusive dominance of a certain group of ideas and sentiments. Whether, in this condition, he believes in himself and his calling, or not, is a meaningless question. Whilst the orgasm lasts there is no place for comparison or doubt. And since he really produces by his frenzy or possession a great effect upon the spectators—though not always the precise effect of curing a patient or of controlling the weather at which he aimed, we need not wonder if he believes himself capable of much more than he ever accomplishes. The temperament, most favourable to a wizard's success is not likely to be accompanied by a disposition to the positive or sceptical attitude of mind. Moreover, to the enthusiast for whom belief is necessary, it is also necessary to create the evidence. That the end justifies the means is not his explicit maxim, but a matter of course. Gibbon comments on "the vicissitudes of pious fraud and hypocrisy, which may be observed, or at least suspected, in the characters of the most conscientious fanatics[1]." After failures, indeed, or in the languor that follows his transports, there may come many chilling reflections; only, however, to be dispersed by an invincible desire to believe in his own powers and in the profession to which he is committed.

The 'white' wizard may pacify a troubled conscience by reflecting that at any rate he discharges a useful social function; as in fact, he does, so far as he relieves the fears of his tribesmen and gives them confidence. To understand the 'black' wizard, we must turn to the dark side of human nature. That a man should resort to magic or sorcery to avenge himself or his kinsmen, or to gratify his carnality, jealousy or ambition, is intelligible to everybody; but that he should make a profession of assisting others for a fee to betray, injure, or destroy those with whom he has no quarrel, seems almost too unnatural to be credible. Yet it admits of a very easy explanation. The love of injuring and slaying is deeply rooted in us: men afflicted with homicidal neurosis are known to the asylums and to the criminal courts of all civilised nations; assassins on hire have often been notoriously obtainable. To slay in cold blood by violence, or even by poison, seems, however, less revolting than to slay by sorcery and obscene rites. But the fascination of this employment may be further understood by considering the attraction that secret power and the proof of their own cunning has for many people. To slay is sweet; but the ancient hunter depended more upon strategy than upon the frontal attack; and no strategy is so secret or needs so much skill as the Black Art. Finally, if sorcery is persecuted, it excites the contra-suggestibility which in some men becomes a passion capable of supporting them at the stake. Hence the malevolent wizard may feel a vocation, may believe in his own powers; and, of course, he will be confirmed in his belief by the fear excited wherever his reputation spreads. If you put yourself in his place whilst practising some unholy rite, you may become aware that the secrecy, the cunning, the danger, the villainy and the elation of it exert a peculiar fascination, and that this is enhanced by

[1] *Decline and Fall*, ch. XVII.—in discussing the character of Julian.

foul and horrible usages, such as appeal to the perverted appetites of insanity.

(*b*) The deceit employed by a wizard in conjuring, ventriloquising, dressing up, keeping a 'familiar,' choosing favourable opportunities for his *séance* and inciting himself to frenzy, seems incompatible with sincerity; but whilst he knows such proceedings to be artifices, he also knows them to be necessary to the effect which he produces. For that effect, so far as it depends upon wizardly practices, not on such means as massage, or poisons, or drugs, is always subjective—an influence on other men's belief. Are we not demanding of him greater discrimination than he is likely to enjoy, if we expect him to see the hollowness of his profession, because some of the means by which he operates are not what his clients suppose them to be? If it be said that the wizard's stock excuses for failure—that there has been a mistake in the rites, or that another wizard has counteracted his efforts—are those of a man who has anticipated detection in fraud and prepared a way of escape, it may be replied that, according to accepted tenets concerning wizardry, these excuses are reasonable and not necessarily subterfuges.

The professional attitude may induce a man to exonerate himself and his colleagues in certain dubious dealings, for the sake of the public utility of their office on the whole; which is so manifest to him and to them, and is also acknowledged by the public. For a wizard's belief in his art is supported by the testimony of other wizards, in whom he also believes, and by the belief of the tribe generally in the power of the profession, even though he himself be not greatly esteemed. "Who am I," he will ask, "that I should have a conscience of my own?" And if this attitude is inconsistent with keen intelligence and megalomania, such inconsistency is not inconsistent with our experience of human nature.

(*c*) The effects produced by charms, spells and rites, simple or to the last degree elaborate, purely magical or reinforced by spirits, are always subjective, but are believed to have a much wider range. The wizard seems to make the sun rise when he summons it just in time every morning; to cause clouds to gather in the sky when he invokes them just before the rainy season; and so on. Such performances convince others, but seem to us poor evidence for anyone who is in the secret. He is not, however, left without further evidence of three kinds:

(i) From the effects of natural causes that form part of his professional resources. Some of a wizard's practices are really good; *e.g.* in curing the sick he may employ massage, or sweat-baths, or skilful surgery, or medicinal drugs, or suggestion, and thereby succeed without any magic; whilst he is incapable of clearly distinguishing these means from useless rites and incantations. He does not understand intimately why *any* method is efficacious, and therefore cannot understand the limits of his power. His whole art is empirical. Even in modern science explanation always ends sooner or later (and often pretty soon) in pointing to some connection of phenomena which we are obliged to accept as a fact: the wizard is always brought to this pass at the first step. He has no generally acknowledged public standard of what may possibly happen: it is only in the mind of a natural positivist that a standard of common sense grows up by experience without explicit generalisation; and this standard is incommunicable. Hence the wizard is always trying experimentally to extend his power—of course on the model of his traditionary art; always desiring power, and believing that he has obtained, because he desires it.

(ii) The wizard's arts are justified by the action of natural causes set in motion by his clients. He prepares the hunter and his weapons for an expedition; the hunter does his best, and his success swells the reputation of the wizard. Similarly in agriculture and in war. And in all these cases nothing is really due to the wizard, except the greater confidence his clients derive from his ministrations: the hunter's hand is steadier; the sower and the reaper work more cheerfully; and the warrior fights more courageously in the belief that his enemies are surely devoted to the infernal gods. But the wizard, with general acquiescence, claims far more than this, and rises in his own esteem as well as in the esteem of his tribe.

(iii) The persevering wizard is often aided by coincidences. An Australian squatter at Morton's Plains, after a drought, promised a native rain-maker half a bullock, a bag of flour and some tea, if he would fill his new tank for him before the morrow night. The rain-maker set his rites and spells to work, filled the tank and got the reward[1]. Whilst Messrs Spencer and Gillen were with the Urabunna tribe, "the leading rain-maker performed a ceremony and within two days there was a downpour—possibly connected with the fact that it was the usual time for rain to fall in that part of the country." The reputation of the rain-maker was firmly established and, no doubt, his self-confidence. The Australian wizard already mentioned, who thought that his magic had entered his own head, claimed to have driven away a comet by means of his magic stones. Certainly the comet disappeared, and what other cause could anyone point out? "At one time the gusts were very un-

[1] Howitt, *Native Tribes of S.E. Aust.* 398.

pleasant and one of the men told a wind-man to make it stop. Accordingly he shouted out to the wind, and in a minute there was a lull; and no one doubted that this was due to the power of the wind-man[1]." Many similar cases might be given[2]. Striking coincidences are not very rare: there is an illogical prejudice that they are rare because they excite wonder. Moreover, as I have observed, so remote a resemblance to the event shamanised or prophesied may be regarded as a fulfilment, that fulfilment is not uncommon[3]. In the interpretation of omens, where there is only the alternative of good or ill success, half the guesses must be right; and by the glozing of doubtful cases, more than half will seem to be right. And as for the effect of coincidence, Mr Basil Thompson says, "Tongans never admit coincidences[4]"; that is to say, in our sense of the word: for them, in what we call coincidence, there must be causation; and this seems to be generally true of the untutored mind. Among the Churaches, the profession of shaman is generally hereditary; but a man may become a shaman against his will. It is enough "to make a lucky guess as to the issue of some event, and people flock to him for advice from all parts[5]." How many failures are necessary to discredit one lucky guess?

These three kinds of evidence in favour of the wizard's power—natural causes set in motion by himself, natural causes introduced by his clients, and sheer favourable coincidences—have so much the air of perceptual proof, or of an appeal to common sense, that the savage positivist who is able to resist them must have a more solid judgment than most of our educated civilised people. Judgment is an innate individual character, on which education has little effect, except in the special department of a man's training. A scientific expert, for example, may be an excellent judge of evidence in his own pursuit, and elsewhere quite helpless; for where he is strong it is not a set of rules but the mass of his special experience that guides him. That out of the mass of general experience, so disorderly and fragmentary as it is, some minds should have the power of extracting common sense, in spite of the misrepresentations of magic and animism, is very remarkable, and very fortunate for the rest of us.

[1] *Across Australia*, 14, 326, 366.

[2] See Haddon, *Camb. Exp. to Torres Straits*, VI. 210; Stefánsson, *My Life with the Esquimo*, 88; Murdoch, "Ethnology of Port Barrow," in *Am. B. of Ethn.* IX. 430; Frazer, *Psyche's Task*, 55; Risley, *The People of India*, 77; Langloh Parker, *The Euahlayi Tribe*, 48, 49, 82, 90; E. Casalis, *Les Bassoutos* (2 ed.), 302, 3.

[3] This *Journal*, VII. 184.

[4] *Diversions of a Prime Minister*, 245.

[5] "Shamanism," *J. Anthrop. Inst.* XXIV. 154.

That the three kinds of evidence which serve so well to confirm belief in wizardry are all of them—as to the connection between a wizard's rites or spells and the event—entirely coincidental, gives some support to the hypothesis that coincidences are the foundation of the belief in Magic.

Bearing in mind, then, that a wizard's practices sometimes include real causes, and that his shallow knowledge disables him for discriminating between causation and hocus-pocus; that magic or sorcery is generally believed in by those about him, who seek his aid, or the aid of others of his class; that both they and he earnestly desire that his pretensions should be well founded; that he produces striking subjective effects, that in such cases his artifices are a condition of his success, and that a good many coincidences, complete or partial, seem to prove that his art has further extraordinary influence upon men and nature—it is no wonder that some wizards are deluded along with their dupes, especially neurotic enthusiasts or men of an imaginative and histrionic temperament.

To explain the possibility of some men being sincere in witchcraft is not to palliate the profession or the practices of witchcraft. For the most part those practices are deceitful. They are not the invention of savage society; society invents nothing; only the individual invents. They are the invention and tradition of wizards, who keep the secret so far as it is to their advantage: of wizards growing more and more professional, and trading upon the fears and hopes, the anxiety and credulity of their fellows. The spirit of superstition is common to the tribe, but its professional exploitation is the work of those who profit by it.

Observing with satisfaction that, even amongst savages, the positive mind can sometimes free itself from popular superstitions and penetrate the disguise of mystery-mongers, one asks why Nature could not produce whole tribes of men so minded, and spare the folly and horror and iniquity which take up so much space in the retrospect of human life. Perhaps because common sense is only related to actual experience, and could not appreciate the necessity of government and social co-ordination as the condition of all improvement in human life, until it already existed. Such co-ordination had, therefore, to grow up without being understood; and it did grow up under the protection of certain beliefs that induced the tribes of men to hold together and subordinate themselves to leaders: amongst which beliefs the superstitions exploited by wizards had no small part.

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THE THEORY OF SYMBOLISM[1].

By ERNEST JONES.

I. Introduction.

My attention was primarily directed to this subject, to the desirability of attaining a fuller understanding of the theoretical nature of symbolism, through observing that it is the interpreting of symbols that calls forth the greatest 'resistance' with patients in psycho-analytic work, and, further, that this is also the centre of the strongest opposition to psycho-analysis in general. This fact is really more curious than it might appear, since the meaning of the symbols in question is the part of psycho-analysis that is most independent of individual psycho-analysts; it stands outside psycho-analysis, being a body of knowledge that is familiar ground in many other branches of science, *e.g.* anthropology, folk-lore, philology, and so on.

On going into the subject deeply, however, its interest and importance rapidly widen, more and more problems open out, and at last, especially if the word symbolism is taken in its widest sense, the subject is seen to comprise almost the whole development of civilisation. For what is this development but a never-ending series of evolutionary substitutions, a ceaseless replacement of one idea, interest, capacity, or tendency by another? The progress of the human mind, considered genetically, is seen to consist, not—as is commonly thought—merely of a number of accretions added from without, but of the following two processes: on the one hand the extension or transference of interest and understanding from earlier, simpler and more primitive ideas, etc., to more difficult and

[1] Amplified from a paper read before the British Psychological Society, January 29th, 1916.

complex ones, which in a certain sense are a continuation of and symbolise the former, and on the other hand the constant unmasking of previous symbolisms, the recognition that these, though previously thought to be literally true, were really only aspects or representations of the truth, the only ones of which our minds were, for either affective or intellectual reasons, at the time capable. One has only to reflect on the development of religion or science, for example, to perceive the accuracy of this description.

It is evidently necessary, therefore, that we try to understand more of the nature of symbolism, and of the way in which it operates. Our effort is met at the outset by this difficulty. The term symbolism has been used to denote very many different things, some of them quite unconnected with one another, and all of them in need of differentiation. Those interested in the various uses of the word may be referred to the historical work of Schlesinger[1], who has collected some hundreds of different meanings and definitions. Etymology is no guide here, for the earliest meaning of the Greek *σύμβολον* does not seem to be the present-day one of a sign, but a bringing or weaving together, an implication which can perhaps be traced in the fact that most symbols have many significations; the root of the word, Sanscrit *gal*, Indogermanic *bal*, referred especially to the flowing together of water.

The word 'symbolism' is currently used both in a wide sense, roughly equivalent to 'sign,' and in a strict sense, as in psycho-analysis, which will be defined later. The following examples illustrate the variety of phenomena included in the former category. It is applied in the first place to objects, such as emblems, amulets, devices, tokens, marks, badges, talismans, trophies, charms, phylacteries. Then it is used to indicate various figures of speech and modes of thought, such as the simile, metaphor, apologue, metonymy, synecdoche, allegory, parable, all of which are of course differentiated by philologists. Mythological, artistic, magical, religious, and mystical fields of thought, as well as that of primitive metaphysics and science, are often called symbolic. There is a symbolism of cubism, of the Catholic Church, of freemasonry, a colour symbolism, and even a symbolic logic. The word is further used to denote various signs, passwords, and customs. Bowing, for instance, is said to symbolise the ancient custom of prostration, and hence respect with absence of hostile intent. Fifty years ago to wear a red shirt or blouse would have been said to symbolise the fact that the wearer sympathised with Garibaldi. The Venetian ceremony in which the Doge

[1] Schlesinger, *Geschichte des Symbols*, 1912.

wedded the Adriatic with a ring symbolised the naval power of Venice. In Frankish law the seller of a plot of ground handed the buyer a single stone from it as a symbol of the transaction, and in ancient Bavarian law a twig was similarly used in the sale of a forest. When, in 1469, Louis XI dispossessed his brother of Normandy he publicly broke the ducal ring; the act symbolised the complete destruction of his brother's authority. Similar examples of the use of the word could be multiplied endlessly.

Now amid this maze of meanings what attributes in common can be found between the various ideas and acts denoted by the word symbol or symbolic? The following ones seem, if not absolutely essential, at least very characteristic, and from them we may advance to a more precise definition of the problem.

1. A symbol is a representative or substitute of some other idea, from which in the context it derives a secondary significance not inherent in itself. It is important to note that the flow of significance is from the primary idea to the secondary, to the symbol, so that typically a more essential idea is symbolised by a less essential. Thus all sorts of important things may be represented by a shred of material called a flag.

2. It represents the primary element through having something in common with it. Thus it would be a stretch of language to call a mnemonic knot in a handkerchief a symbol of the idea that has to be remembered, although some writers do so[1]. The association may be an internal or an external one. An association, however, which is superficial to the reason may often be of significance in feeling, especially in the unconscious.

3. A symbol is characteristically sensorial and concrete, whereas the idea symbolised may be a relatively abstract and complex one. The symbol thus tends to be shorter and more condensed than the idea represented. The explanation of bowing, given above, well illustrates this.

4. Symbolic modes of thought are the more primitive, both ontogenetically and phylogenetically, and represent a reversion to some simpler and earlier stage of mental development. They are therefore commoner in conditions that favour such a reversion, for example in fatigue, drowsiness, bodily illness, neurosis and insanity, and, above all, in dreams, where conscious mental life is reduced almost to a minimum. Thus a tired man usually prefers looking at an illustrated paper, to reading.

[1] *e.g.* Ferrero, *Les lois psychologiques de symbolisme*, 1895, 25 *et seq.*

5. In most uses of the word a symbol is a manifest expression for an idea that is more or less hidden, secret, or kept in reserve. Most typically of all, the person employing the symbol is not even conscious of what it actually represents.

6. Symbols, particularly those strictly so-called, resemble wit in being made spontaneously, automatically, and, in the broad sense of the word, unconsciously[1].

In accord with the last two attributes is the attitude of the conscious mind towards the interpretation of the symbol, in regard to both comprehension and feeling. Namely, the wider and more diluted the sense in which the word symbol is used the more easily is its meaning perceived and the more readily is the interpretation accepted. With (what I shall later term) a true symbol, on the contrary, the individual has no notion of its meaning, and rejects, often with repugnance, the interpretation.

By the enumeration of these six attributes we have narrowed and defined the field somewhat, but they still apply to many different mental processes, in fact to most forms of indirect pictorial representation. The thesis will here be maintained that true symbolism, in the strict sense, is to be distinguished from other forms of indirect representation, and that not merely as a matter of convenience because it is different from the rest, but because the clear conception thus gained of the nature of the differences must prove of value in understanding the most primitive levels in mental development and their relation to conscious thought. Before doing so, and before seeking to define the distinguishing characteristics of true symbolism, it will be profitable briefly to examine a purely linguistic question, namely the metaphorical use of words[2], for the metaphor is certainly one of the most familiar processes that has to be distinguished from symbolism.

The simile is the simplest figure of speech; it logically antedates even the metaphor, and certainly the adjective. In some primitive languages, *e.g.* Tasmanian, there are no adjectives, similes being used in their stead, the reason no doubt being that it is easier to observe a concrete object which can be used in comparison than to abstract the notion of an attribute. The metaphor differs essentially from a simile in the suppression of one of the terms of comparison; in it the words 'as' or 'like' are suppressed, though always implied. Our motive in employing a simile is to add ornament, force, or vividness to the phrase, but it is to

[1] See Ferrero, *op. cit.* 24.

[2] Cp. E. B. Maye, Art. on "Enlargement of Vocabulary" in O'Neill's *Guide to the English Language*, 1915.

be supposed that the original motive, as in Tasmania, was to indicate the presence of an attribute by the simple process of comparison. The dream makes frequent use of this latter device, which is, in fact, its usual way of indicating an attribute; often quite a complicated description of a person can be conveyed by identifying, *i.e.*, comparing, him with someone else. This dream mechanism of identification has points of contact with the metaphor also. Thus if a person's conduct or appearance resembles in some way that of a lion or bull he may masquerade in a dream in the form of the animal, just as in speech we use such expressions as "he was a lion in the fight."

In the evolution, or what philologists call the decay, of the metaphor there are three stages. In the first a word that is most often used in its literal sense is occasionally used in a figurative one, where its metaphorical nature is at once obvious; an example would be "the wrath of the gale." In the second stage, both the literal and figurative senses are familiar, *e.g.* "the depth of despair"; here we are less conscious of the metaphor. In the third stage the figurative sense has become the usual, literal one, and the original one is forgotten; 'melancholy' no longer suggests black bile nor 'acuity of mind' a cutting edge. Here the decay of the metaphor is complete, and the figurative 'symbol' has acquired an objective reality of its own in place of the subjective one of the earlier stages.

The nature of metaphor will be discussed below in connection with the distinction between it and true symbolism. But consideration of the evolution of the metaphor, as just indicated, already teaches us amongst other things that the simile is the primary process, there being sufficient likeness between two ideas for them to be treated as at least in some respect equivalent. We note, further, the gradual transference of significance from one use of a word to another, ending in the independence of the original metaphor, which has acquired a reality of its own. This process is no doubt parallel to the gradual extension and evolution of the ideas themselves that are denoted by the words.

About the motives for metaphor-making more will be said presently, but a few remarks may be made at this point. A prominent motive seems to be to heighten appreciation on the hearer's part by calling to his mind another image more *easily* apprehended or comprehended, usually one more familiar in respect of the attribute implied (though by no means necessarily in other respects); or, obversely, to eke out the relative paucity of attributive description. The difficulty which the metaphor thus eases may be of either intellectual or affective origin.

II. True Symbolism.

What I here propose to call true symbolism is one variety of the group of indirect representation to which six attributes were attached above. Accordingly it will also possess, besides these, other attributes distinguishing it from the rest of the group. Before defining these I wish to prepare the reader's mind by remarking that an important characteristic of true symbolism is that the interpretation of the symbol usually evokes a reaction of surprise, incredulity, and repugnance on the part of those unfamiliar with it. An example that well illustrates these features is the interpretation of the familiar Punchinello of the marionette stage as a phallic symbol, on which something may be added by way of explanation.

The conception of the male organ as a mannikin is extremely widespread and, by the process known to mythologists as 'decomposition[1],' it often becomes personified and incorporated in an independent figure. A large number of the dwarfs and gnomes so common in folk-lore and legend are of this nature[2], their characteristic attributes being that they are deformed, ugly caricatures of men, wicked and even malign—yet sometimes willing to be friendly and to yield services on certain conditions, able to perform wonderful and magical feats, and winning their own way in spite of their obvious disadvantages. Sand's description of Punchinello is in these respects typical[3]: "il a le cœur aussi sec que son bâton, c'est un égoiste dans toute l'acception du mot. Sous une apparente belle humeur, c'est un être féroce; il fait le mal pour le plaisir de le faire. Se souciant de la vie d'un homme comme de celle d'une puce, il aime et cherche des querelles....Il ne craint ni Dieu ni diable, lui qui a vu passer, sous son nez crochu et verruqueux, tant de sociétés et de religions... (speaking of his passion for women) malgré ses bosses et sa figure peu faite pour séduire, il est si caustique, si persuasif, si entreprenant et si insolent, qu'il a des succès." Nodier fittingly apostrophises him "O Polichinelle, simulacre animé de l'homme naturel abandonné à ses instincts[4]." His physical characteristics well accord with this interpretation; the long hooked nose, long chin, projecting hump on his back, prominent stomach, and pointed cap. The prototype of all modern polichinellos is the Neapolitan *polecenella*, who cannot be traced further

[1] See Ernest Jones, *American Journal of Psychology*, XXI. 105–106.

[2] See, *e.g.* Freud's analysis of "Rumpelstilzchen," *Internat. Zeitschr. f. ärztl. Psychoanalyse*, I. 148.

[3] Maurice Sand, *Masques et Bouffons*, 1860, I. 124.

[4] Nodier, quoted by Sand, *op. cit.* 147.

back than the Renaissance. It is highly probable, however, that he is a lineal descendant of the Maccus of the Roman atellanes (introduced in the sixth century), for the statue of Maccus in the Capponi museum at Rome shews the closest resemblance to the modern figure.

The attribute of comicality attaching to such figures is of considerable interest in more than one direction. The idea of the male organ as a comic mannikin, a 'funny little man,' is a very common one and is much more natural to women than to men. The idea is a sub-section of phallic symbolism, concerning which the reader may be reminded of the following points: there are two broad classes of such symbols, the patriarchal symbols of the eagle, bull, and so on, representing the father's power and rights, and the matriarchal symbols representing the revolutionary son. The latter are again divided into two sub-groups, those, such as the devil, the cock, the serpent, and so on, which are tabooed and interdicted, and those, such as the goat, the ape, and the ass[1] (the animal sacred to the worship of Priapus, with which the figure of Punchinello is constantly brought into association), which are contemned as ridiculous and comic.

I might add that there is a slight trace of the original revolutionary meaning of the matriarchal phallic symbol left in the pose of such comic figures—the most striking example of which was the mediaeval court jester—as critics who lash the conventions of society or the foibles of rulers. There is a hint of this point in one of Bernard Shaw's prefaces[2]; it runs: "Every despot must have one disloyal subject to keep him sane. ...Democracy has now handed the sceptre of the despot to the sovereign people; but they, too, must have their confessor, whom they call Critic. Criticism is not only medicinally salutary: it has positive popular attractions in its cruelty, its gladiatorship, and the gratification given to envy by its attacks on the great, and to enthusiasms by its praises. It may say things which many would like to say, but dare not....Its iconoclasms, seditions, and blasphemies, if well turned, tickle those whom they shock; so that the Critic adds the privileges of the court jester to those of the confessor. Garrick, had he called Dr Johnson Punch, would have spoken profoundly and wittily; whereas Dr Johnson, in hurling that epithet at him, was but picking up the cheapest sneer an actor is subject to."

We have next to consider the respects in which this example differs from those given earlier in the paper, and it will be well first to examine the definitions offered by other writers. The most exact of these is that

[1] See Storfer, *Marias Jungfräuliche Mutterschaft*, 1914.

[2] G. B. Shaw, *Plays Unpleasant*, 1898, p. viii.

given by Rank and Sachs[1], which I will quote in full: "A final means of expression of repressed material, one which lends itself to very general use on account of its especial suitability for disguising the unconscious and adapting it (by compromise formations) to new contents of consciousness, is the Symbol. By this term we understand a special kind of indirect representation which is distinguished by certain peculiarities from the simile, metaphor, allegory, allusion and other forms of pictorial presentation of thought material (after the manner of a rebus), to all of which it is related. The symbol represents an almost ideal union of all these means of expression: it is a substitutive, perceptual replacement-expression for something hidden, with which it has evident characteristics in common or is coupled by internal associative connections. Its essence lies in its having two or more meanings, as indeed it originated itself in a kind of condensation, an amalgamation of individual characteristic elements. Its tendency from the conceptual to the perceptual indicates its nearness to primitive thought; by this relationship symbolisation essentially belongs to the unconscious, but, in its function as a compromise, it in no way lacks conscious determining factors, which in varying degrees condition both the formation of symbols and the understanding for them."

The writers then specify the characteristics of true symbols as follows[2]: "Representation of unconscious material, constant meaning, independence of individual conditioning factors, evolutionary basis, linguistic connections, phylogenetic parallels in myths, cults, religion, etc." These attributes will next be examined and commented on in order.

1. *Representation of unconscious material.* This is perhaps the characteristic that most sharply distinguishes true symbolism from the other processes to which the name is commonly applied. By it is meant, not so much that the concepts symbolised are not known to the individual, for most often they are, as that the affect investing the concept is in a state of repression, and so is unconscious. Further, the process of symbolisation is carried out unconsciously, and the individual is quite unaware of the meaning of the symbol he has employed, and often of the fact that he has employed one at all, for he takes the symbol for reality. The actual comparison between the idea symbolised and the symbol has either never been conscious at all, or has been forgotten. In many cases

[1] Rank and Sachs, *Die Bedeutung der Psychoanalyse für die Geisteswissenschaften*, 1913, 11.

[2] *op. cit.* 18.

this point of comparison is evident as soon as one's attention is directed to the fact of comparison. In other cases considerable reflection is needed to discover it, and in some cases it is not yet patent—that is to say, any possible points of comparison between the two ideas seem too tenuous to justify the symbolism, even when the fact of the latter is undoubted.

2. *Constant meaning.* The statement here implied needs some modification. A given symbol may have two or occasionally even more meanings; for instance, in dreams a room may symbolise either a woman or a womb. In that case the interpretation will depend on the context, the associations, and other material available. A preference for one of these meanings can sometimes be correlated with the social class, the mental circle, the race, or the mental constellation of the individual. But the possible variation in meaning is exceedingly restricted, and the striking feature is its constancy in different fields of symbolism, dreams, myths, etc., and in different kinds of people. Further, in interpretation it is usually a question, not of either this meaning or that, but of both. In unconscious condensation, *e.g.* in dreams, there are several layers, in each of which one of the meanings is the true one. When these points are appreciated it will be seen that there is little scope for arbitrariness in the interpretation of symbols.

3. *Non-dependence on individual conditioning factors.* Though not strictly independent of individual factors, true symbolism is not conditioned by these alone. In the creation of a given symbol the individual has but a restricted range of choice, more important determining factors being those that are common to large classes of men or, more often, to mankind as a whole. The part played by individual factors is a much more modest one. While the individual cannot choose what idea shall be represented by a given symbol (for the reason just mentioned), he can choose what symbol out of the many possible ones shall be used to represent a given idea; more than this, he can sometimes, for individual reasons, represent a given idea by a symbol that no one else has so used[1]. What he cannot do is to give a regular symbol a different meaning from anyone else; he can merely choose his symbols or make new ones, and even in the latter case they have the same meaning as they would with other people who might use them.

This curious independence of symbolic meanings raises in another form the old question of the inheritance of ideas. Some writers, *e.g.* Jung, hold that anthropological symbolism is inherited as such and explain in

[1] See Freud, *Die Traumdeutung*, 4e Aufl. 1914, 261.

this way its stereotyped nature. For reasons I have developed elsewhere[1], I adhere to the contrary view that symbolism has to be re-created afresh out of individual material, and that the stereotypy is due to the uniformity of the fundamental and perennial interests of mankind. If this view is true, then further study of the subject must yield important conclusions as to the nature of the latter.

4. *Evolutionary basis.* This will be dealt with later.

5. *Linguistic connections.* We have seen that in symbolism the unconscious notices and makes use of comparisons between two ideas which it would not occur to our conscious mind to bring together. Now the study of etymology, and especially of semantics, reveals the interesting fact that, although the word denoting the symbol may have no connotation of the idea symbolised, yet its history always shows some connection with the latter. This connection may be one of different kinds. Thus it may appear in one sphere of thought, *e.g.* wit, when it is not present in the ordinary use of the word: for example, the well-known 'officers' remounts' joke current during the South African War illustrates the unconscious association between the ideas of riding and of coitus, although this association is very far from being present in most spheres of thought. It may appear in an older, and now obsolete use of the same word, in its root, or in cognate words.

This may be illustrated from the example of symbolism depicted above. The name Punchinello, is an English contamination (see below) derived from the Neapolitan *pol(l)ecenella* (modern Italian *pulcinella*), which is the diminutive of *pollecena*, the young of the turkey-cock (the modern Italian *pulcino* means pullet, *pulcinello* being its diminutive); the turkey-cock itself is a recognised phallic symbol, as, indeed, is the domestic cock, both ideationally and linguistically. The Latin root is *pullus*, which means the young of any animal; the phallus is often, for obvious reasons, identified with the idea of a male child, a little boy or little man. The reason why the name came to be used in this connection is thought to be the resemblance between the nose of the actor and the hooked bill of the bird, and again it may be pointed out that nose and beak are common phallic symbols.

The name *polecenella*, or its English variant 'polichinello' (*via* the French *polichinelle*), was contaminated with the English word 'punch,' the main meaning of which is a tool for perforating material, with or without the impressing of a design (*e.g.* to pierce metal or to stamp a die); it used to mean a dagger (another common symbol). The word is short

[1] *Imago*, I. 1912, 486, 487.

for 'puncheon,' which used to mean a bodkin or dagger, and is now used in carpentry to denote 'a short upright piece of timber'; it comes from the late Latin *punctiare*, to prick or punch. Pepys, in his *Diary*, April 30, 1669, calls punch "a word of common use for all that is thick and short." Suffolk punches are thick-set draught horses with short legs. To sum up, the four ideas that keep recurring in connection with the name Punchinello are (1) a caressing name for male offspring, equivalent to 'little man,' (2) a projecting part of the body, (3) the notion of piercing or penetrating, and (4) that of shortness and stoutness—four ideas that admirably serve to describe the male organ and nothing else; indeed, there is no other object to which the curious combination applies of stoutness and pricking.

In connection with the phallic signification of the staff wielded by Punchinello one may remark that the word is cognate with the M.H.G. *staben*, to become stiff, both probably coming from a pre-Teutonic root *sta*, meaning to stand up. A more familiar piece of knowledge is that the word 'yard,' used as a measure of length, had three centuries ago two other current meanings, (1) a staff, and (2) the phallus; it is still used in the latter sense by sailors. It is an equivalent of the jester's bauble. In addition to the long nose and staff, already mentioned, Punchinello displays several other phallic attributes, the dog Toby being one. The fact that such a symbol can in its turn have similar symbols attached to it, a fact strikingly illustrated in the phallic ornaments worn as amulets by Roman ladies[1], confirms the view taken above of the identification of man with phallus.

Even with symbol words where it is hard to trace any association between them and the words denoting the ideas symbolised, such an association is often apparent in the case of synonyms or foreign equivalents. A good example is our word room—a regular unconscious symbol for woman—where one has to go to very remote Aryan sources, *e.g.* Old Irish, to find any trace of a feminine connotation; one has only to turn, however, to the German equivalent, *Zimmer*, to find that the compound *Frauenzimmer* is a common colloquialism for woman.

6. *Phylogenetic parallels.* One of the most amazing features of true symbolism is the remarkable ubiquity of the same symbols, which are to be found, not only in different fields of thought, dreams, wit, insanity, poetry, etc., among a given class and at a given level of civilisation, but among different races and at different epochs of the world's history. A symbol which to-day we find, for instance, in an obscene joke, is also

[1] See Vorberg, *Museum eroticum Neapolitanum*, Sect. 'Bronzen.'

to be found in a mythical cult of Ancient Greece, and another that we come across only in dream analysis was used thousands of years ago in the sacred books of the east. The following examples may be quoted in illustration of this correspondence. The idea of teeth, in dreams, is often symbolically related to that of child-birth, a connection that is never to be found in consciousness; in the Song of Songs we read: "Thy teeth are as a flock of sheep, which go up from the washing, whereof everyone beareth twins, and there is not one barren among them." The idea of a snake, which is never consciously associated with that of the phallus, is regularly so in dreams, being one of the most constant and invariable symbols: in primitive religions, and even in the Old Testament, the two ideas are so interchangeable that it is often hard to distinguish phallic from ophitic worship. The idea of father or mother is constantly symbolised in dreams by that of king or queen respectively. The word king is ultimately derived from the Sanscrit root *gan*, meaning to beget; *ganaka* was the Sanscrit for father, and occurs also in the Vedas as the name of a well-known king. The word queen comes from the Sanscrit *ganî*, which means simply mother. The Czar of Russia is, or rather was until recently, called the 'Little Father,' the same title as the Hunnish Attila (diminutive of Atta = father). The title 'Landesvater' is commonly used in Germany, just as the Americans still call Washington the Father of his Country. The ruler of the Catholic Church is called the 'Holy Father,' or by his Latin name of Papa.

By adding the six attributes just discussed to the more general six mentioned earlier we have formulated a conception of symbolism as distinct from the other kinds of indirect representation. The precise differences and relationship between them will be discussed more fully below, and we may conclude this section by a brief consideration of the actual content of symbolism.

The number of symbols met with in practice is extraordinarily high, and can certainly be counted by thousands[1]. In astonishing contrast with

[1] There is no satisfactory comprehensive work on the content of symbolism. The most reliable collection, unfortunately much too unfinished for what is needed, is that given in Freud's *Traumdeutung* (4e Aufl., S. 262–274), amplified in his *Vorlesungen zur Einführung in die Psychoanalyse* (Zweiter Teil, 1916, S. 164–180). The numerous examples scattered through Otto Rank's works can also be depended on. In Stekel's *Sprache des Traumes* and his *Angstzustände* there is an extensive material, useful to those capable of criticising it. On the anthropological side one may mention the well-known works by Bachofen, *Versuch über die Gräbersymbolik der Alten*, 1859; Burton, *Terminal Essay of the Arabian Nights*, 1890; Cox, *Mythology of the Aryan Nations*, 1870; Dieterich, *Mutter Erde*, 2e Aufl., 1913; Dulaure, *Des divinités génératrices*, 1805 (much enlarged in a German edition by Krauss and

this stands the curious fact that the number of ideas thus symbolised is very limited indeed, so that in the interpretation of them the complaint of monotony is naturally often heard.

All symbols represent ideas of the self and the immediate blood relatives, or of the phenomena of birth, love, and death,—in short, the most primitive ideas and interests imaginable. The actual number of ideas is rather greater, however, than might be supposed from the briefness of this summary—they amount perhaps to about a hundred—and a few supplementary remarks are necessary. The self comprises the whole body or any separate part of it, not the mind; perhaps twenty different ideas can here be symbolised. The relatives include only father, mother, brother, sister, son, daughter; various parts of their bodies also can be symbolised. Birth can refer to the ideas of giving birth, of begetting, or of being born oneself. The idea of death is in the unconscious a relatively simple one, that of lasting absence; it always refers to the death of others, for the idea of one's own death is probably inconceivable as such in the unconscious, being always converted into some other one. Love, or more strictly sexuality, comprises a very considerable number of distinct processes, including some, such as excretory acts, that are not commonly recognised to have a sexual bearing; it would lead us too far to enumerate and describe them all here, but it may be said that the total conception thus reached closely corresponds with Freud's theory of sex[1]. The field of sexual symbolism is an astoundingly rich

Reiskel, *Die Zeugung in Glauben, Sitten und Bräuchen der Völker*, 1909); Faber, *Origin of Pagan Idolatry*, 1816; Fanin, *Secret Museum of Naples*, Engl. Transl., 1872; Fergusson, *Tree and Serpent Worship*, 1873; Forlong, *The Rivers of Life*; Higgins, *Anacalypsis*, 1833–1836; Inman, *Ancient Faiths embodied in Ancient Names*, 1868, and *Ancient Pagan and Modern Christian Symbolism* (the most useful book on the subject), 1869, Second Edition, 1874; Hargrave Jennings, *The Rosicrucians*, 1887; King, *The Gnostics and their Remains*, 1864; Payne Knight, *A Discourse on the Worship of Priapus*, 1786, New Edition, 1871, and *The Symbolical Language of Ancient Art and Mythology*, 1818, New Edition, 1876; Moor, *Hindu Pantheon*, 1810; Staniland Wake, "The Influence of the Phallic Idea in the Religions of Antiquity," *Journ. of Anthropology*, 1870, Nos. 1 and 2, and *Serpent Worship*, 1888; Wake and Westropp, *Ancient Symbol Worship*, Second Edition, 1875; Westropp, *Primitive Symbolism*, 1885; together with the less known works by Campbell, *Phallic Worship*, 1887; Freimark, *Okkultismus und Sexualität*; Hermann, *Xenologie des Saeming*, 1905; Kittel, *Über den Ursprung des Lingakultus in Indien*, 1876; Laurent and Nagour, *L'occultisme et l'amour*; Maehly, *Die Schlange im Mythus und Cultus der classischen Völker*, 1867; de Mortillet, *Le Signe de la Croix avant le Christianisme*, 1866; Sellon, "Phallic Worship in India," *Memoirs of the Anthropological Society*, I., and *Annotations on the Sacred Writings of the Hindus*, New Edition, 1902; Storfer, *op. cit.* A number of recent books, *e.g.* those by Bayley, Blount, Churchward, Hannay, are of much less value than their pretensions would suggest.

[1] See Freud, *Drei Abhandlungen zur Sexualtheorie*, 1905, or Chapter III of my *Papers on Psycho-Analysis*, Second Edition, 1918.

and varied one, and the vast majority of all symbols belong to this category[1]; there are probably more symbols of the male organ itself than all other symbols put together. This is a totally unexpected finding, even more so than the paucity of symbolised ideas in general, and is so difficult to reconcile with our sense of proportion that it needs an effort to refuse the easy escape of simply denying the facts, a feat which is greatly facilitated by the circumstance that thanks to our education the facts are not at all accessible. Rank and Sachs's comments in this connection are of interest[2]: "The prevalence of sexual meanings in symbolism is not to be explained merely by the individual experience that no other instinct is to the same extent subjected to social suppression and withdrawn from direct gratification as the sexual instinct, which has been built up from multiform 'perverse' components, and the mental domain of which, the erotic, is therefore extensively susceptible of, and in need of, indirect representation. Much more significant for the genesis of symbolism is the phylogenetic fact that in primitive civilisations an importance was attached to sexual organs and functions that to us appears absolutely monstrous, and of which we can form some approximate idea from the results of anthropological investigations and the traces remaining in cults and myths."

III. Genesis of Symbolism.

In true symbolism, we have seen, a comparison between two ideas, of a kind that is alien to the conscious mind, is established unconsciously, and then one of these—which may conveniently be called the secondary idea—may unknowingly be substituted for, and so represent, the first, or primary idea. From this two questions immediately arise: Why are two ideas identified which the conscious mind does not find to be similar, and why does the one idea symbolise the other and never the reverse?

Taking the former question first, we note that it is the primitive mind, not the adult, conscious mind which institutes the comparison between the two ideas. This conclusion is confirmed by everything we know about symbolism, the type of mental process, the high antiquity, in both the individual and the race, of the actual symbols themselves, and so forth; even the few new symbols that are made by the adult, *e.g.* the Zeppelin one, are created by the primitive, infantile mind that persists throughout life in the unconscious.

[1] See Schlesinger, *op. cit.* 437, *et seq.*

[2] Rank and Sachs, *op. cit.* 12.

Just as the simile is the base of every metaphor, so is an original identification the base of every symbolism, though the two processes must not be confounded. As Freud puts it[1], "What to-day is symbolically connected was probably in primaeval times united in conceptual and linguistic identity. The symbolic relationship seems to be the remains and sign of an identity that once existed."

The tendency of the primitive mind—as observed in children, in savages, in wit, dreams, insanity, and other products of unconscious functioning—to identify different objects and to fuse together different ideas, to note the resemblances and not the differences, is a universal and most characteristic feature, though only those familiar with the material in question will appreciate the colossal scale on which it is manifested. It impresses one as being one of the most fundamental and primordial attributes of the mind. In explanation of it there are two hypotheses, which may here be indicated. The one most usually accepted would refer the phenomenon under discussion, as well as most others of symbolism, to the structure of the undeveloped mind, for which reason it might be termed the static hypothesis; the main feature to which they call attention is the intellectual incapacity for discrimination. The second, psycho-analytical hypothesis considers this factor important but inadequate, and postulates other, dynamic factors as well.

In my opinion, not one factor, but three factors are operative. The first, which is the only one usually recognised, but which I think is much the least important, is mental incapacity. The second, which I shall point out presently, has to do with the 'pleasure-pain principle,' and the third, to which Rank and Sachs call attention, with the 'reality-principle[2].'

The first factor, which I think I shall be able to prove cannot be exclusive, is well indicated in the following passages. Pelletier says[3]: "Il est à remarquer que le symbole joue un très grand rôle dans les divagations des aliénés; cela est dû à ce que le symbole est une forme très inférieure de la pensée. On pourrait définir le symbole comme la perception fausse d'un rapport d'identité ou d'analogie très grand entre deux objets qui ne presentent en realité qu'une analogie vague." We shall see that the disproportion in the importance of the analogy depends on the different points of view of the patient and the doctor rather than

[1] Freud, "Die Traumdeutung," *op. cit.* 261.

[2] For the exact sense in which these terms are used, see my *Papers on Psycho-Analysis*, Second Edition, 1918, Chap. I.

[3] Pelletier, *L'Association des Idées dans la Manie aiguë*, 1903, 129.

on any intellectual inferiority of the former. Jung, from a similar standpoint, writes[1]: "The apperceptive defect is manifested in a lessened clearness of ideas. If the ideas are not clear, neither are the differences between them." He says further: "I will only point out that the many significations of the individual dream images (Freud's 'over-determination')[2] is a sign of the lack of clarity and definition in dream thought. Because of the defective sensibility for differences that prevails in dreams the contents of both complexes can become confounded at least in symbolic form." Both these authors were probably influenced by the common, but fallacious, view of dreams and insanity as *defective* mental products. Silberer, however, approaching the matter from quite another point of view, also writes[3]: "In agreement with the majority of writers, I see the chief and most general condition of symbol-formation—valid with the phenomena of health and disease, in the individual and in the race—in an *inadequacy* of the apprehensive faculty in regard to its object, or, as one might also say, in an *apperceptive insufficiency*." We may admit the presence of this factor so far as it goes, but I think it can be shown that what appears to be apperceptive incapacity is very often merely a non-functioning due to other causes. It is true that the primitive mind very often does not discriminate, but that is not because it cannot, for when it needs, it certainly can, to a remarkable extent.

The second factor leading to lack of discrimination is that when the primitive mind is presented with a new experience it seizes on the resemblances, however slight, between it and previous experiences; and this for two reasons, both of which have to do with the pleasure-pain principle. The first of these is that the mind—above all the primitive mind, which is ruled by this principle—notices most what most *interests* it personally, what, therefore, is most pleasurable or most painful. Distinctions indifferent to it are ignored. In practice one is apt erroneously to assume that the interests of the primitive mind are necessarily the same as our own conscious ones; actually, however, the relative proportion of interest is often astoundingly different in the two cases. The unexpected associations made by a child when confronted with a novelty are often very amusing to us, for example, the remark that soda-water tastes like a foot that has gone to sleep. Darwin's oft-quoted example of the child who, on first seeing a duck, onomatopoeically named it 'quack,' and then later applied this word also to flies, wine, and even a sou (which

[1] Jung, *Über die Psychologie der Dementia praecox*, 1907, 72.

[2] This is the same as the condensation or over-identification under discussion.

[3] Silberer, *Jahrbuch der Psychoanalyse*, III. 680.

had eagle's wings), is rightly explained by Meumann[1], who points out that the child noticed only what interested him, namely the flying and the relation to fluid, and so used this word to denote these two phenomena in whatever form they occurred; it was not the duck as a whole that was named 'quack,' but only certain abstracted attributes, which then continued to be called by the same word. The second of the two reasons referred to above is more general and far-reaching. When a new experience is presented to the mind it is certainly *easier* to perceive the points of resemblance between it and previous familiar experiences. One often hears, for instance, such a remark as "The ideas in that book were too strange for me to take in on first reading it; I must go through it again before passing an opinion on it." In such a case if one notices only the points of resemblance there is effected an obvious economy of effort, which is a fundamental human trait; Ferrero[2] aptly calls it "la loi de l'inertie mental" and "la loi du moindre effort." This is of course governed by the hedonic pleasure-pain principle, though the fact is often obscured by writers on ethics. The association between ease and pleasure, and between difficulty or labour and pain, is primordial and well illustrated by the words used to denote them. The word painful was used in Middle English in the sense of industrious. The French *travail*, work, is cognate with the Italian *travaglio*, which means suffering; the Italian word for work, *lavoro*, comes from the Latin *labor*, pain. The Greek *πένομαι* means both to work and to suffer, as does the Hebrew *assab*. We appropriately refer to child-birth as labour.

The third factor is not sharply to be distinguished from the last, though it refers rather to the 'reality-principle.' It is clear that the appreciation of resemblances facilitates the assimilation of new experiences. Our instinctive tendency in such a situation is to link on the new to the old. In this way the process of fusion or identification aids our grasp of reality and makes it possible for us to deal with it more adequately. True, it is a process with grave possibilities of defects, it being an everyday occurrence that we assimilate the new too closely in terms of the old, but to assimilate it at least in some degree is the only way in which we can deal with it at all. Rank and Sachs[3] have an illuminating passage on the relation of symbolism to this primary identification in the service of adaptation: "Psychologically considered, symbol-formation remains a regressive phenomenon, a reversion to a certain stage of pictorial thinking, which in fully civilised man is most

[1] Meumann, *Die Sprache des Kindes*, 1903.
[2] Ferrero, *op. cit.* 6, 18, 23.
[3] Rank and Sachs, *op. cit.* 17.

plainly seen in those exceptional conditions in which conscious adaptation to reality is either restricted, as in religious and artistic ecstasy, or seems to be completely abrogated, as in dreams and mental disorders. In correspondence with this psychological conception is the original function, demonstrable in the history of civilisation, of the identification underlying symbolism[1] as a means to adaptation to reality, which becomes superfluous and sinks to the mere significance of a symbol as soon as this task of adaptation has been accomplished. Symbolism thus appears as the unconscious precipitate of primitive means of adaptation to reality that have become superfluous and useless, a sort of lumber-room of civilisation to which the adult readily flees in states of reduced or deficient capacity for adaptation to reality, in order to regain his old, long-forgotten playthings of childhood. What later generations know and regard only as a symbol had in earlier stages of mental life full and real meaning and value. In the course of development the original significance fades more and more or even changes, though speech, folk-lore, wit, etc., have often preserved more or less plain traces of the original association."

The two last factors, the importance of the pleasure-pain principle and of adaptation to reality in respect to primitive lack of discrimination, throw light on one of the most puzzling phenomena of symbolism—namely, the extraordinary predominance of sexual symbols. A Swedish philologist, Sperber[2], has in a remarkable essay elaborated the theory, several times suggested on other grounds by biologists, that sexual impulses have played the most important part in both the origin and later development of speech. According to this theory, which is supported by very weighty considerations, the earliest speech sounds were those that served the purpose of calling the mate (hence the sexual importance of the voice to this day), while the further development of speech roots accompanied the performance of work. Such work was done in common and, as is still customary enough, to the accompaniment of rhythmically repeated speech utterances. During this, sexual interest was attached to the work, as though—so to speak—primitive man reconciled himself to the disagreeable but necessary task by treating it as an equivalent of, and substitute for, sexual functioning. Words used during these common tasks thus had two meanings, denoting the sexual

[1] Note how nicely the authors distinguish between identification and symbolism in this connection.

[2] Sperber, "Über den Einfluss sexueller Momente auf Entstehung und Entwicklung der Sprache," *Imago*, 1912, I. 405.

act and the equivalent work done respectively. In time the former meaning became detached and the word, now applying only to the work, thus 'desexualised.' The same would happen with other tasks, and so a store of speech roots gradually accumulated, the original sexual significance of which had been lost. Sperber then illustrates, with an extensive material, the fact that words having a sexual connotation possess a perfectly astounding capacity for development and extension into non-sexual fields. Partly owing to the careful expurgation of our etymological dictionaries, it is not generally known that an enormous number of common words in present-day use have been derived in historical times from this source, attaining their present meaning through a primary sexual association that has now been forgotten. In the light of work like Sperber's we begin to understand why there is such an amazing number of symbols for sexual objects and functions, and, for instance, why weapons and tools are always male symbols while the material that is worked on is always female. The symbolic association is the relic of the old verbal identity; things that once had the same name as a genital organ can now appear in dreams, etc., as a symbol for it. Freud[1] aptly likens symbolism to an ancient speech that has almost vanished, but of which relics still remain here and there.

According, then, to the view here developed, the identification that underlies symbolism is mainly determined by the two factors discussed above, which may be summarised as the tendencies to seek pleasure and avoid pain, and to learn to deal with reality in the easiest and most sparing way. It was just the way in which primitive man must have met the world, the desire for ease and pleasure struggling with the demands of necessity. He succeeded by making a compromise in which he sexualised his tasks. A few examples may be given from the vast subject of the associations between ploughing in particular, or agriculture in general, and sexual activities. Most of the tools used are phallic symbols (the word itself is the commonest vulgar designation), a statement that can easily be proved from folk-lore and mythology, while the conception of the earth as woman, and especially as mother, is universal and fundamental[2]. Sophocles' *Oedipus* repeatedly speaks of "the mother-field from which I sprouted." Shakespeare makes Boult, on the point of deflorating the recalcitrant Marina, say: "An if she were a thornier piece of ground than she is, she shall be ploughed[3]." The words for 'plough' in

[1] Freud, *Vorlesungen zur Einführung in die Psychoanalyse*, Zweiter Teil; "Der Traum" 1916, 181.

[2] See Dieterich, *Mutter Erde*, 2e Aufl. 1913.

[3] *Pericles*, Act IV. Sc. VI.

Latin, Greek, and oriental languages were customarily used also to denote the sexual act[1], and we still use such words as 'seed,' 'fertility,' 'barrenness,' for vegetation as well as for human beings. The association becomes quite manifest in the well-known fertilising magic, a custom that lasted late into civilised times; it consisted in a naked pair performing the sexual act in the field, so as to encourage the latter to imitate their example. The Greek words for garden, meadow, field, common female symbols, were also used to denote the female genital organ.

If, as is here maintained, the individual child re-creates such symbolism anew, *i.e.* if he (largely unconsciously) perceives these comparisons which are alien to the adult conscious mind, then it is plain that we shall have radically to revise our conception of the infantile mind, and especially in regard to sexuality. This has already been done by Freud on other grounds, after he had empirically discovered from psycho-analyses that the unconscious mind of the child, and even the conscious one, is much more sexual in character than had ever been supposed[2]. In fact, the whole process to which he has given the name 'sublimation[3],' is probably an ontogenetic repetition of the one just described, whereby sexual energy is gradually drained into other non-sexual channels. The activity—tasks in the life of primitive man, games in that of the child—becomes by degrees independent of this source of interest that is not inherent in itself, but the ancient association remains in the unconscious, where in suitable circumstances it may again manifest itself in the form of symbolism.

It will not have escaped the attentive reader than in this discussion all the stress has been laid on the defective discrimination shown by the primitive mind, while nothing has been said about the respects in which it shows an unwonted power of discrimination[4]. Yet this is also a striking characteristic of both children and savages, though not of the unconscious mind. In the case of savages, it has curiously been used as an argument in support of the current theory of the defective intellectual powers on the part of the primitive, but, in my judgment, closer consideration proves just the contrary. Herbert Spencer, in his *Principles of Sociology*, has collected a series of examples where there are many separate words for individual acts, but no generic one for the act itself—thus thirty

[1] Kleinpaul, *Das Leben der Sprache*, 1893, III. 136.

[2] Freud, *Drei Abhandlungen*, *op. cit.*

[3] See Chapter XXXIV. in my Papers, *op. cit.*

[4] A consideration which in itself finally proves that the prevalent hypothesis of the primitive lack of discrimination—that this is due to intellectual incapacity—is inadequate to cover the whole ground.

words for washing different parts of the body and none for the act of washing. The Arabians are said to have over 500 words to designate lions, in various aspects, but no word for lion; 5744 for camels, but none for a camel. This is certainly a powerful argument against any inherent incapacity for discrimination, as the holders of that hypothesis maintain exists. Whereupon they simply change their ground, and, being bent on convicting the primitive of intellectual inferiority, they now quote such facts to show that he is incapable of abstracting; this is, at all events, a different thing from being incapable of discriminating. Thus Stout[1] writes: "It certainly appears odd that a lower grade of intellectual development should be marked by superior nicety and precision of discriminative thought. The truth is that these distinctions, so plentiful in savage languages, are due rather to an incapacity for clearly apprehending identity in difference, than to a superior power of apprehending difference in identity." This argument, however, has been very neatly disposed of by Hocart[2], who has pointed out that the key to the whole question is the matter of interest. Comparing the Fijian language with English, as an example, he shows that the Fijian handles in gross where we do in retail, but that the converse is equally true. Where our interest is very great we have no generic terms, because the differences are so important as to overshadow the resemblances; in such cases the Fijian, with less interest, will use a general and often vague term to cover the whole. The distinction, for instance, is so important among a bull, a cow, an ox, a steer, a calf, a bullock, a heifer, and so on, that we have no single word to denote the species as a whole except cattle, which is collective. Indeed the same law may be observed to hold good even between different classes in the same country. The laity uses the generic term 'horse,' but a horse dealer, *i.e.* someone with a great interest in the matter, has no such generic term; to him a horse is a certain variety of the animal and is different from a stallion or a mare. Similarly we speak of ships as a class of objects of which there are many varieties, but to a sailor a ship is definitely a vessel with a bowsprit and at least two square-rigged masts; the distinctions between different vessels are to him more important than the resemblances.

It is well known that abstract terms arise originally from concrete ones; we see here that they characteristically arise as a generalisation from a single example, thus the order of development seems to be concrete, general, abstract. This conclusion can also be supported from

[1] Stout, *Analytic Psychology*, 1902, II. 231.

[2] Hocart, This *Journal*, v. 267.

consideration of the order of development of the parts of speech. Thus, as Wundt shows[1], adjectives, which are of relatively late development, had originally the same form as substantives and were, to begin with, merely special nouns. For example, a brown leaf and a green leaf were two distinct words, having nothing in common with words for other objects that are red or green. Then one of these 'green' words, one where the element of greenness was very prominent (perhaps with leaves), was extended to other objects when it was wished to call special attention to the green aspect of this object, *e.g.* a green-leaf cloth, losing in time its substantival connotation of leaf. It is known, for instance, that the Greenlanders have separate names for each finger and that when they want to use a name for fingers in general they employ the name of the principal one (the thumb) for this purpose. They are here reaching from the particular to the general, the first stage of conceiving the abstract.

It will be seen that our custom of using the word ship to denote all sea-going vessels constitutes in type a reversion to the primitive, infantile custom of not discriminating from relative lack of interest, and so, in a sense, is all generalisation. The essential difference between what is called a valuable generalisation, *e.g.* a scientific one, and the simple grouping together characteristic of the primitive mind resides in the practical worth of the generalisation. To the child, no doubt, its identifications are useful personally as a great generalisation is to a man of science, but, while they may be equal subjectively, they are not objectively. The second kind takes into better account the facts of external reality, is altogether on a more real and less subjective plane; in short, there is all the difference that exists between the simple pleasure-pain principle and the reality-principle. From this point of view there opens the possibility, which cannot be followed up here, of a theory of scientific discovery, invention, etc., for this psychologically consists in an overcoming of the resistances that normally prevent regression towards the infantile, unconscious tendency to note 'identity in differences,' the whole being of course worked out on the plane of reality though the impetus comes from the association between the unconscious ideas that the 'real' external ones can symbolise.

We have next to turn to the second of the two questions raised at the beginning of this section—namely, why it is that of two ideas unconsciously associated one always symbolises the other and never the reverse. To illustrate by an example what is meant: a church tower in a dream, as in anthropology, often—though, of course, not always—

[1] Wundt, *Völkerpsychologie*, 1904, Bd I. Teil II. 289.

symbolises the phallus, but a phallus never symbolises a church tower. This irreversibility alone demolishes the hypothesis that symbolism is due solely to any apperceptive insufficiency, since then the symbolism should be reciprocal. The point is clearly put by Ferenczi, who writes[1]: "One was formerly inclined to believe that things are confounded because they are similar; nowadays we know that a thing is confounded with another only because certain motives for this are present; similarity merely provides the opportunity for these motives to function." Assuming, then, that two ideas have become closely associated, in the way described above, what are the motives that lead to one of the ideas replacing the other, whereas the reverse never occurs? The answer will, of course, be found only by consideration of the material content of the ideas themselves. The two most prominent features in regard to these are: First, the ideas symbolised are the most primordial that it is possible to conceive and they are those invested with the strongest primary interest. Secondly, that attaching to them all are powerful affective and conative processes which are in a state of psychical repression; they are, in fact, the most completely repressed mental processes known.

It is impossible not to connect these two considerations. It is a well-established observation of clinical psychology that when a strong affective tendency is repressed it often leads to a compromise-formation —neurotic symptoms being perhaps the best known example—in which both the repressed and the repressing tendencies are fused, the result being a substitution-product. From this it is a very slight step to infer that symbols are also of this nature, for they, like other compromise-formations, are composed of both conscious and unconscious elements. Symbolism certainly plays an important part in many neurotic symptoms; a castration complex, for instance, often results in a phobia of blindness, the eye being one of the commonest somatic phallic symbols[2]. That symbolism arises as the result of intrapsychical conflict between the repressing tendencies and the repressed is the view accepted by all psycho-analysts. It is implicit, for instance, in Ferenczi's[3] actual definition of symbols as "such ideas as are invested in consciousness with a logically inexplicable and unfounded affect, and of which it may be analytically established that they owe this affective over-emphasis to *unconscious* identification with another idea, to which the surplus of affect really be-

[1] Ferenczi, *Contributions to Psycho-Analysis.* English Transl. by Ernest Jones, 1916, 237.

[2] See Ferenczi, "On Eye Symbolism," *op. cit.* 228–232.

[3] Ferenczi, *op. cit.* 234.

longs. Not all similes, therefore, are symbols, but only those in which the one member of the equation is repressed into the unconscious." According to him, the most primary kind of symbolism is probably the equating of one part of the body with another, one subsequently replacing the other[1]; there thus comes about an over-emphasis of the upper part of the body in general, interest in the lower half being repressed (Freud's "displacement from below upwards").

All psycho-analytical experience goes to show that the primary ideas of life, the only ones that can be symbolised—those, namely, concerning the bodily self, the relation to the family, birth, love, and death—retain in the unconscious throughout life their original importance, and that from them is derived a very large part of the more secondary interests of the conscious mind. As energy flows from them, and never to them, and as they constitute the most repressed part of the mind, it becomes comprehensible that symbolism should take place in one direction only. Only what is repressed is symbolised; only what is repressed needs to be symbolised. This conclusion is the touchstone of the psycho-analytical theory of symbolism.

IV. Functional Symbolism.

The theory of symbolism just described is manifestly not complete: it does not, for instance, explain why only certain possible comparisons are used as symbols, nor why some symbols are found predominantly in certain fields, *e.g.* dreams, and others mainly in different fields, *e.g.* wit. While, however, the theory needs amplifying and supplementing, I would maintain that it does at least begin to introduce order into a confused subject, notably in distinguishing between symbolism and other forms of figurative representation.

Further progress in clarification may be gained by examination of the work of what may be called the post-psycho-analytical school of writers, Adler, Jung, Maeder, Silberer, Stekel, with their English followers, Eder, Long, and Nicoll. The feature common to the members of this school is that, after gaining some knowledge of psycho-analysis, they have proceeded, by rejecting the hardly-won knowledge of the unconscious, to re-interpret the psycho-analytical findings back again into the surface meanings characteristic of pre-Freudian experience, retaining, however, the psycho-analytical technical terms though using them with quite different implications. The conception of symbolism has especially

[1] Ferenczi, *op. cit.* 232.

suffered from the confusion thus re-introduced, for it has been diluted to such an extent as to lose all exact descriptive value. Thus Jung makes constant use of the term 'Libido-symbol,' but, as Libido means to him psychical energy in whatever form and symbol means simply any form of indirect representation, the term comes to mean merely "any mental process that is substituted for any other." He does not hesitate to use the term 'symbol' in precisely the reverse sense from that in which it is used in psycho-analysis. Take the case of a patient where an associative connection has been established between a given symptom (*e.g.* inhibition in performing a particular act) and an unconscious incest complex[1]. By the psycho-analyst the symptom would be regarded as the result of the complex and, in certain circumstances, as a symbol for it; Jung, on the other hand, calls the complex the symbol of the symptom, *i.e.*, according to him, an unconscious idea may be a symbol of a conscious one.

Silberer's work is in some respects in a different category from that of the other writers mentioned, for he is the only member of this school who has made a positive contribution to the theory of symbolism; unfortunately incautious presentation of even this has made it possible for other writers, particularly Stekel, to exploit it in a reactionary sense. His work, which is incorporated in half a dozen essays[2] deserves, however, to be carefully read by any one seriously interested in the problems of symbolism, and a short abstract of it will be attempted here.

In his first contribution Silberer already set forth the two main points in his work, both of which he later expanded in great detail; one relates to the conditions favourable to the production of symbolism, the other to the distinction between different types of symbolism. As will be seen, he uses the term in a much wider sense than that given it in the two preceding sections of this paper. His starting-point was the personal observation that, when he was endeavouring to think out a difficult problem in a state of fatigue or drowsiness, a visual picture appeared which, on analysis, was soon seen to be a pictorial representation of the ideas in question. To this he gave the perhaps not very appropriate term of "auto-symbolic phenomenon." This itself he divided into three

[1] The example is taken from Jung's *Collected Papers on Analytical Psychology*, Second Ed. 1917, 219, 220.

[2] Silberer, "Bericht über eine Methode, gewisse symbolische Halluzinations-Erscheinungen hervorzurufen und zu beobachten," *Jahrbuch der Psychoanalyse*, 1909, I. 513; "Von den Kategorien der Symbolik," *Zentralblatt für Psychoanalyse*, II. 177; "Phantasie und Mythos," *Jahrb. d. Psychoanal.* II. 541; "Symbolik des Erwachens und Schwellensymbolik überhaupt," *ibid.* III. 621; "Über die Symbolbildung," *ibid.* 661; "Zur Symbolbildung," *ibid.* IV. 607.

classes, according to the content of what is symbolised: (1) 'Functional phenomena,' representing *the way in which* the mind is functioning (quickly, slowly, lightly, heavily, cheerfully, carelessly, successfully, fruitlessly, strainedly, etc.), (2) 'Material phenomena,' symbolising *what* the mind is thinking, *i.e.* ideas, (3) 'Somatic phenomena,' symbolising bodily sensations. Silberer[1] emphatically denies any genetic difference between the three classes; in my opinion, this is an important error which becomes later the source of many misunderstandings. He holds, further[2], that the functional symbolism never occurs alone, but only as an accompaniment of the others.

We will next follow Silberer's development of the first question, concerning the conditions under which symbolism arises. The first situation he studied was where there was an equal-sided conflict between the desire to go to sleep and some factor disturbing this, either mental (effort to work, etc.) or physical. It will be noticed that this differs from the psychical situation which, according to Freud, is responsible for dreams merely in that in the latter case the desire is to continue sleeping; in both cases it is desire for sleep versus some disturbance. He soon described the conditions in wider terms[3], the conflict being between the effort towards apperception of any idea on the one side and any factor that made this difficult on the other; the latter factor may be either temporary, such as sleepiness, fatigue, illness, and so on, or more permanent, such as relative intellectual incapacity in comparison with the complexity of the idea. In his most elaborate analysis of the psychical situation he formulated the following factors[4]. Symbolism tends to arise either when one's mental capacity is *no longer* equal to grasping a set of ideas that one formerly could, the result of fatigue, illness, etc., or else when the mental capacity of the individual or of the race is *not yet* able to grasp an idea which some day in the future it will. In both cases it will be possible on some other occasion to recognise that the symbolism is either a regression to or a non-emergence from an inferior and more primitive mode of thought, more primitive both in being sensorial instead of conceptual and in being associative instead of apperceptive (in Wundt's terminology). Now the factors concerned in symbolism can be divided into two groups: (1) what Silberer calls the *positive factors*, those tending to bring a given idea into consciousness, or to keep it there; and (2) the

[1] Silberer, *op. cit.* I. 515.
[2] *Ibid. op. cit.* II. 558; III. 688; IV. 610.
[3] *Ibid. op. cit.* II. 612; III. 676.
[4] *Ibid. op. cit.* III. 683, 684, 717; IV. 608, 611.

negative factors that prevent it from entering consciousness in an apperceptive form and only allow it to enter in a sensorial form, *i.e.* as symbolism.

Silberer derives the energy of the positive factors from two sources: in the first place from the affect investing the idea in question, *i.e.* from the dynamic forward-moving tendency of the mental process itself, and, in the second place, from the conscious wish to think in this particular direction. He writes (of the positive factor)[1]: "It either makes the necessary claim on my attention on its own account, through the affect it brings with it, or I grant it this claim by using my will-power to select and hold to a thought which in itself is of no interest to my feelings, and so deliberately recommend it to my attention as an interesting matter." This division is simply the psychologist's distinction between passive and active attention. To the psycho-analyst the difference is that in the former case the interest (to the ego) is inherent and direct, whereas in the latter case it is due to an indirect association.

The negative factors he also divides into two classes, both of which result in a state of relative apperceptive insufficiency (see quotation in Section III). They are (1) intellectual in kind, either imperfect development (individual or racial) of mental capacity, or a transitory weakening of the apperceptive function through a general diminution of mental energy (sleep, fatigue); (2) affective, which either hinder the entrance of the idea by means of the pleasure-pain mechanism (repression) or allow autonomous complexes to rob the function of attention of a part of its energy and so lead to a general diminution of the apperceptive capacity. The affects thus have both a specific and a general effect as negative factors. In addition they often also act positively, for they themselves may force their way into consciousness, in symbolic guise, instead of the other ideas they have just inhibited. It is clear that in this last point Silberer is referring to repressing forces, to the inhibiting affects that go to make up Freud's 'censor,' and we shall see that it is to this aspect of the conflict that he devotes most attention. His attitude to Freud's conception of repression and censorship is indicated by his remark that the resistance shown in dream analysis is the reverse side (*Kehrseite*) of the apperceptive insufficiency[2].

Silberer recognises that the apperceptive weakness can never be the determining cause of any specific symbol[3], and was thus led to formulate the statements above quoted regarding the 'positive factor,' *i.e.* the determining cause. Nevertheless, his predominant interest is with the

[1] Silberer, *op. cit.* IV. 611. [2] *Ibid. op. cit.* III. 682. [3] *Ibid. loc. cit.* 678.

other side of the subject—namely, with the general conditions that predispose to symbolism. He is chiefly concerned with the factors that *allow* symbolism to occur more readily, rather than with the operative factors that actually bring it about; just as most psychologists deal with the factors that favour the process of forgetting, not with those that actually make us forget. So when he comes to define the different kinds of processes grouped under the name symbolism—the task attempted in this paper—it is from this side alone (of general predisposition) that he attacks the problem. Speaking of the manifold causes of apperceptive insufficiency, he says[1]: "It is here we really have the key for a unitary conception of all the kinds of symbol-formation[2] that are to be found. For the essential differences in the different phenomena of symbolism do not seem to me to reside in the process itself; *i.e.* although these phenomena fall into groups, the differences are secondary manifestations in them which do not concern the symbol-building as such. On the contrary, the differences reside primarily in the factors that bring about the apperceptive insufficiency." The classification effected on this basis will be considered presently.

We have next to pursue the development of Silberer's ideas on the nature of the different forms of symbolism, as distinguished according to its content (see above). To the conception of 'somatic phenomena' he adds nothing further, and I will only remark that it is much more closely allied to that of 'functional' than to that of 'material phenomena.' These latter two groups of phenomena correspond so closely with the groupings of symbols based on another mode of classification that they may be considered together with them. In this second classification Silberer[3] divides symbols, not according to their content, as formerly, but according to the factors that have led to the apperceptive insufficiency which he regards as the fundamental basis of all symbolism. The two classes thus distinguished he calls merely the first and second type respectively, but he makes it fairly plain elsewhere[4] that the material phenomenon is characteristic of the former and the functional of the latter. The first type is that which arises on the basis of an apperceptive insufficiency of purely intellectual origin, where the symbolised idea is not hindered by the influence of any affective complex; the second type

[1] Silberer, *loc. cit.* 683.

[2] The significance of this passage is heightened by the fact that the author is here using the word symbolism in almost the same comprehensive sense in which the term 'indirect representation' is used in this paper.

[3] *Ibid. op. cit.* III. 688; IV. 609.

[4] *Ibid. op. cit.* III. 717.

arises, on the other hand, on the basis of an apperceptive insufficiency of affective origin. So the classification founded on the content (though not the nature) of the positive factors[1] comes to very much the same result as that founded on the variety of the negative or predisposing factors, and we may use the terms 'material' and 'functional' to denote the two types respectively.

We saw above that Silberer's first conception of *functional symbolism* was that it represented the way in which the mind was working (slowly, quickly, etc.). In my experience, and, I may say, also in that of Professor Freud's (oral communication), this is a very exceptional occurrence, and one that probably indicates a specially philosophic and introspective type of mind, such as Silberer's own (from which most of his examples are taken). Further, I am more than doubtful whether the functioning of the mind is ever pictorially represented apart from the occasions on which the mind actually feels or thinks of this functioning. In fact, I think this can be shown to be so in the case of an interesting sub-variety of functional symbolism to which Silberer has given the name of "threshold-symbolism" (*Schwellensymbolik*)[2], where the passage from one state of consciousness to another, *e.g.* into or out of sleep, is indicated by appropriate imagery.

However this may be, Silberer soon enlarged the conception of functional symbolism in a quite surprising manner. He began by regarding the process of 'repression' as a mode of mental functioning and coined for the pictorial representation of it the term 'cryptogenic symbolism[3].' He then extended the conception to include practically all functions of the mind except the ideational, and to refer especially to all affective processes[4]. Here it is no longer a question of the *way in which* the mind is working, but of *what* is working in the mind. According to him, therefore, the greater the extent to which affective moments are in play in the production of a given symbol the more definitely does this belong to the second type of symbolism, characterised by the 'functional phenomenon.' This view is also in harmony with the very interesting remarks he makes on the relation of functional symbolism to gesture language, mimicry[5], etc., for, of course, the latter is simply an expression of the emotions.

If now we recall the strict sense of the word symbol, as used in the

[1] For the meaning of these terms see above.
[2] Silberer, *op. cit.* III. 621–660.
[3] *Ibid. op. cit.* II. 580, 581.
[4] *Ibid. op. cit.* III. 698, 717, 719.
[5] *Ibid. op. cit.* II. 547, 549; III. 690.

previous section of this paper, it is evident that a symbol of that kind represents not only the idea symbolised but also the affects relating to it, or, at all events, some of these. It does this in the same way as the simile indicates an adjectival attribute—namely, by likening the object in question to another one that obviously possesses this attribute, except that in the case of symbolism the one idea is altogether replaced by the other. The affective attitude in this way indicated may be either a positive or a negative one, *i.e.* it may be either unconscious or conscious, the primary attitude or that resulting from repression. An example of the latter would be the well-known serpent symbol. This symbolises at the same time the phallus itself by means of the objective attributes common to both (shape, erectibility, habits—of emitting poison and of creeping into holes, etc.) and a subjective attitude towards it, compounded of fear, horror and disgust, that may in certain circumstances be present, *e.g.* when the subject is a prudish virgin and the object belongs to a distasteful person[1]. Now Silberer would call the two things here symbolised material and functional phenomena respectively, and he considers that psycho-analysts pay too much attention to the former to the relative exclusion of the latter. The explanation of this, however, is that in the interpretation of such symbols psycho-analysts are at the moment chiefly concerned with the positive meaning, the negative aspects being dealt with in another connection (resistance, repression, etc.). The noteworthy point here is that Silberer takes into consideration almost exclusively the negative or secondary affects, so that as a matter of practice the term functional symbolism comes to be almost synonymous with the psycho-analytical 'censor,' *i.e.* the inhibiting affects, or, at most, the positive affects that have been *modified* by the censor. For Silberer, therefore, a psycho-analytical symbol is composed of a material phenomenon (idea symbolised) and a functional one (reactionary affects), both of which are usually conscious processes or nearly so, and he tends to leave out of account the real reason for the whole symbolism—namely, the unconscious, positive affects that are not allowed to appear in consciousness. His overlooking of this essential aspect of the problem accounts also for his curious statement[2] that the universality, or general validity and intelligibility, of a symbol varies inversely with the part played in its causation by affective factors, for it is just these symbols that are most characteristically universal. Relative unfamiliarity with

[1] The positive affects of the complex are obviously also represented, else there would be no such thing as serpent worship.

[2] Silberer, *op. cit.* III. 689, 690; IV. 614.

the unconscious itself has here led him grossly to underestimate the extent to which primitive affective trends are generic, though, it is true, he does verbally admit this in a limited degree[1].

It is probably also this unfamiliarity, or lack of conviction, which leads Silberer to say that 'material' symbols can change into 'functional' ones, a matter which is worthy of special attention since examination of it will, I think, reveal the essential differences between true symbolism and metaphor. He writes[2]: "Recent psycho-analytic investigations have shown that symbols which originally were material become used in a functional sense. If one analyses someone's dreams for a long time one finds that certain symbols, which perhaps at first made only an occasional appearance to denote the content of some idea or wish, keep recurring and so become a standing or typical figure. And the more established and pronounced a typical figure of this sort becomes the more do they recede from the original ephemeral signification, the more do they become the symbolic representative of a whole group of similar experiences, of, so to speak, a mental chapter; until finally one may regard them as simply the representatives of a mental tendency (love, hate, tendency to frivolity, to cruelty, to apprehensiveness, etc.). What has happened there is a transition from the material to the functional by means of what I call an internal intensification." This conclusion is, in my opinion, a fallacious interpretation of a correct observation. The observation is that after a patient has discovered the meaning of a (true) symbol he often strives to weaken and explain away the significance of this by trying to give it some other, 'functional,' more general (and therefore more harmless) interpretation. These abstract and metaphorical interpretations do, it is true, bear a certain relationship to the fundamental meaning of the symbol, one which we shall have to examine presently, but the patient's strong preference for them is merely a manifestation of his resistance against accepting the deeper meaning, against assimilating the unconscious. (This very resistance to the unconscious is shown in Silberer's use of the word 'ephemeral' in the passage just quoted, for if there is any truth at all in psycho-analysis, or, indeed, in any genetic psychology, then the primordial complexes displayed in symbolism must be the permanent sources of mental life and the very reverse of mere figures of speech.) Some patients become exceedingly adept at this method of protecting themselves from realisation of their unconscious: when they interpret their dreams every boat-

[1] Silberer, *op. cit.* III. 690.

[2] *Ibid. Probleme der Mystik und ihrer Symbolik*, 1914, 153.

race becomes the ambition to succeed on the river of life, the money they spill on the floor is a 'symbol' of wealth, the revolvers that are fired in front of women and behind men are 'symbols' of power, and, finally, even openly erotic dreams are de-sexualised into poetic allegories[1]. If, now, the psycho-analyst allows himself to be deluded by these defensive 'interpretations,' and refrains from overcoming the patient's resistances, he will assuredly never reach a knowledge of his unconscious, still less will he be in a position to appraise the relative importance of unconscious trends and those of the surface. By this I do not in any sense mean that the latter are to be neglected, or in their turn under-estimated, but simply that one should not put the cart before the horse and talk of something secondary and less important being *symbolised* by something primary and more important.

Throughout his later work Silberer implies that the process just discussed, of material symbolism changing into functional, occurs not merely during the course of a psycho-analysis, but spontaneously as part of the development both of the individual and of the race. What I should call a *levelling* of this sort does, it is true, go on, but the all-important point is that it does so only in the more conscious layers of the mind, so that to describe the process of symbolism in terms of it represents only a very partial truth. The order of events is rather as follows. The ideas or mental attitudes unconsciously represented in true symbols yield, of course, as the result of repression a great many other manifestations besides symbolism. These may be either positive in kind, as the result of sublimation and other modifications, or negative, such as reaction-formations. They, like symbolism, are conscious substitutes for, and products of, unconscious mental processes. From this consideration it is intelligible that many of these other conscious products stand in an associative connection with various symbols, both being derived from the same sources. But the connection is collateral, not lineal; to speak of one conscious idea symbolising another one, as the post-psycho-analytical school does, is very much like talking of a person inheriting ancestral traits from his cousin. It is true that a given symbol can be used to represent or indicate (for reasons of convenience, vividness, etc.) a collateral mental attitude derived from the same source; this is, in fact, the chief way in which secondary, metaphorical meanings get attached to symbols. But just in so far as this takes place the further removed is the process from symbolism. Very commonly a combination is found, so that the figure in question is partly symbolical, *i.e.* it represents un-

[1] See in this connection Jung, *op. cit.* 221.

conscious mental attitudes and ideas, and partly metaphorical, *i.e.* it indicates other collateral ideas. In some uses the symbolical meaning may be entirely absent, which is what I imply by the word levelling; what Silberer, however, calls the passing of material symbolism over into functional I should prefer to describe as the replacement of symbolism by metaphor (*i.e.* by an associative connection between collaterals), and the difference is a great deal more than one of words. Further, far more often than might be imagined the symbolical meaning is present at the same time as the metaphorical, though from the nature of things it is much more likely to be overlooked, or discounted. This is very striking in the case of everyday superstitions, such as, for instance, that about the spilling of salt, where in addition to or in place of the current secondary interpretations the unconscious sexual symbolism that constitutes the basis of most superstitions can be shown to be actively operative in an astonishing number of those addicted to the superstition in question.

These last considerations may now be summarised in more general terms. To begin with, a concrete idea is symbolised by being represented by another concrete idea that usually has a double relationship to it: (1) an objective one, in that the object or process possesses material attributes similar to those possessed by the idea symbolised, and (2) a subjective one, in that the mental attitude towards it is similar, in some respects, to that towards the primary idea. The symbol later becomes secondarily connected, in an associative manner, with other mental attitudes derived from the same source, and is often used to indicate them. With increasing mental development these tend to become more and more general and abstract, for, as the very word implies, all abstract ideas are abstractions of concrete ones, and therefore always ultimately derived from these; so that finally we see a concrete idea, originally used to symbolise a repressed concrete idea, used to express an abstract thought. Hence the common but mistaken view that symbolism in general represents the abstract in terms of the concrete[1]. Silberer, by first extending the term "functional symbolism" from its original sense to cover the concrete representation of affective processes in general, and by then confining it to the cases where these are secondary in nature, recedes from the conception of true symbolism and reaches once more the popular conception of symbolism as the presentation of the abstract in terms of the concrete.

It is now time to illustrate these points by actual examples, and we may begin by the one last mentioned, that of the serpent. This is one

[1] *e.g.* Silberer, *op. cit.* III. 662.

of the most constant symbols of the phallus[1], and from experiences and thoughts in connection with this object the general conception of 'sexuality' is largely derived. According to the Jung-Silberer school the image of a serpent in a dream[2] will symbolise the abstract idea of sexuality more often than the concrete idea of the phallus, whereas to the psycho-analytical school it only *symbolises* the latter, though of course it is commonly *associated with* the former; the practical difference this makes is that, according to the latter school, any meaning of the dream context which is expressed in terms of the general idea is secondary to, derived from, and dependent on a deeper meaning in the unconscious which can only be expressed in terms of the concrete. Again, the unconscious assimilates the general idea of knowledge in terms of the more specific idea of sexual knowledge, which in its turn is assimilated as sexual power; the association is indicated in the Biblical phrase "to know a woman." For this reason the idea of the serpent has become associated, especially in the East, with that of knowledge, so that it commonly serves as an emblem of wisdom (as do so many other sexual symbols, *e.g.* salt). But to say that a serpent may 'symbolise' *either* a phallus *or* wisdom is to confound two entirely different psychological processes. The relation between them might be further illustrated by comparing these two situations: (1) the case of a man who casually makes use of the colloquial expression 'he is a wily old snake'; here it may well be that the metaphor is purely external, being based on his having heard or read that there is some supposed association between snake and cunning; (2) that of a man who personally and instinctively *feels* that the snake is a fit, natural, and intelligible emblem for the ideas of wisdom and cunning; here one would certainly expect to find that the idea is acting as a true, unconscious, phallic symbol.

A wedding ring is an emblem of marriage, but it is not a symbol of it. When a man woos a woman he instinctively makes her a present of objects, such as bracelets, brooches, and later an engagement ring, that have the attribute of holding what is passed through them, and unconsciously are symbols of the female organ. At marriage he gives her one of the most perfect symbols of this kind, a plain gold ring, in return for the complete surrender to him of the object it symbolises. The ceremony connotes a group of abstract ideas, fidelity, continuity, etc.,

[1] Very occasionally it can also symbolise the intestines, or their contents, but, so far as I know, nothing else.

[2] I am speaking of cases where the dream image is a symbolic one, which, of course, it need not be.

with which the ring is now brought into association, and for which it can then serve as an emblem, though never as a symbol.

Most charms, talismans, and amulets are genital symbols, predominantly male. Just as they now bring good luck, or ward off bad luck, so in earlier ages they guarded against the evil powers of magical influences. That these apotropaeic qualities were almost exclusively ascribed to genital symbols is due to two circumstances, first the exaggerated association in the primitive mind between the genital organs and the idea of power or potency, and secondly the fact that originally nearly all evil magical influences were imgained to be directed against the sexual organs and their functions. As I have shown elsewhere[1], for example, practically all the dreaded evil actions of witches in the middle ages were symbolic representations of the 'ligature,' *i.e.* of the attempt to injure sexual potency; they were, in short, castration symbols. The surest safeguard against this calamity was the demonstration, by assertion, that the threatened part was safe; the mechanism is similar to that of the talion. This train of thought naturally led to charms being associated with the idea of safety in general, particularly as a protection against death or mutilation, as is pathetically shown on a large scale in the present war. Anxious relatives who press a horseshoe or a 'fums up' on their man when he leaves for the front have not the faintest idea of the meaning of their superstitious act, but that this meaning is not simply a historical one can often be shown by analysis of their dreams, where the true symbolism becomes apparent; the unconscious often knows what the person is doing so much better than the conscious mind.

To take another current, and more important analogy. Modern economists know that the idea of wealth means simply 'a lien on future labour,' and that any counters on earth could be used as a convenient emblem for it just as well as a 'gold standard.' Metal coins, however, and most of all gold, are unconscious symbols for excrement, the material from which most of our sense of possession, in infantile times, was derived. The ideas of possession and wealth, therefore obstinately adhere to the idea of 'money' and gold for definite psychological reasons, and people simply will not give up the 'economist's fallacy' of confounding money with wealth. This superstitious attitude will cost England in particular many sacrifices after the war, when efforts will probably be made at all costs to reintroduce a gold currency.

[1] Ernest Jones, *Der Alptraum in seiner Beziehung zu gewissen Formen des mittelalterlichen Aberglaubens*, 1912, 106–110.

We incidentally referred above to the association between the phallus and the idea of power. This is especially close in the case of that of the father, for whom, as was explained above, the idea of the king is an unconscious symbol. His special symbol, the sceptre, thus comes to be the emblem of regal authority, *i.e.* for the pious respect due to the father. This mental attitude originates, at least in its extreme forms, largely as a reaction against the more primitive and instinctive jealousy and hatred of the father, part of the famous Oedipus-complex[1]. This primitive attitude is expressed in the unconscious of practically all men as the desire to kill, or at least to castrate, the father, a desire that doubtless was literally gratified in primeval times[2]. The mind now recoils from such a horrific conception, and in connection with it we have two beautiful examples of how it deals with this type of truth by diluting its meaning, by changing material symbolism into the harmless functional kind. According to the Jung-Silberer school, the unconscious wish to kill the father merely 'symbolises' such tendencies as the desire to overcome the old Adam in us, to conquer the part of us that we have inherited from the father, or, even more generally, to overcome a previous point of view. As might have been expected, the same ideas of father-murder or father-castration frequently occur in mythology and the older religions—if not in all religions—and mythologists have similarly deprived them of any literal meaning by interpreting them as harmless and interesting representations of such natural phenomena as the phases of the sun and moon, vegetative or seasonal changes, and so on.

Freud[3] has shown what an essential part this murder impulse has played in the development of religion, not only in primitive systems such as the totemistic, but also in the higher forms, and it is probable that the phallic worship which takes such a central place in earlier religions—and is far from absent in those of our own time—is derived, not only from the extraordinary over-estimation (from our point of view) of the importance of sexual functions characteristic of the primitive mind, but also as a reaction against the hostility toward the patriarchal phallus, and therefore also the divine one; in consciousness adoration for the patriarchal phallus becomes over-emphasised just because in the repressed unconscious there is the contrary attitude of hostility. Phallic worship, therefore, was determined by more than one cause, but it was funda-

[1] For an exposition of this see Freud, *Traumdeutung*, 1914, 192–201; Rank, *Das Inzest-Motiv in Dichtung und Saga*, 1912; Ernest Jones, "The Oedipus Complex as an Explanation of Hamlet's Mystery," *Amer. Journ. of Psychology*, XXI.

[2] See Darwin, *The Descent of Man*, 1871, Ch. xx.

[3] Freud, *Totem und Tabu*, 1913.

mentally concerned with a real phallus. When the facts of Eastern phallic religions began to reach Europe in the nineteenth century they seemed so incredible that they had at all costs to be re-interpreted into harmless terms, and the view, still prevalent, was adopted that the worship had nothing to do with the phallus as such, but was really directed towards the abstract idea of the divine creative power, which we personify as the Creator, and for which the phallus was a 'symbol' appropriate to simple minds. Reflection shows that the abstract idea in question must itself have been derived from the concrete idea symbolised by the phallic image, so that we have here one more instance of confusion between descendence and collateralism; according to the view just mentioned, the order of development was first concrete phallus, then abstract idea of generation (in so far as it would be admitted that this idea came from the former), then symbol of the abstract idea, whereas to the psycho-analyst the abstract idea and the symbol are related to each other, not as cause and effect, but only as proceeding from a common cause. Indeed, from the standpoint of scientific thought, the abstract idea that is here supposed to be symbolised is altogether illusory; we have no experience, in either the physical or spiritual world, of creation, for what masquerades as such always proves on closer inspection to be only transformation[1]. Yet so hard is it for the human mind to rid itself of such fundamental illusions that the necessity of postulating a creative force is one of the chief arguments adduced in favour of a belief in theism, and even relatively sceptical thinkers like Herbert Spencer feel obliged to fall back on the conception of a 'First Cause.'

We have so far considered the symbol in its relation to the idea unconsciously symbolised, and have reached the conclusion that in the psycho-analytical sense the symbol is a substitute for the primary idea, compulsorily formed as a compromise between the tendency of the unconscious complex and the inhibiting factors, whereas the functional interpretation is mainly concerned with the more conscious reactions to and sublimations of the unconscious complex. We have next to deal with another aspect of the problem—namely, the relation of the symbol to the idea it immediately expresses, *e.g.* no longer with the relation of the serpent symbol to the phallus, but with that of the serpent symbol to the serpent itself. We have, in other words, to consider symbolism in terms of the reality-principle, instead of, as before, in terms of the pleasure-principle.

[1] The whole question is pithily condensed in the expression "The wish is *Father* to the thought."

In dreams, myths, and similar material, we find the image of the sun used to symbolise the eye, the father, or the phallus. What bearing has this symbolism on man's conscious thoughts concerning the sun in other respects? The problem divides itself into two—namely, the question of more or less scientific knowledge concerning the sun, dictated to some extent by man's primary instinct for knowledge, and secondly the more practical aspects of how to deal in daily life with the external phenomena in question (heat, shade, darkness, etc.). It is only in civilised man that this distinction holds, and even there only in part, for it is everywhere hard to separate the mere curiosity for knowledge from the practical aspects of the necessity for, or desirability of, knowing. I feel sure that a great deal of what is attributed to man's pure desire for knowledge, the discoveries he makes, and so on, are really dictated much more by the impulses set up by necessity, which may be either external or internal; how well the old adage "necessity is the mother of invention" is being illustrated at the present day!

Our problem is especially manifest in regard to what Wundt terms the "mythological stage of knowledge." This does not here involve the problem of mythology as a whole, which has more to do in general with the material *versus* functional controversy dealt with above, as Silberer[1] has well illustrated in a number of familiar examples. As he has also well expounded[2], a most important point to bear in mind in regard to the mythological stage of knowledge is that it is a relative concept. No knowledge is recognised to be mythological by the person who believes in it, at least not at the moment he does so believe. This, however, is also true of symbolism. It is only when we disbelieve in their objective and literal reality that we recognise them to be symbols, though even then we usually have no idea of what they had been symbolising. So a mythological piece of knowledge is at the time it is accepted, and for those who accept it, the only form of truth then possible; it is an adequate form of reality for a certain level of development. A 'higher' or more objective form of truth would be rejected, for either intellectual or affective reasons, and 'not understood.' Silberer[3] thinks that on the whole the first type of symbolism, the material phenomenon, predominates in this process. Taking the idea of symbolism in its strict sense, there is no doubt that, as both Silberer[4] and Rank and Sachs[5] point out, its

[1] Silberer, *op. cit.* II. 573–586.

[2] *Ibid. op. cit.* II. 606, 607; III. 662–666.

[3] *Ibid. op. cit.* III. 689.

[4] *Ibid. op. cit.* III. 692.

[5] Rank and Sachs, *op. cit.* 17.

occurrence in this connection serves the function of rendering it easier to assimilate the perceived material that is being dealt with; the mind assimilates it in terms of the previously familiar. What really happens is that the unconscious assimilates the new material in terms of its own thoughts, the process discussed in Section III of this paper, the result of which may be the appearance in consciousness of a symbol of the unconscious thought.

So far all is clear, but the point that is disputed in this connection is whether the symbol can bear any relation, and if so what, to the idea (the 'higher form of truth') that will later, in either the same individual or another, replace the symbol and this mythological stage of knowledge. Can the later, more objective form of knowledge be already implicit in the earlier symbolical presentation of the attempt to deal with the problem? Silberer does not definitely answer this question, but Jung[1] would unhesitatingly answer it in the affirmative, and, I think, in all cases.

To my way of thinking the matter is more complex than would appear from this statement of it. There is certainly some connection in most cases between the symbol and the 'future idea,' but, in my opinion, it is very much the same as, though not quite identical with, the connection discussed above between the symbol and the functional interpretation. I do not think that the future idea is implicit in the symbol; on the contrary, the existence of the symbol—to be more accurate, the symbolic use of the symbol—is often the very thing that is preventing the idea from being formulated. As has been explained above, the mind always tends to assimilate a new percept in terms of some unconscious complex, and every step in progress in the line of the reality-principle connotes, not only a use of this primordial association, but also a partial renunciation of it; a surrendering of the personal, subjective factor and an attending, which might almost be called sensorial, to the objective attributes of the new percept. Let us follow the example chosen above of the sun. One of the earliest conceptions of this was that it was a mighty eye, the resemblances—in connection with light, etc.—being fairly evident. Later it was regarded as a moveable lamp, and later still as a hot gaseous body around which the earth revolves. If in one of these later stages of knowledge the image of the sun appeared in a dream as a substitute for that of an eye we should, of course, call it a symbol, but in the first stage the ophthalmic idea of the sun would most accurately be described as a symbolic equivalent. Now how did the progress in knowledge take place, and what is the relation of the symbol to the

[1] See especially Jung, *op. cit.* Ch. xv.

future idea of the sun? The first stage is simple enough. It is nothing but an identification of the new percept with an old one, a temporarily successful assimilation of it in terms of the old and more familiar one. I imagine that every fresh attribute observed about the sun and its behaviour, every fresh thought about it, was in turn dictated by a similar association, usually unconscious, with some previously familiar idea; or, put in another way, that attention was seriously directed to each fresh attribute through the interest already residing in the previously familiar idea with which the new attribute got associated on the ground of however faint a resemblance, for it is truly astounding how the human mind can escape paying attention to evident, and even important, observations in which it is *not* interested. But, and this is the all-important point, in this second stage the assimilation does not lead to pure symbolism; it is enough to direct attention, and give interest, to the fresh observation, but this is interpreted by a process of ratiocination in conjunction with the facts of external reality, no longer solely in terms of the pre-existing idea, as in the first, more symbolical stage of knowledge. In so far as it is no longer thus interpreted in the older fashion there is involved a corresponding renunciation, in favour of the reality-principle and its advantages, of the pleasure yielded by the easier and more primitive process of complete assimilation. According to the findings of psycho-analysis, *all* mental progress is accompanied with partial renunciation of some primitive form of pleasure—which is probably the reason why it is so slow—and the process just indicated is no exception to the rule.

The following example also illustrates the same point. Lightning, like mistletoe, was at first, and for thousands of years, imagined to be divine soma[1], *i.e.* semen, a notion the last form of which was the conception of a special magnetic or electric fluid; it is interesting, by the way, that the same conception—here termed magnetic fluid, vital fluid, mesmeric fluid, etc.—was long held as the theory of what used to be called 'animal magnetism,' *i.e.* hypnotism. Increased knowledge as to the nature of lightning essentially connoted, among other things, the partial surrendering of this unconscious assimilation, the giving up of the symbol magnetic fluid, though in the unconscious symbolism that is the basis of neurotic symptoms, *e.g.* brontephobia, the ancient association between lightning and semen recurs, and it is to be noted that we still popularly conceive of electricity as the flow of a current. Our general

[1] See Kuhn, *Die Herabkunft des Feuers*, 1859; and the comments on it in Abraham's *Traum und Mythus*, 1909.

question, therefore, of whether the future conception is already implicit in a latent state in the symbol, can be answered affirmatively only in a very restricted sense—namely, that part, and often only a small part, of the mental material that will later be converted into the more developed conception is already present, but that the idea as such is certainly not present, even in the unconscious, so that obviously it cannot be 'symbolised.'

Similar remarks hold good in the case of more complex stages in the advance of knowledge, such as scientific generalisations, as also with other conscious tendencies and interests. From one point of view these may be regarded as sublimations from unconscious complexes, developments which are, of course, greatly modified by contact with external reality and by conscious elaboration. They, like symbols, come about as the result of the conflict between unconscious impulses and the inhibiting forces of repression, but they differ from symbols in that, whereas with the latter the full significance of the original complex is retained unaltered and merely transferred on to a secondary idea (that of the symbol), with the former the psychical energy alone, not the significance, is derived from the unconscious complexes and is transferred on to another set of ideas that have their own independent significance. It is true that here also regression may lead to true symbolism, where the ideas resulting from sublimation may temporarily lose their own intrinsic meaning and sink back to become mere symbols of the complexes from which their energy was largely derived. But in this case they are symbols in the strict sense and do not symbolise the sublimations, in spite of their indirect association with these. A typical example of the whole process would be the one discussed above in connection with Sperber's views, the case of agricultural work. At first these were identified with sexual acts and later achieved an independence of their own, but in neither of these stages could they be called sexual symbols, for they were not being used as pure substitutes; they become symbols only when, as in dreams, myths, etc., they for a time lose their actual meaning (wholly or in part) and are then used as substitutes for the ideas with which they were originally identified.

We have now considered three aspects of symbolism: its relation to the unconscious complex (Sections II and III); to the other derivatives of this (functional symbolism); and to external reality. We have last of all to consider briefly a fourth aspect, that to which Silberer has given the name 'anagogic[1],' and which is very similar indeed to Adler's 'pro-

[1] Silberer, *op. cit. Probleme*, etc. 138.

grammatic' and Jung's 'prospective' meaning of symbolism[1]. The last two terms are wider ones and include the 'development of the future idea' conception just discussed as well as the anagogic one; we are here concerned, therefore, only with the latter one.

By the anagogic signification of symbolism is meant the mystical, hermetic, or religious doctrine that is supposed to be contained in the symbol. The symbol is taken to be the expression of a striving, more or less conscious, for a high ethical ideal, one which fails to reach this ideal and halts at the symbol instead; the ultimate ideal, however, is supposed to be implicit in the symbol and to be symbolised by it. Along this path the post-psycho-analytical school[2] loses itself in a perfect maze of mysticism, occultism, and theosophy into which I do not propose to penetrate; Silberer implicitly, and Jung explicitly, abandon the methods and canons of science, particularly the conceptions of causality and determinism, so that I may consider myself absolved from the task of attempting to unravel the fallacies that have culminated in their present views. As the philosophers would say; it is impossible for us to adhere to one universe of discourse.

It is clear that the anagogic aspect of symbolism is only a special case of the general 'future idea' conception discussed above, and that the relation between the symbol and the ethical ideals in question is much the same as that already explained as subsisting between it and the various functional aspects, particularly those referring to sublimated interests and activities. In fact, the only difference that Silberer[3] discerns between the anagogic and functional aspects is that the former refer to future mental attitudes and the latter to present ones; when the anagogic ideal has been attained it passes into functional symbolism[4], a conclusion that confirms my previously expressed suspicion as to the reactionary tendency of his general conception of functional symbolism.

V. Review of Conclusions.

The main thesis of this paper is that it is possible usefully to distinguish, under the name of symbolism, one fundamental type of indirect representation from other more or less closely allied ones, and that consideration of the points of distinction throws a light upon the nature

[1] Silberer, *loc. cit.* 193, 207.

[2] See especially Jung, *op. cit.* and *The Principles of the Unconscious,* 1916; Silberer, *op. cit. Probleme*, etc.

[3] Silberer, *op. cit. Probleme,* etc. 155.

[4] *Ibid. loc. cit.* 194.

of indirect figurative representation in general, and of symbolism in particular.

Using first the term symbolism in its older broad sense (to include metaphors, etc.) we can make the following generalisations. All symbolism betokens a relative incapacity for either apprehension or presentation[1], primarily the former; this may be either affective or intellectual in origin, the first of these two factors being by far the more important. As a result of this relative incapacity the mind reverts to a simpler type of mental process, and the greater the incapacity the more primitive is the type of mental process reverted to. Hence in the most typical forms the symbol is of the kind of mental process that costs least effort, *i.e.* is sensorial, usually visual; visual because in retrospect most perceptual memories become converted into visual form (most memories of childhood, etc.), this in turn being partly due to the special ease of visual representation. For the same reason symbolism is always concrete, because concrete mental processes are both easier and more primitive than any other. Most forms of symbolism, therefore, may be described as the automatic substituting of a concrete idea, characteristically in the form of its sensorial image, for another idea which is more or less difficult of access, which may be hidden or even quite unconscious, and which has one or more attributes in common with the symbolising idea.

The essential difficulty that goes with all forms of symbolism is in the adequate apprehending (and therefore also in the conveying) of feeling. This is doubtless to be ascribed to the innumerable inhibitions of feeling which psycho-analysis has shown to be operative throughout the mind, and which naturally exhibit a more concentrated force in some regions than in others; it is therefore to be expected that the most typical and highly developed forms of symbolism will be found in connection with those regions. Even the weakest form of symbolism, however,—for instance, the metaphor—comes into this category. For example, Keats wishes to convey his exaltation at the sense of discovery experienced on first looking into Chapman's *Homer*. He finds it impossible to do this directly, for any mere direct statement of the fact would leave us cold. He succeeds in transmitting to us some of his own thrill only by likening his sensations to those of someone who just has

[1] This generalisation is about equivalent to that implied in Silberer's term 'apperceptive insufficiency,' but he tends to regard this incapacity as the essential cause of symbolism, while I regard it merely as an indispensable condition. I also lay much more stress on the affective causes of it than he does.

discovered a new planet or a new ocean[1]. The simile used by Keats strictly stands for an adjective—wonderful, inspiring, or what not—preceding the word 'exaltation'—and this is true of all similes and metaphors. The problem thus arises: in what way is the replacement of an adjective by a concrete likeness related to the question of inhibited feeling?

The basal feature in all forms of symbolism is identification. This—one of the most fundamental tendencies of the mind—is much more pronounced in its more primitive regions. The lack of discrimination connoted by it is only in a very slight degree conditioned by imperfect intellectual development, for the tendency to identify is mainly due to the following two factors, which relate to the pleasure-principle and the reality-principle respectively. In the first place, it is easier, and therefore pleasanter, to note the features of a new idea that resemble those of an older and more familiar one. Further, the mind tends to notice especially those features that interest it because of their resemblance to previous experiences of interest. In the second place, the appreciation of resemblances facilitates the assimilation of new experiences by referring the unknown to the already known. Even this factor, and obviously the first one, is much more an affective than an intellectual one. These identifications profoundly influence the course of further mental development along both affective lines (sublimations) and intellectual ones (increased knowledge, science).

In so far as a secondary idea *B* receives its meaning from a primary idea *A*, with which it has been identified, it functions as what may be called a symbolic equivalent of *A*. At this stage, however, it does not yet constitute a symbol of *A*, not until it replaces *A* as a substitute in a context where *A* would logically appear. There is an overflow of feeling and interest from *A* to *B*, one which gives *B* much of its meaning, so that under appropriate conditions it is possible for *B* to represent *A*. According to the view here maintained, the essential element of these conditions is an affective inhibition relating to *A*. This holds good for all varieties of symbolism, in its broadest sense.

Affective inhibition can, of course, be of the most varying degree, and on this variation greatly depends the multiplicity of the processes that are grouped under the name of symbolism. When the inhibition is at its maximum symbolism arises in its most typical form. The distinctions between this and other forms of indirect pictorial representation are qualitative as well as quantitative, and they are so important that it is

[1] Here, as is often the case, the inhibition of imaginative feeling that has to be overcome is in the hearer.

here proposed that the term symbolism be reserved for them solely[1]. It is already explicitly used in this sense by psycho-analysts, and implicitly by many anthropologists and mythologists, and it seems worth an effort to try to get it generally accepted thus. The two cardinal characteristics of symbolism in this strict sense are (1) that the process is completely unconscious, the word being used in Freud's sense of "incapable of consciousness," not as a synonym for subconscious, and (2) that the affect investing the symbolised idea has not, in so far as the symbolism is concerned, proved capable of that modification in quality denoted by the term sublimation. In both these respects symbolism differs from all other forms of indirect representation.

The typical attributes of *true symbolism*, as modified from the description given by Rank and Sachs, are: (1) Representation of unconscious material; (2) Constant meaning, or very limited scope for variation in meaning; (3) Non-dependence on individual factors only; (4) Evolutionary basis, as regards both the individual and the race; (5) Linguistic connections between the symbol and the idea symbolised; (6) Phylogenetic parallels with the symbolism as found in the individual exist in myths, cults, religions, etc. The number of ideas that can be symbolised is remarkably small in comparison with the endless number of symbols. They are fewer than a hundred, and they all relate to the physical self, members of the immediate family, or the phenomena of birth, love, and death. They typically, and perhaps always, arise as the result of regression from a higher level of meaning to a more primitive one; the actual and 'real' meaning of an idea is temporarily lost, and it is used to represent and carry the meaning of a more primitive idea with which it was once symbolically equivalent. When the meaning of the symbol is disclosed the conscious attitude is characteristically one of surprise, incredulity, and often repugnance.

Progress beyond the early stage of symbolic equivalency takes place (*a*) intellectually, by the transference of the symbolic meaning to the idea *B* becoming subordinated to the acquirement of a 'real,' objective meaning intrinsic in *B*; (*b*) affectively, by a refinement and modification of the affects investing *A* (sublimation), which permits of their becoming

[1] Mr J. C. Flügel has suggested to me that, as an alternative to my proposal, the term 'cryptophor' be used as a counterpart of 'metaphor,' so that one might speak of cryptophoric as contrasted with metaphoric symbolism, instead of, as I propose, speaking of symbolism as contrasted with petaphoric representation. The objection I see to his suggestion is that, if the same word 'symbolism' be still used generically for the two classes (for the qualifying adjective would often be omitted in practice), the current confusion between them would only be perpetuated.

attached to non-inhibited, conscious, and socially useful or acceptable ideas and interests. Both of these processes connote a partial renunciation as regards the original complex A, with, however, a compensatory replacement of it by other ideas and interests. Whenever there is a failure in this process of sublimation there is a tendency to regress towards the primary complex A, or, rather, this complex, being no longer indirectly relieved, once more tends to re-assert itself. Inhibiting forces prevent its doing so in its original form, and as a result of this intrapsychical conflict it may express itself by means of one of its original symbolical equivalents, *e.g.* B, which then carries, in a substitutive manner, the significance of A and is its symbol. Once this has occurred further progress can only take place by the same process as that just described, a loosening of the ideational links between A and B and a renunciation of the need of the complex A for direct gratification. Progress, therefore, in contradistinction from the views held by the post-psycho-analytical school, does not take place *via* symbolism, but *via* the symbolic equivalents that are the basis of this; symbolism itself, in fact, constitutes a barrier to progress. This is best seen in the blind alley of neurotic symptomatology.

The most important member of this school, from the point of view of symbolism, is Silberer, whose views have therefore been dealt with at some length in this paper. The differences between his conclusions and my own may shortly be expressed as follows. We are concerned with three groups of psychical material: (1) the unconscious complexes, (2) the inhibiting influences (Freud's ethical censor) that keep these in a state of repression, and (3) the sublimated tendencies derived from the unconscious complexes. In my judgement, the relation of symbolism to these three groups is this. Like the third group, symbols are the product of intrapsychical conflict between the first two groups. The material of the symbol is taken from the third group. The second group, which prevents the first one from coming to direct expression, is to some extent represented in the formation of the symbol; but the dynamic force that creates the symbol, the meaning carried by the symbol, and the reason for the very existence of the symbol, are all derived from the first group, the unconscious complexes.

The fundamental fallacy of Silberer's work, as it seems to me, is that he tends to confound the process of symbolic equivalency with that of symbolism itself[1], as was indicated above in regard to the relation be-

[1] The same fallacy as that involved in Maeder's confusion of the latent and manifest contents of dreams, and with the same practical result—the attributing of ethical tendencies to a process that has only an indirect relationship with them.

tween symbolism and mental progress. As a result of this he brings symbolism into a forced relationship with the other product of the unconscious, the third group just mentioned, and tends to regard the symbol as the representative of this further product instead of its being the representative of the first, primary group. Further, on the base of the subordinate part played by the second group in the formation of symbols, and the fact that it is to some extent represented in the symbol, he attaches an altogether exaggerated importance to this second group as constituting the meaning of the symbol, and especially to those aspects of the second group (the ethical ones) that are akin to the third group. To put the matter still more concisely: according to the conclusions here reached, the material of a symbol is derived from the third group, while its meaning is derived essentially from the first group, to only a very limited extent from the second, and not at all from the third; according to Silberer, the meaning of a symbol is derived mainly from the second and third groups, and only to a very limited extent from the first.

I agree, however, that a symbolic image may be used to represent the second or third groups of psychical material in question as well as the first, but in this function it is acting as a metaphor, not as a symbol, and it might then be usefully termed an emblem or sign. When this is the case—*i.e.* when a true symbol is being used metaphorically—all that the second or third group of psychical processes can do is to select for its own purposes an already created symbol; it never contributes, to any important extent, to the actual creation of the symbol. Silberer, in my opinion confounds the use of metaphor with that of the symbol and so mistakes the nature of the true symbol, ascribing to it attributes that properly belong to the metaphor. There are many features in common between the two processes—it would be impossible to confound them otherwise and the object of this paper would be superfluous—and I do not for a moment wish to maintain that they are totally different in nature. But the differences between them, notably in their relation to the unconscious (together with the other features of symbolism discussed above) are also important.

There are, broadly speaking, two kinds of metaphor, with all gradations between them. With the first kind an analogy is perceived and made use of between two ideas that is true, objective, and of some value; thus in the phrase "to find the key to this problem" the analogy between such a situation and that of discovering how to enter a room difficult of access is of this nature. With the second kind the analogy is only supposed to subsist, it is subjective and often untrue in fact; thus the phrase "as wise as a serpent" is of this nature. Serpents are, in fact, not wiser than most other animals and the false attribution of wisdom to them is secondary and due to a process of true symbolism, as has been expounded earlier in this paper. With the first kind the association is intrinsic, with the second it is extrinsic, depending, however, on an underlying identity in the source of both ideas (in so far, of course, as they are symbolic).

In a metaphor an abstract adjectival description is replaced by a more concrete simile. Experience shows this to be a more vivid and successful way of conveying the desired meaning and of evoking the appropriate feeling tone. The explanation is that the more primitive method, *i.e.* recourse to the concrete and sensorial, stands nearer to the sources of feeling. In the evolution, in both the individual and the race, from the original concrete to the general, and from this to the abstract, there is an increasing inhibition of feeling accompanying the greater objectivity. Concrete images are as a rule more personal, familiar, subjectively toned, and invested with more feeling than abstract terms. The difference is most plainly seen in the fields where there is most inhibition. There is a considerable difference between damning a man's eyes and merely consigning him to perdition. By the use of suitable abstract circumlocutions, aided by foreign and less familiar technical terms, it is possible to discuss various sexual topics in any society without any difficulty, but—to take the other extreme—the use of some gross obscene word, familiar in childhood, but since discarded, will often bring about a marked uprush of unpleasant emotion.

Therefore when it is wished to apprehend or convey a vivid impression, a strong feeling, recourse is had to the primitive method of likening the idea to an associated concrete image, because in this way some inhibition is overcome and feeling released; what is popularly called stimulating the imagination is always really releasing the imagination from its bonds. The over-profuse use of metaphors, as that of slang—which fulfils the same psychological function—is well known to be the mark of expressional incapacity; the person belongs to what, in association work, is called the predicate type.

Theoretically and logically the simile is the first stage of the metaphor. But, for the motives expounded above in connection with the process of identification, the two sides of the equation become fused into one at the very onset, with a resulting economy in psychical effort. The savage does not say "John is like a lion"—still less does he say "John is as brave as a lion"—he boldly asserts that "John is a lion." And when we cannot find language sufficiently vivid to convey our admiration of John's courage we revert to the primitive method of the savage and say likewise that "John is a lion."

One further point. The process known as the decay of a metaphor, whereby the original literal meaning of the word is lost and its figurative meaning receives an accepted and independent significance, is akin to what was described above as the renunciation of a symbolic meaning, whereby the symbolising idea becomes emancipated from its adventitious meaning and achieves a separate existence.

I will now attempt a final *summary* of these conclusions. The essential function of all forms of symbolism, using the word in the broadest and most popular sense, is to overcome the inhibition that is hindering the free expression of a given feeling-idea, the force derived from this, in its forward urge, being the effective cause of symbolism. It always constitutes a regression to a simpler mode of apprehension. If the regression proceeds only a certain distance, remaining conscious or at most preconscious, the result is metaphorical, or what Silberer calls 'functional,' symbolism. If, owing to the strength of the unconscious complex, it proceeds further, to the level of the unconscious, the result is symbolism in the strict sense. The circumstance that the same image can be employed for both of these functions should not blind us to the important differences between them. Of these the principal one is that with the metaphor the feeling to the expressed is over-sublimated, whereas with symbolism it is under-sublimated; the one is an effort that has attempted something beyond its strength, the other is an effort that is prevented from accomplishing what it would.

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WHY IS THE 'UNCONSCIOUS' UNCONSCIOUS?[1]

I

By MAURICE NICOLL.

A discussion dealing with the nature of the unconscious is inevitably difficult. If we wish to understand the significance of the teaching of Jung upon the nature of the unconscious, it is first necessary to gain some idea of the original teaching of Freud. The original teaching of Freud was that the unconscious part of the human psyche contained only what had once belonged to the conscious personal life. It became unconscious because it was repressed. It was repressed because it was painful, or grossly antagonistic to conventional standards. Thus, the unconscious, from Freud's original standpoint, comes into existence during the life of the individual as a result of repression. It is therefore a secondary product. From this point of view the 'unconscious' is unconscious because of repression, a process peculiar to humanity. It begins at an early period in every human life. At birth there is no unconscious, and at puberty there is an unconscious, and this unconscious is only a repressed part of the person's conscious experience. This repressed portion, according to Freud, is largely the so-called infantile sexuality. We can say that this kind of unconscious is like a cage opening off the main living-room of consciousness into which we put the things that have become dangerous. The main task of life is to keep the door of the cage shut. If the door is not shut properly, we become neurotic or insane. I do not know exactly what the Freudian school believe at the present moment, over and above this original view. I do not think it is possible to find very clear formulations in the modern Freudian writings.

Jung, the leader of the Zurich school, takes another view of the unconscious. His teaching has led to a split with Freud, mainly over the question of sexuality. To Jung the primitive life force, or *libido*, is not sexuality, but an energy one of whose manifestations is sexuality. Jung does not regard the unconscious only as something acquired

[1] Contribution to a Symposium at a Joint Session of the British Psychological Society, the Aristotelian Society and the *Mind* Association, July 6, 1918.

during personal existence through repression. It is also an inheritance—a racial background of the mind. This, for Jung, constitutes the *collective unconscious*, and its contents are inexhaustible; that is, no amount of analysis can exhaust them. Jung brings the matter of the inexhaustibility of the unconscious as an argument against the Freudian view. If the unconscious is merely a certain repressed part of the psychic life of the individual, then it should theoretically be possible to exhaust its contents or do away with it by analysis—that is, by making it conscious. Experience seems to show that this is far from possible, and that the unconscious continues to weave its dreams and phantasies ceaselessly. What Freud calls the unconscious, Jung calls the *personal unconscious*, and this is but an excerpt of the collective unconscious, containing repressed and forgotten material that has an intimate and personal significance. The contents of the collective unconscious are impersonal and are made up of what Jung calls the *primordial thought feelings*. These primordial thought feelings are shared in common by mankind and form the primitive pattern of all thought, which we, according to our mental powers, work up into more or less elaborate systems. Therefore the roots of thought and feeling reach down beyond personal history, beyond the personal unconscious, into racial strata where lie the primordial thought feelings.

In order to understand what is meant by primordial thought feelings I might suggest that we compare them to certain primordial re-actions, such as pleasure and pain. Some qualities seem to be inherent in the human soul, and we cannot fundamentally attribute them to education. The appreciation of beauty and ugliness depends, I think, on a primordial duality in us. Naturally, education will develop these primordial thought feelings in one direction or another. Perhaps all the primordial things have a dual aspect or ambivalency. Certain gestures and expressions must surely be primordial—laughter and rage. Our basic emotions are surely primordial. And then there is the war—were the deeps of the human soul only what Freud has taught, whence comes all this devoted sacrifice?

We have also to consider that we seem to understand more than we have actually experienced, and far more than we can express, but I do not propose to discuss this very difficult thesis. I will only mention that if our capacity for understanding did not transcend our personal conscious experience, the outlook for art, drama and literature would be sterile. Shakespeare could not have existed; or, to put it another way, he would be largely meaningless. All great art lifts us far beyond our conscious selves, but when the spell is over, we lose the vision and wonder at the deeps within us.

Jung quotes the fantasy of a schizo-phrenic patient of Maeder's—a locksmith—who said that the world was his picture-book. The fantasy, or primitive idea, of this uneducated patient is the same primordial thought feeling as underlies the whole system of Schopenhauer's philosophy which conceives the world as Will. The difference between the locksmith and Schopenhauer is one of elaboration and detail. The primordial thought feeling is the same, and exists in us all.

There is one very important thing which the original Freudian view of the unconscious does not fully explain. It is the language of the dream. There is sound evidence to show that the infant experiences dreams before it can speak, and I think most people will agree that animals dream. Dreaming therefore precedes the function of language. Dreaming is pictorial language. It is primitive language—a primitive way of thinking. Jung saw in the dream symbol a primitive and primary representation. Freud thought the dream symbol was not a real representation, but something secondary, the outcome of repression. It was a method of disguise, a process of camouflage, whereby the unpleasant repressed contents of the unconscious could gain admittance into consciousness, by avoiding the endopsychic censor. But in the preface to the third English edition of *The Interpretation of Dreams* Freud says that he has learned to attach a greater value to the significance of symbolism in dreams, "or rather, in the unconscious thinking." From this and from other recent writings it would appear that Freud recognises in some degree that there is a language of dreams inherent in the unconscious, a primitive way of thinking which we inherit from our ancestors. Many years ago Jung came to the view that the dream is an archaic process of thought. It is a way of looking at things that belongs to the dim past. Now if the dream is thought at a deep level, in order that we should understand it, it must be developed up to the level of waking thought, and not only reduced down to a more primitive level of sexuality. Take the cartoon as an example—an ordinary *Punch* cartoon. It is a way of speaking, and its language is pictorial like that of the dream. We can reduce it to a sexual basis if we like, but are we then to say, "this cartoon is nothing but certain basic sexual components"? I do not think that we usually apply that method to the understanding of a cartoon, but on the contrary, we develop the cartoon up to the level of contemporary thought. We interpret its condensed symbolisms by adding to them—by what Jung calls the 'hermeneutical' method.

Associated with the names of Jung, Meyer, Hoch and MacCurdy, is the modern view of the two great groups of functional insanity—mania-depression and dementia praecox—which teaches that they are manifestations of a retreat or regression to a more primitive stage of human adaptation. They are constructive efforts at adaptation to reality—but instead of being progressive adaptations, they are regressive. Jung's teaching on the meaning of the neuroses is in the same enlightening vein.

We have therefore to consider the unconscious from an evolutionary point of view; and to the question, why is the 'unconscious' unconscious?, we may answer that it is unconscious because it is not yet fully adapted to reality. The unconscious contains nascent thought—thought that has not yet been fashioned into the form that is useful to consciousness. The unconscious contains the raw material of the conscious life. It contains the germinal stuff, the bulbs and roots, which exist below the surface because, as such, they are unadapted and meaningless to us. Their blossoms are what we value. It is only when a man is insane that they come into conscious expression directly, and then we see how unadapted are his nascent fantasies. It must be pointed out here that, if this theory is valid, we must expect to find in the unconscious, through its product the dream, traces of all human qualities—the heroic and upward striving as well as the bestial—the forces of progression as well as the forces of regression. Jung has insisted strongly upon this, and Maeder uses a striking phrase in this connexion when he says that in the dreams of neurotics—those who have regressed partially from the reality-function—we can find the "drowned voices of progression." Freud, as I understand him, sees only the regressive voices in the dream, the sirens of infantile sexuality.

The primordial thought feelings contain—in Jung's words—"not only every beautiful and great thought and feeling of humanity, but also every deed of shame and devilry of which human beings have ever been capable"; therefore the sources of conflict must lie in the unconscious itself, and not only in the restraints of an acquired morality imposed on the growing life, as Freud once thought.

The biological aim behind evolution seems to have been to thrust consciousness up to the gateways of incoming experience in order to free it from the past, from the already experienced. A study of the human nervous system, in the light of the recent work of Head, Sherrington, Rivers, Riddoch and others in the domain of neurology, leads one to this conclusion. The very existence of the reflex is dramatic evidence enough. The machinery of reflexes, of automatic acts, of habits, frees consciousness to deal specifically with incoming experience. I suppose we believe in the evolution of the body; the next step is to believe in the evolution of consciousness. If we do believe in the evolution of the mind we should not find it strange that in underlying consciousness there should exist more and more primitive layers of thought and feeling which are lit up during sleep. For in sleep we leave the focussed levels of waking consciousness and regress to levels where we look at our problems in a way which once belonged to the waking life of dim ancestors. But we call it a dream.

Jung suggests that the contents of the collective unconscious consist of archaic human functions from which spring the myths. But besides these archaic human functions there is also the residue of functions belonging to the animal ancestry of mankind, an ancestry which covers a vastly greater period than that of human existence. These archaic residues may become pathologically active when the life current, or the *libido*, streams backwards, away from reality. This streaming backwards from a hard task in reality is called regression.

A patient of mine who later developed dementia praecox frequently dreamt that he was becalmed in a small ship upon a smooth and empty sea. He was dangling his hand in the water when suddenly some monster of the deep, which he thought was a large yellow crab, seized it and began to pull him down.

From the Jung standpoint this dream represents the inner situation of the patient. It shows in primitive pictorial language the inner currents and tensions in the patient's psyche. It is a question whether the dreamer can pull up the monster, or whether the monster will pull down the dreamer. The monster in the deep is a symbol for that amount of *libido* which has regressed into the collective unconscious. It appears in the form of a large crab because it is a quantity of energy which has only a collective value, and no individual value, in that it is at a primitive invertebrate level, as it were, and not yet adapted to human function. Unless this energy can be freed and pulled up into the ship on the surface—that is, made available for conscious application—there will be tremendous danger. The ultimate fate of the patient was that he became dragged down beyond recall into the inexhaustible primordial fantasies of the collective unconscious.

To sum up, the 'unconscious' is unconscious because life is a process of progressive evolution, and the content of the healthy conscious mind requires to be closely adapted to reality if the individual is to be successful. Therefore the progressive transmutations of psychic energy are carried out at levels beneath consciousness, just as the progressive transmutations of the embryo are carried out in the womb of the mother, and it is only the comparatively adapted form that is born into waking life. Thus from this point of view we must regard the unconscious as the inexhaustible source of our psychic life, and not only as a cage containing strange and odious beasts.

WHY IS THE 'UNCONSCIOUS' UNCONSCIOUS?[1]

II

By W. H. R. RIVERS.

It may seem to many that the question asked in the title of this symposium is premature, that we must know far more than we do at present about the nature of the 'Unconscious,' *how* it becomes and remains unconscious, and again enters into consciousness, before we can expect to understand *why* it is unconscious. It is, however, interesting, even if it may not often be useful, to speculate about problems the solution of which cannot immediately be expected. Such speculation can have no advantage unless the object about which we speculate and the nature of the problem are clearly defined. It is necessary, therefore, to begin by considering the sense in which the term 'unconscious' is to be used and the exact meaning of the question by means of which the problem to be discussed has been formulated. The necessity for clearness concerning the nature of our problem and the meaning we attach to the term 'unconscious' is well illustrated by the opening contribution to this symposium. Captain Nicoll puts on one side as hardly worthy of consideration that part of the unconscious which is derived from individual experience, and deals almost exclusively with a form of the unconscious with which he believes Mankind to be endowed at birth. Following Jung, he holds that this part of the unconscious, composed of elements called 'primordial thought feelings,' is derived from some collective source of knowledge, and answers the question of our title by the statement that this storehouse of material is unconscious because it is not yet fully adapted to reality.

I shall return to his treatment of the subject after I have considered that kind of unconscious experience the existence of which can be demonstrated with certainty. I will only remark here that that part of the mind of which the individual human being is already in possession when he comes into the world covers the same field as, if it be not

[1] Contribution to a Symposium at a Joint Session of the British Psychological Society, the Aristotelian Society and the *Mind* Association, July 6, 1918.

identical with, that which the psychologist knows as instinct. It may be noted that some of the instances chosen by Captain Nicoll to illustrate what he means by a 'primordial thought feeling,' such as the basic emotions and the expressions of laughter and rage, are nothing but instincts or components of instincts. I hope to show that if we approach the subject through the channel of individual experience we shall be led to just such material as that chosen by Captain Nicoll to illustrate his meaning; but this experience has a function very different from that he assumes, and it fulfils a very different purpose in our mental life.

I will begin by excluding from our purview one of the senses in which we speak of the 'unconscious.' At any given moment by far the larger part of our total experience is unconscious. Only that which is in the focus of attention at the moment is clearly in consciousness, and only a very minute fraction of the whole can even be regarded as coming within the fringe of consciousness. At any given moment there is a vast body of unconscious experience which is only unconscious because it is not at the moment within the field of attention. It is clear that this kind of the 'unconscious' does not come within the scope of our question. If it be included the answer would be ready to hand. It needs no special discussion to enable us to understand why all our experience cannot be in consciousness at once. The kind of unconscious experience which needs explanation is that which never becomes manifestly conscious under the ordinary conditions of healthy waking life, but only appears in manifest consciousness under such special conditions as sleep or hypnosis.

The kind of experience I shall have especially in mind during my consideration of the problem is that of a sufferer from a dread of confined spaces, whose case I have recorded elsewhere[1]. A striking experience at the age of four had been completely absent from his manifest consciousness for nearly thirty years, although it had evidently been closely connected with his morbid symptoms. In this case the re-entrance into manifest consciousness took place during the reflexion following a dream after the attention of the dreamer had been especially directed towards some experience of childhood which could be connected with his symptoms. The case has many parallels in psychological literature. The feature which gives it especial importance is that it was possible to obtain confirmatory evidence of the occurrence of the thirty-year old incident from the parents of the patient and thus do away with all possibility of its being a fiction arising in the patient's mind through

[1] *Lancet*, Aug. 18, 1917.

its own activity or as the result of suggestion by the physician, a possibility which can rarely be excluded in the cases hitherto recorded.

In the case of my claustrophobic patient we have a striking experience at the age of four, which had clearly been connected throughout the patient's life with conscious experience of a painful kind. Yet until it returned to consciousness during my treatment it had been for many years completely unknown to the patient, and this despite prolonged attempts to recover some experience of childhood which could explain his symptoms. It is such experience, inaccessible to manifest consciousness and yet not merely persistent, but persisting in some active form, that I shall have especially in mind in discussing why the 'unconscious' is unconscious.

It may be well at this point to refer to the concept of dissociation of which this case of claustrophobia presents a characteristic instance. Though the infantile experience was absent from consciousness for nearly thirty years, it was in existence all the time in an active state, ready to set up the emotion of dread whenever conditions arose similar to those which produced the terror of the four year old child. This state of active existence apart from experience readily accessible to consciousness is known as dissociation. A dissociated body of experience is one which is separated from experience readily accessible to consciousness by some kind of obstacle only to be overcome under certain special conditions, such as sleep or hypnosis.

In the case of my claustrophobic patient, experience which was once in consciousness in a state of extreme intensity was, by some mechanism we do not at present understand, shut off or dissociated. When we ask why this unconscious experience should be unconscious, the question is complex. We want to know first why experience becomes unconscious, and secondly, why, having become unconscious, it should persist in this dissociated state, ready to reappear in consciousness, even after years of dormancy, if certain special conditions arise. If an answer to our question is to satisfy us, it should take account of these two separate problems.

A third problem involved in the question of our title is why experience thus lying dormant should yet preserve a certain activity showing itself in the form of vague dreads, apparently motiveless impulses, or other manifestations usually classed together as symptoms of disease, such symptoms for instance as those which troubled my patient whenever he was in, or even imagined himself in, a confined space.

I propose in this contribution to confine my attention to the first and second of these aspects of the question of our title. I have to consider why experience passes into that form of unconsciousness we call dissociation, and why the experience which thus becomes unconscious should persist in this state.

In considering why experience becomes unconscious, I propose to begin with two examples of dissociation from the lower animals. It is a special character of the life-history of mammals as compared with that of many other animals that it is relatively uniform and shows a high degree of continuity. It is free from those complete metamorphoses which occur in the lives of insects or those more gradual but yet vast transformations which occur in certain fishes and in the amphibia. The necessity for the passing of experience into a state of unconsciousness is especially great in animals whose life-history is made up of phases widely different from one another. In many forms of animal life the persistence in conscious form of experience gained in one phase when the animal has passed into another would be so disturbing as to render existence impossible.

Let us take as an example such a creature as the butterfly. During the larval stage the caterpillar builds up a highly complex body of experience based on a certain mode of progression and certain methods of obtaining and utilising food. In so far as this experience is conscious we must suppose that it takes part in the regulation and success of the life of the animal. If this experience should persist in the consciousness of the imago or were liable to be recalled under the stimulus of the leaves or other object which excited the interest of the larva, this recall could only impair and prejudice the movements of the imago adapted by its form to a wholly different mode of existence. The perfectly adjusted flight of the butterfly could only be disturbed if memories derived from the larval stage of existence entered into its consciousness. Again, in such an animal as the frog, memories of the aquatic period of its life-history tending to produce reactions of the caudal extremity of the animal could have nothing but a disturbing effect if they rose into the consciousness of the adult.

I cite these two cases because they are only striking examples of differences which exist between the infantile and adult experience of every animal including Man. The special character of the reactions of the human infant is that they consist largely of explicit expressions of affective states and of immediate responses to external stimuli. If the modes of consciousness connected with these forms of reaction persisted without modification in later life, they could only interfere with the very different and far more complex reactions of the adult. There is a definite reason why the conscious states connected with infantile reactions should become unconscious. Even in an animal whose life-history is as uniform as that of Man the different phases are sufficiently distinct to provide an ample reason why experience should become unconscious. This reason is to be found in the diversity of the different phases of the life-history and the incompatibility of the reactions of one phase with those of another.

It may be noted at this point that in the examples I have chosen the experience which is suppressed belongs to the domain of instinct. In the case of the butterfly and the frog this is clearly so, and the experience of the human infant, which needs similar suppression owing to its incompatibility with adult reactions, also belongs to a department of conduct which must be ranged with the instincts of the lower animals. The conduct of the infant depends on inherited impulses directly related to the primary needs of life.

The suppression of the larval experience of the insect or amphibian would seem to be a characteristic example of dissociation. We may regard the insect and amphibian as animals in which the process of dissociation has to be peculiarly complete owing to the absolute dependence of the animal upon instinct and the absence of any means of instruction by other members of the species, such as exist in varying degrees in animals whose life-history has been moulded upon different lines.

This suggests that dissociation is a process which is especially developed among animals dominated by instinct and is connected with this aspect of mental life. It becomes a question whether the dissociation of the mental life of Man is not a mode of reaction belonging originally to its instinctive aspect which has continued to be utilised after this aspect has been largely replaced by intelligence or reason. Dissociation would thus be a state especially prone to come into existence whenever it is required to put instinctive modes of reaction into abeyance, to suppress instinctive modes of behaviour which would interfere with the harmony of an existence based on less immediate and more modifiable reactions.

A difficulty which meets this view must be mentioned here. The immediate affective responses of the human infant which require to be controlled or suppressed in order that they shall not interfere with the reasoned behaviour of the adult can hardly be regarded as subject to dissociation, for they are ready to appear at any period of life if conditions arise suited to call them forth. This would seem to show that dissociation is not the only means by which the human organism deals with the instinctive modes of reaction belonging to the earlier phases of its history. If, as I suppose, dissociation is a process to which instinctive reactions are especially susceptible, it is only brought into action under special circumstances.

There is thus reason to believe that as discriminative and intelligent modes of reaction developed, the process of dissociation, so closely connected with instinct, was itself modified. In place of the complete suppression which was needed in the butterfly or frog, there came into existence a mechanism whereby certain parts of an instinctive mechanism which were useful could be utilised, while other parts detrimental to welfare could be suppressed or dissociated. This may be illustrated by an example taken from one of the lower forms of the mental experience of Man. The researches of Head and his colleagues[1] have shown the existence on the afferent side of the nervous system of mechanisms the nature of which is most naturally explained if they be the relics of an early stage of development which have been overlaid by sensory mechanisms of a different kind, dependent on different principles of action. The evidence for the existence of such a primitive mechanism at the peripheral end of the sensory nervous path is derived from observation of the mode of return of sensibility when a divided nerve has been reunited so that the process of regeneration may take place. During this process sensibility returns in two stages. The first kind of sensibility to return is characterised by its relative crudeness and vagueness, by the immediacy and unmodifiable character of its response and by the prominence of its affective tone, especially on the unpleasant side. This kind of sensibility, which Head has termed protopathic, has all the characters which we associate with instinct, while the later or epicritic mode of sensibility confers those powers of discrimination and modifiability under external conditions which belong especially to intelligence and reason.

At the central end of the sensory path there is a similar distinction. A cruder mode of sensibility characterised by the prominence of its affective tone is associated with the activity of an organ, the thalamus, which we know to belong to an early stage of evolution of the nervous system, while another mode of sensibility, endowed with the power of discrimination, has its seat in the latest and highest product of animal evolution, the cortex cerebri or neo-pallium.

[1] *Brain*, 1905, XXVIII. 99 and 116; 1908, XXXI. 323; 1911–12, XXXIV. 102.

Not only have we in the sensory nervous system two mechanisms, one associated with instinctive, the other with intelligent modes of reaction, but the lower or instinctive remains through adult life largely in a state of suppression or dissociation. This suppression is especially pronounced in that part of the protopathic system which has to do with the appreciation of space. The spatial reactions of the protopathic system are characterised by certain manifestations of radiation and reference to distant parts[1] which are normally absent over those parts of the surface of the body where the earlier instinctive is controlled by the later epicritic system. We have not, as in the case of the insect, the complete suppression or dissociation of a mechanism belonging to an earlier mode of existence. There is instanced here a mode of action in which Nature has utilised such parts of a mechanism as will serve her purpose while at the same time making use of the process of suppression or dissociation to put out of action such portions of the earlier apparatus as were useless or actually prejudicial to welfare. The peculiar conditions occurring during the regeneration of a reunited nerve allow one to produce, and even to watch, the process of suppression[2], the mode of reaction thus suppressed being of a kind which would be wholly incompatible with the proper activity of the highly exact epicritic mechanism of spatial appreciation.

The observations of Head and his colleagues show the existence in man, at a low level of psychological activity, of just such suppression of instinctive modes of reaction as we have seen to be necessary in animals altogether dependent on instinct. The main difference is that in man the suppression or dissociation is less complete. Nature appears to have made use of the process of suppression just in so far as it was serviceable. Certain manifestations of the instinctive protopathic mechanism have been suppressed, while others have been preserved and utilised to blend with the later epicritic development to form a product useful to the adult.

So far I have considered especially why experience becomes unconscious, or more exactly why it should pass into the special form of unconsciousness known as dissociation. We have found reason to believe that the process of dissociation is especially connected with instinctive modes of reaction, and that in the higher animals, and especially in Man in whom instinct has become subject to reason, the process has been employed in a selective manner, certain elements of an instinctive complex having been suppressed while others have been combined with

[1] *Brain*, 1908, XXXI. 412. [2] *Ibid.* 1908, XXXI. 406.

later more discriminative and modifiable modes of reaction. I have now to inquire whether this partial utilisation of instinctive modes of reaction does not provide the clue to the second problem raised in the question of our title, why the unconscious should persist.

If we assume that instincts are organic wholes, all parts of which stand or fall together, it will follow that when part of an instinct is utilised and maintained in activity by incorporation into some more complex product of development, the rest of the instinct also continues to exist. If it is not to interfere with the harmony of existence it has to be treated on the lines especially associated with instinct. Nature is in the position of one who wishes to use something and finds that she cannot choose part and ignore the rest. She must be content to take the bad with the good, the useless with the useful. In the case before us, Nature when confronted with the necessity for the continued existence of elements of instinct which were of no use for her purpose, found ready to her hand the process of dissociation, especially connected with instinct, by means of which the useless or harmful components of the material she was using could be suppressed.

It may be pointed out here that this principle permeates the whole of evolution. The example of the afferent nervous path which I have chosen to illustrate suppression or dissociation at a low level of psychological activity is at the same time an example of suppression, or as it is there more usually termed, inhibition, in the physiology of the nervous system. The process of repression of the psychologist, with its resulting state of suppression, corresponds exactly to the inhibition of the physiologist. Especially through the work of Hughlings Jackson[1] and Sherrington[2] we have learned how this process enters into every phase of activity throughout the evolution of the nervous system. Nature has always utilised already existing structures and functions, combining the new with the old, utilising what she needs and inhibiting those parts of the older products which would interfere with the normal action of the new. The symptomatology of disorders of the nervous system mainly depends on the coming to the surface under pathological conditions of those older and cruder activities which have been controlled or suppressed by the later products of evolution.

I can now return to the case I have chosen as my special example of unconscious experience, that of a patient with claustrophobia. This patient had an experience at the age of four which called forth in him

[1] See *Brain*, 1915, XXXVIII. 1.

[2] *The Integrative Action of the Nervous System*, London, 1915.

an extreme degree of the fear which forms the affective side of the instinct of self-preservation. If this experience had been rationalised and brought into subjection to the powers of reflexion and reason, the memories of the occurrence would have come to form part of the general body of experience of the child, liable to recall whenever suitable associations came into action. Owing to the special circumstances under which the experience occurred and the nature of the conditions under which the child was brought up, this did not take place. The child told no one of its trouble and the immature mind was left to find its own solution of the situation. Nature had here to do with the instinct of fear and utilised the process of dissociation which, as we have found reason to believe, is especially connected with that kind of experience. In order to get rid of the fear which seemed likely, if it had persisted, to destroy the child, it suppressed or dissociated from the rest of consciousness the memory by means of which the fear was kept in consciousness. Though thus dissociated the suppressed fear persisted, producing a vague dread whenever the child found itself in surroundings which would tend to awaken the dissociated memory.

When the child becomes a man, he is called upon to take part in war. The conditions of warfare would probably in any case have tended to reawaken the old memories through their general relation to the instinct of self-preservation. It happened, however, that his occupation led him to do much of his work in dug-outs, which called forth his dread of confined spaces in an accentuated form. This dread produced, or helped to produce, a neurosis, during the treatment of which the long dissociated memory was recalled to consciousness and reintegrated with his normal personality. Once recalled to consciousness, the instinctive mode of reaction, which had had so great an effect upon the child of four as to make its suppression necessary, became powerless before the intelligence of the man of thirty.

Using this case as an example of the unconscious, the answer to our question is that experience becomes unconscious and persists in this state because Nature is accustomed to utilise a process closely associated with instinct to put out of action instinctive modes of behaviour which would interfere with the proper working of a mechanism formed through the combination of instinctive with intelligent modes of reaction. Experience becomes unconscious because instinct and intelligence run on different lines and are in many respects incompatible with one another. Just as the experience of the larva is dissociated from that of the imago because the two are adapted to wholly different modes of existence, so do instinctive reactions tend to be dissociated from intelligent experience whenever the immediate and unmodifiable nature of the one becomes incompatible with the delicate discriminations and adjustments by means of which Man adapts his behaviour to the highly complex needs of the society to which he belongs.

The life of the higher animals, and of Man above all others, is a continuous struggle between two opposed principles of mental activity. Nature cannot do without instinct and continually utilises it to form products blended with reason. In the process of blending she has made great use of the process of dissociation which is especially connected with instinct. Whenever instinct tends to interfere unduly with reason, Nature has ready to her hand the process of dissociation which is instinct's peculiar instrument. The experience which is thus dissociated persists, partly because of the inherent vitality of instinct, partly because the suppressed experience usually forms an integral part of a complex, other constituents of which have been utilised and incorporated into the personality of the experient.

In conclusion I must return to the treatment of our problem adopted by Captain Nicoll. It will be remembered that, following Jung, he supposes the unconscious to have two constituents, one derived from personal experience, the other from some impersonal or collective source. His contribution deals with the latter, while I have dealt exclusively with the former aspect of the unconscious. I have now to inquire whether it is possible to reconcile, or at least to bring into relation with one another, the two different methods of treating the subject.

The main aim of my own contribution has been to show that the unconscious is the home of instinct and that the mechanism by which experience becomes and remains unconscious is one especially connected with this mode of mental activity. As I have already pointed out, several of the examples chosen by Captain Nicoll to illustrate the primordial thought feelings of the impersonal unconscious clearly belong to the domain of instinct. In so far as the primordial thought feelings of Jung and Nicoll are such reactions as 'pleasure and pain,' the 'gestures and expressions' of 'laughter and rage,' or the 'basic emotions,' they correspond closely with the elements of which I suppose the unconscious to be composed.

Moreover, Captain Nicoll and I are in agreement in that we both regard the problem from the evolutionary point of view. The difference between us is that, whereas I look on the unconscious, in the special sense in which I use the term, as a storehouse of instinctive activities which are partially or wholly incompatible with later products of evolution, Captain Nicoll sees in the unconscious an inexhaustible reservoir with which Man is provided at his birth and from which he can draw material for his psychic development. He gives little indication of the nature of these contributions made by the unconscious, but to his master, Jung, they include material for the highest flights of genius, such as the discovery of the law of conservation of energy[1]. I cannot see that Captain Nicoll has really answered the question of our title in so far as such constituents of the unconscious are concerned.

[1] *Analytic Psychology*, 2nd ed., London, 1917, 411.

WHY IS THE 'UNCONSCIOUS' UNCONSCIOUS?[1]

III

By ERNEST JONES.

We seem to be fairly well agreed as to the sense in which the term 'unconscious' is being used in this symposium, and I need merely refer to Dr Rivers's definition of the subject to express my assent with it. I desire only to call further attention to the important consideration that a large number of the most significant mental processes designated as unconscious have *never* been conscious; the matter is therefore much wider than the mere question of profound forgetting, interesting as may be the problems aroused by this question.

The standpoint I wish to represent is neither a philosophical nor a speculative one, but the scientific one of attempting to base conclusions on actual experience and investigation of the relevant phenomena. There are many methods, *e.g.* hypnotism, automatic writing, etc., for recovering forgotten memories, for penetrating into the preconscious and what might be called the upper layers of the unconscious; but, so far as I know, the only method at present available for exploration of the unconscious proper is that of psycho-analysis, devised by Professor Freud of Vienna, and the conclusions here presented accord closely with those Freud has reached as the result of his researches by means of psycho-analysis.

Strictly speaking, consideration of the nature, content and characteristics of the unconscious should precede a discussion of its source and the reasons for its existence, and this not only on logical grounds, but because the knowledge thus involved forms an indispensable basis for any such discussion. I cannot avoid, therefore, entering to some extent at least on these preliminary questions.

In the first place, an outstanding feature of all unconscious processes is that the attempt to explore them and make them conscious is always accompanied by manifestations of active opposition on the part of the

[1] Contribution to a Symposium at a Joint Session of the British Psychological Society, the Aristotelian Society and the *Mind* Association, July 6, 1918.

subject. Dr Rivers is no doubt alluding to this when he speaks of the unconscious being separated from consciousness by "some kind of obstacle only to be overcome under certain special conditions." From the fact of this active opposition, which can be overcome only by the expenditure of a considerable amount of psychical energy, Freud infers that the obstacle in question is not static, but dynamic in nature, *i.e.* that some force is in operation, and he makes the further inference that the opposition, technically called 'resistance,' which is displayed when any attempt is made to render a previously unconscious process conscious is identical with the force, then called 'repression,' which is in constant action keeping such processes unconscious. This consideration in itself seems to me to dispose of Jung's hypothesis—if I understand Captain Nicoll's exposition of it aright—namely, that the main reason for the existence of the unconscious is its lack of development.

A second feature of the content of the unconscious yields an explanation of the motive power attaching to the force of repression. This is that unconscious mental processes are of such a nature as to be in sharp conflict with the tendencies and attitudes of the conscious mind. I fully agree with Dr Rivers in his repeated reference to the 'incompatibility' of the unconscious with consciousness—or, as he also puts it, of many aspects of instinct with intelligence—though I am not sure how far we are in accord as to the nature of this incompatibility. One must, of course, subscribe to his description of the broad contrast between "the immediate and unmodifiable nature of instinct" and "the delicate discrimination and adjustments of intelligence"—as also to his rough correlation of instincts with the unconscious and intelligence with consciousness; but this seems to me to be of too general an order to take us further than the first step in the search for an explanation of the barrier separating the unconscious from consciousness. The conflict in question strikes one as being too poignant and severe to be accounted for by any mere preference on the part of consciousness for a more discriminating, or even a more advantageous, mode of reaction. I would venture to suggest that, in the endeavour to establish a purely utilitarian[1] basis for the exclusion of unconscious mental processes, Dr Rivers has been too much influenced by the English biological doctrines of the nineteenth century, which, as is now known, interpreted all vital processes too exclusively in terms of survival value.

Actual experience of the phenomena concerned makes it very difficult to bring more than a part of them under the head of utility values, and

[1] I use the word here in its biological, not its philosophical sense.

we are obliged, in the first place at all events, to state them in affective terms. It may be stated as a general law that what in the unconscious has a positive affective tone, *i.e.* of pleasure (*Lust*), has in consciousness a negative affective tone, *i.e.* of displeasure (*Unlust*); or, as the Germans put it, the affective tone becomes served with a negative prefix. I should say that the most essential characteristic of repression lies in this affective transformation of pleasure into displeasure. The incompatibility of which Dr Rivers speaks is thus seen to be of a hedonic nature, though, it is true, examination of its *ultimate* source may perhaps approximate our two points of view more closely than would appear from this statement.

Further inquiry into the nature of this affective conflict shews that it is specific, not general in kind. The tendencies of the unconscious are such as to be incompatible with the standards of the conscious self on ethical, social, moral, or aesthetic grounds, rather than on grounds of expediency or advantage; they are such as are felt to be shameful, wrong, horrible, or disgusting. They are 'tabu' to the conscious self, abhorrent to its standards, and repudiated by its ideals. A typical example would be the impulse to murder a near relative. It would, in my opinion, be incorrect to ascribe the non-yielding to this common unconscious impulse entirely to motives of expediency, to the consideration that such conduct would, in Rivers's phrase, be "detrimental to welfare"; it is surely due rather to the affective counter-motives of repugnance which inhibit the very thought itself.

It is instructive to correlate this contrast between the unconscious and conscious selves with that between the infantile and adult selves, for without a doubt they are genetically related to each other. The conduct and impulses of the infant are in most respects impossible to an adult on exactly the grounds, enumerated above, on which the unconscious is reprehensible to consciousness. One has only to picture the details of an infant's daily life to realise the truth of this statement, and it is even more extensively true of those impulses the correct nature of which is constantly misinterpreted by the adult, examples being the sexual character of many of the sensations associated with excretory functions, breast-feeding, and other harmless activities. These infantile impulses and tendencies are suppressed and modified during development, in the way in which Dr Rivers describes that instincts are suppressed and modified for the purposes of intelligence. Many of the tendencies have been inhibited from the start and never allowed to reach consciousness, the significance of others has been similarly kept back, while still others have undergone a subsequent suppression and have been buried in the unconscious. Freud's view is that the unconscious of the adult is constituted by these infantile tendencies, to some extent reinforced by later ones. The unconscious is thus the infantile part of the mind, and, as Dr Rivers points out, the part in closest relation to the primary instincts.

It will be seen that the conflict between the unconscious and consciousness may be paraphrased as the conflict between the animal and the human in man, or between the primitive and the civilised. While it is obviously true that the instincts of the unconscious, like all instincts, are the typical primitive, inherited part of the mind, and that the inhibiting forces of repression represent, both ontogenetically and phylogenetically, a later acquirement, Captain Nicoll is in error in ascribing to Freud the view that the unconscious is repressed by morality acquired by the individual, for not only, according to Freud, is it repressed by other influences besides moral ones, but he has shewn there is reason to think that a great many, and not the least important, of the inhibiting forces are inherited as the result of experiences and selection of preceding generations. Repression (in the technical sense), *i.e.* keeping from consciousness, is simply one of the ways in which the civilised mind deals with its primitive, ancestral heritage, and one must imagine that the passing millennia leave their imprint on the inborn capacity to accomplish this task.

So far, I think, we are on solid ground which can at any time be confirmed by any one who takes the trouble to familiarise himself with the experiences of psycho-analysis. When we pursue further the historical genesis of the mechanism of repression and the split between the unconscious and consciousness, the way becomes more obscure. No doubt Dr Rivers is right in postulating some biological and presumably physiological archetype for the processes of suppression and repression, but this has yet to be discovered[1]. The interesting analogy he draws with the larval phase of butterflies and frogs, though remote, is suggestive, but I could wish that he had given us some examples taken from the non-human mammalia.

Freud's researches have followed a more purely psychological direction, and the conclusions he has reached are avowedly more tentative than those of his referred to above. In his opinion, the unconscious and the conscious minds are preceded by two mental systems, which he

[1] An attempt was made by Ortvay, "Eine biologische Parallele zu dem Verdrängungsvorgang," *Internat. Zeitschr. f. ärztl. Psychoanalyse,* II. 25.

calls the primary and secondary systems respectively. Like most scientific men, he regards all activity on the part of a living organism as being fundamentally destined to restore the state of equilibrium that has been disturbed by some stimulus, of either internal or external origin, and thus to allay or to still some excitation; in the mental sphere Freud terms this mechanism the fulfilling of a wish, using the expression in a broad sense. In the adult mind this gigantic reflex is commonly not recognised to be such, partly because of the enormous complexity of the intervening processes and partly because the difficulty of tracing the ultimate sources of stimulation fortifies the illusion of spontaneity. He pictures, now, the first mental reaction of the new-born infant as being quite incoordinate and undirected; the fulfilment of the wish—that cold or hunger should be allayed, etc.—is brought about entirely by the environment, the mother or nurse. The next time a similar stimulation occurs, a diffuse excitation is set up, which radiates in every direction. This reaches the memory-trace left by the previous experience of gratification, re-animates it, and the infant experiences what may be called a hallucinatory gratification of its need, in which a perceptual identity of the current situation with the previous one is brought about. Where, however, the exciting stimulus is insistent, such as in the case of an organic need, the hallucinatory gratification soon ceases to maintain its capacity for satisfying the need and recourse must be had to other methods. The diffuse excitation presses against the motor end of the psychical apparatus, and the infant incoordinately squirms and cries until the environment is moved to replace the hallucinatory perception by real sensations, of repletion or what not.

It is at this point that the activity of the second mental system sets in, the bitter experience of life having demonstrated the inadequacy of the first—just as later it teaches the adolescent the unsatisfactoriness of day-dreaming (*i.e.* hallucinatory gratification) as a permanent method of gratifying desires. In the second system the tendency of the psychical impulse to 'regress,' as it is technically called, from the memory picture back towards a corresponding perception is inhibited, and the accompanying energy is directed towards another way of attaining the desired goal (the reproduction of the satisfying perception)—namely, a more or less complicated motor process as a result of which an alteration in the environment is brought about that produces a real perception instead of an imaginary one.

The distinguishing mark of the second system is that there is a control and inhibition of the free movement of psychical energy characteristic of the first system. In the latter everything is concentrated on the one feature of allowing the freest possible movement throughout the whole system, and the same is true of the subsequently developed unconscious. Thus an excitation passes with the greatest readiness from one idea or memory to another, no form of association being too narrow a bridge to allow of the passage. The well-known characteristics of logical thought are entirely lacking; direct contradictions are ignored, the slightest play on words is made use of, ideas are grouped together that have only faint resemblances between them, and there results a general levelling of thought not altogether unlike what psychiatrists call a 'flight of ideas.' In the second system, on the contrary, a ban is placed on this freedom of movement, and that for two reasons. The first reason is an economical one. As much of the energy as possible has to be reserved for its ultimate purpose of altering the environment by means of motor effort, and so dissipation of it has to be minimised. The energy accompanying the original excitation is localised at its appropriate site while tentative efforts are made to seek out the memory-traces that are most suitable for leading to the desired motor goal. The second reason for the inhibition in question has to do with the pleasure-pain principle. If the primary stimulus, such as pain, is definitely disagreeable, then incoordinate motor manifestations are aroused until sooner or later the infant becomes withdrawn from its sphere of action. Here there is no subsequent tendency to re-animate the painful perception; on the contrary, there will be a tendency to get away from the painful memory whenever there is any chance of its being aroused, and to excite the memory would bring with it the danger of the excitation passing over, as a regression, into the perception, with the production of fresh pain. This turning away from the painful memory is the prototype of what in later life would be termed 'psychical repression'; it no doubt represents the primitive movement of flight from danger or pain. In the first system, therefore, anything disagreeable is simply ignored and cannot be incorporated in any psychical concatenation; the system can do nothing except imagine the fulfilment of wishes. The second system, on the other hand, cannot ignore the painful in this way, for it has to have at its disposal as many memories as possible in order to use the knowledge of their nature in making its selection of suitable paths for affecting the outer world. It can make use of a painful memory for this purpose only when it is able in some way to prevent the development of pain that stimulation of it would naturally cause. It does this in the same way as it prevents a dissipation of energy—namely, by exerting an inhibiting and localising influence, in this case on a painful affect. A common analogy to this in daily life is when a man brings himself deliberately to speak of a matter which under other circumstances, *e.g.* when he is unprepared or when his feelings are not under control, would prove extremely painful; that is to say, although the idea is actively conscious he can suppress the feelings connected with it. This affective inhibition is never quite complete, but if it fails to take place the primordial pleasure-pain principle enters into operation, the memory is discarded and cannot be used even by the second mental system. This is just what happens in the mechanism of psychical repression[1].

Freud considers that the two mental systems just outlined are the precursors of the unconscious and conscious mind of later life. The mind of the infant can hardly be called either unconscious or conscious, though probably it is nearer to the former. The control of the primary system by the secondary remains imperfect throughout life, and under a number of circumstances the latter—our logical, conscious thinking—falls partly or entirely under the influence of the older one. The most perfect examples of this are in insanity and dreams, less complete ones being psychoneurotic symptoms and the every-day slips of the tongue and pen, errors in memory, apparently inexplicable forgetfulnesses, and like tricks of the mind. In all these cases the secondary system has fallen under the influence of the primary and now unconscious one, and the apparent failure in the mental machinery is really nothing else than the normal, correct functioning of a quite different mental system. Inadequate control on the part of the secondary system is made possible by the circumstance that it grows far more rapidly and changes far more extensively than the primary one, the result of which is that the primitive and unalterable wishes of the latter come to stand in more and more flagrant contradiction with the standards of the secondary system, exactly as those of infancy do with the standards of later years. A child of two will in all innocence enjoy doing things that disgust him when he is a few years older; a boy of five will torture animals, hurt his sister's feelings by destroying her most cherished possessions, and purloin the property of others, with a callousness that would be impossible to the same boy a few years later; a girl of six will wear skirts of a length that would bring a blush to the cheeks of the maiden of sixteen, and

[1] This paragraph is a modified version of some passages in Chapter XXXVI. of my *Papers on Psycho-Analysis*, Second Edition, 1918. Several other chapters in the book also deal with the subject of the unconscious.

similar examples are matters of every-day observation. In such situations it is no longer possible for the secondary system to restrict its activity to the mere finding of suitable ways to alter the environment so as to bring about a gratification of the wish that can only be imaginatively gratified by the primary system. It is now opposed to the wish itself, and vetoes the gratification of it. Before the unconscious wish can achieve expression it has somehow to pass the barrier constructed by the secondary system, and it can do this only by first undergoing a number of profound modifications and restrictions.

It will be seen that for the psycho-analytical school of psychology the question asked in the title of this symposium is met by the answer that the 'unconscious' is unconscious because of the inhibiting pressure of the affective factors grouped under the name 'repression.' Our attention thus passes to the nature and genesis of the mechanism of repression, to which the main part of my remarks has been devoted. I have last to discuss the difference between the view here advanced concerning the nature of repression, one which may be called the 'hedonic' view, and what I conceive to be Dr Rivers's view, which I shall venture to designate by the term 'utilitarian.' According to him, the unconscious is repressed because its activity would be disadvantageous to an organism that requires a more discriminating and modifiable adjustment to reality, *i.e.* because of the utility and survival value gained by repression; whereas I attribute its repression to a purely affective conflict, to what might almost be called a question of taste, which may be either useful or merely stupid and even harmful. I certainly hold that the hedonic view is directly based on experience of the phenomena relating to repression, and I can adduce ample evidence of mental processes repressed through distaste, where the repression has been much more injurious than beneficial. Indeed, a good deal of repression of the unconscious seems to me to be blind, irrational, and altogether wanting in any criterion of usefulness to the individual; this may be true, for instance, of the repression evinced in Dr Rivers's case of claustrophobia.

But the matter is much more complex than might appear from the antithesis just established. While it is true that the conflict between the unconscious and consciousness is shewn by experience always to proceed on hedonic lines, it will be remembered that, according to Freud's theory, these two regions of the mind are genetically preceded by two simpler modes of activity, the primary and secondary mental systems respectively, in the inter-relation of which we can perceive evidences of the operation of Dr Rivers's utilitarian principle. In Freud's terminology, the primary mental system, like the later unconscious, is dominated entirely by the hedonic 'pleasure-pain principle,' while the secondary one is dominated by the 'reality-principle,' *i.e.* by the subordination of immediate pleasure or avoidance of pain to the exigencies of objective reality. The gain here achieved for the welfare of the organism is evident, so that we may say that the contrast or opposition between the two systems is based on the utilitarian principle, in exactly the way described by Dr Rivers when speaking of instinct and intelligence. In the substitution of the secondary for the primary system the tendencies of the latter are inhibited, guided, and controlled; but I think it is important to distinguish this process from the mechanism of actual repression, where the attempt is made to obliterate the primary tendencies. Further, there is reason for supposing that the reality-principle itself is developed as an extension and elaboration of the original pleasure-principle—so as more surely to secure a lasting and satisfactory gratification of the primary hedonic tendencies, the allaying of the stimulus—rather than by the introduction of an altogether new criterion of utility. It would seem, therefore, as if we can perceive in the course of development the following order of events: First, the growth of the utilitarian reality-principle, which gradually comes to control and even in a large measure to supplant the more primitive hedonic pleasure-pain principle. Later a change in affective values, whereby what was originally pleasurable, and which remains so in the unconscious, becomes 'displeasurable' and highly distasteful to the more rapidly developing conscious system, the one more in contact with external reality; and it is at this point that the secondary conscious mentality has recourse to the hedonic and non-utilitarian mechanism of repression, which results in the constituting of the true unconscious.

The three contributors to the symposium take up different positions in regard to this very question. Captain Nicoll holds that the unconscious is *not yet* adapted to reality, but he gives no reason why it should not be, and, from his lofty conception of its nature, one might have expected that it easily would be. Dr Rivers holds that the unconscious is *no longer* adapted to reality, though it was at some earlier phase of development, either individual or racial. In my opinion, the unconscious is *sometimes* better adapted to reality than consciousness and *sometimes not*; the criterion is often irrelevant and never cardinal.

I find it difficult to establish any points of contact with Captain Nicoll's presentation of Jung's view. He makes such elementary mistakes concerning Freud's theory—as, for instance, when he ascribes to him the view that all unconscious processes must at one time have been in the individual's consciousness—that it is clear he has not grasped it. His example of unconscious symbolism—that a crab (why a yellow one?) can represent "a quantity of energy which has only a collective value, and no individual value, in that it is at a primitive invertebrate level, as it were, and not yet adapted to human function"—contradicts all that I hold to be established on this important matter[1], for in the unconscious such a highly elaborate and lately acquired conception certainly cannot be symbolised by a simple concrete idea, the whole process of symbolism being just the reverse of this. I imagine that Jung's position rests on a pseudo-philosophical and religious basis that is irreconcilable with any scientific view. It is characterised by a repudiation of the principle of determinism, together with the assumption that the spiritual qualities of man are inherent from the beginning as such, instead of being the product of the evolution of lowlier ones. The contrast between this view and Freud's is just the same as that between the positions adopted by Drummond and Wallace, on the one hand, and Darwin and Huxley, on the other, regarding the origin of the mind and soul—a matter which in the scientific world was decided half a century ago.

[1] See my paper, "The Theory of Symbolism," in the present number of this Journal.

PUBLICATIONS RECENTLY RECEIVED

Two Studies in Mental Tests. By C. C. BRIGHAM. Princeton Psychological Contributions. Psychol. Monogr. Vol. 24, No. 1, pp. 1–254. Pub. Psychol. Rev. Co. Princeton, N.J.

These studies are valuable contributions to the literature of the Binet Scale. Most of the topics dealt with have been discussed by previous writers; but the work has the merit of being restricted to the careful and thorough study of certain specific factors which are important determinants of the results obtained by the application of Binet's Scale.

The first study deals with "Variable Factors in the Binet Tests." The factors discussed are the "personal equation" of the experimenter, the correlation of the results with school-grade, and with sex-differences. A noteworthy feature of the work is that the author uses as his data the results of the individual tests, and not the gross results of mental age. The effect of the personal equation was found to be considerable with the following tests: Copying the diamond, indicating omissions, defining in terms superior to use, memory drawings, absurdities, and reconstructing dissected sentences. The discrepancies between the results of four experimenters were due to differences in the apparatus used, in the technique adopted, and in their judgment of the value of the responses; but the author believes it is possible to eliminate all these sources of error. The effect of school training was most marked in the tests of counting stamps, counting backwards, enumerating days of the week and months, giving the date, naming coins, making change, and reconstructing dissected sentences. The results of the individual tests indicated a general correlation with the school-grade; but the correlation with sex-differences proved to be slight.

The data and conclusions of this first study support the view that the Binet Scale is chiefly valuable from the qualitative rather than the quantitative standpoint. Nothing but good can result by frankly admitting that the scale in its present form is a crude yard-stick and not a vernier; and it is to be regretted that in so many recent studies, elaborate and delicate statistical methods have been applied to results obtained with this scale. The practical and theoretical interests of psychology are at present better served by studies such as the second in this volume, which deals with "The Diagnostic Value of some Mental Tests." The author has investigated which tests of the Binet Scale are of greatest importance in differentiating subnormal from normal individuals. Data were also obtained with a number of supplementary tests of a more practical character, *e.g.* puzzle-, construction-, and code-tests. The difference between the percentages of normal and retarded subjects that passed a test was regarded as a measure of its diagnostic value. It is not surprising that the three Binet tests of most diagnostic value proved to be, the comprehending of difficult questions, the reconstructing of dissected sentences, and the detecting of absurdities; but probably most persons with some experience in using the scale will be surprised that the arranging of five weights, and the describing and interpreting of pictures proved to be tests of little diagnostic value. The author interprets his data with due regard to the complexity of factors involved.

The Development of Intelligence in Children. Pp. 336. $2.00. *The Intelligence of the Feeble-minded.* Pp. 328. $2.00. By A. BINET, and TH. SIMON. Translated by E. S. KITE, Vineland Research Laboratory. Pub. by Williams and Wilkins, Co. Baltimore.

These two volumes which give the English-speaking countries the opportunity of reading the articles of Binet and Simon in a convenient form, will no doubt be well received. Miss Kite has performed, efficiently and opportunely, a valuable service in translating the articles which have proved to be landmarks in the field of applied psychology. The views and results given in these articles have been so widely discussed that it is unnecessary to write at length about them here. After reading these two volumes, it is difficult to avoid the conclusion that the writings are of greater scientific value from the diagnostic than from the quantitative standpoint. Nevertheless, the contents of the first volume which deal with the quantitative researches have received much more attention during the last five years than those of the second volume. Another feature which strikes the reader of these volumes is the extent to which the originators of the measuring scale anticipated many of the criticisms which subsequent writers have so frequently repeated. It is to be hoped that one result of the publication of these two volumes will be a more thorough understanding of the specific ends the scale was designed to serve, which should lead to a better appreciation of its potentialities and limitations.

Reality and Truth, a Critical and Constructive Essay concerning Knowledge, Certainty and Truth. By JOHN G. VANCE, M.A., Ph.D. Longmans, Green and Co. Pp. xi + 344.

Mr Vance has written an interesting work on epistemology, which may be read with profit by the advanced student, or by the beginner as an introduction to the subject. He has endeavoured with some success to avoid technical terms, and his style is clear and vivacious. His method is to begin with a modified form of the Cartesian doubt as a means of discovering what is indubitable, and thus he arrives at consciousness, the three laws of thought or being, and causality, as the foundations of his system. The principle of causality he deduces from the law of contradiction by an argument which, in the reviewer's opinion, is fallacious (pp. 106–112).

The author has studied Psychology, and sometimes approaches his subject by psychological paths. The seventh chapter, explaining how we know the nature of a real world, is based upon his own experimental study of meanings. He finds that there are two distinct modes of knowledge, sensorial (including imagery) and intellectual, neither reducible to the other; and (p. 315) declares with hardly enough discussion that, unlike the sensorial element of knowledge, the intellectual does not work immediately through the nervous system or any bodily organ.

Mr Vance's reading in the various schools of philosophy has been extensive, and he gives in ch. IV an elaborate criticism of Descartes, and of Kant in chs. XI and XII. This makes it a matter of surprise that he should so lightly pass over the difficulties that embarrass the inquirer concerning the validity of our knowledge of external reality. But, indeed, the general tendency of his work is to pacify the doubting mind. He has projected another volume on God, freedom and immortality, and indicates in ch. XIII the general method of his argument.

On the whole, there is much sense in this book and much original reasoning. Perhaps one cause of our not being convinced by a new system of philosophy is very much like Coleridge's excuse for not believing in ghosts: "No, madam, no: I have seen too many of them."

The Belief in God and Immortality, A Psychological, Anthropological, and Statistical Study. By JAMES H. LEUBA, Professor of Psychology and Pedagogy in Bryn Mawr College, Boston. Sherman, French and Co. 1916 Pp. xvii + 340. Price $2.00 net.

This book is divided into three parts. The first section contains an anthropological study of the primary belief in Immortality, and its origins; and a discussion of various attempts to demonstrate the modern belief. The writing is lively and interesting, but nothing else can be said in its favour. The anthropology is all at second or third hand, while what metaphysical discussion is attempted is necessarily hurried and inadequate.

The third part consists of a critical study of the "present utility of the beliefs in personal immortality and in a personal God." Leuba considers this to be exceedingly small.

The real justification for the publication of the book, however, is in the second section, which is "a statistical study of the belief in a personal God and in personal immortality in the United States." The method of the *questionnaire* was adopted, and three fairly extensive examinations were carried out. First Leuba dealt with the belief in God among American College students. The records, which show an extraordinary fluency of expression on the part of the students, certainly suggest no wide-spread definiteness of belief. Similar conclusions follow from the second investigation, which dealt with the belief in immortality among the same class of person. The third and most thorough study concerns the belief in God and immortality among "American scientists, sociologists, historians, and psychologists." Leuba attempted to deal with philosophers also, but the attempt broke down, because his subjects were unable to answer questions the terms of which they found to be extremely ambiguous. His results, which are well-handled and exhibited, suggest, as he remarks, a wide-spread rejection of the two fundamental dogmas of Christianity. In each class of persons taken the number of believers is considerably less than the number of non-believers. Physicists appear to provide more believers than psychologists, sociologists, and biologists, a result which Leuba suggests to be due to the fact that the latter groups better appreciate the reign of law in organic life. It is interesting that the psychological group is the only one which provides a fewer number believing in immortality than believing in God. Rejection of belief is, Leuba thinks, very largely a consequence of development of individuality.

The book is undoubtedly interesting and stimulating. Leuba, however, rather too readily leaves the psychological for the speculative, and in that way often seems to weaken his presentation.

Present Day Applications of Psychology with Special Reference to Industry, Education and Nervous Breakdown. By Charles S. Myers, M.A., M.D., Sc.D., F.R.S., Director of the Psychological Laboratory, Cambridge; Lieut.-Colonel, R.A.M.C.; sometime Consulting Psychologist, B.E.F. London: Methuen and Co., Ltd. 1*s*. net.

This suggestive little book forms a summary, admirably impartial and concise, of the recent achievements of applied psychology. It falls into two parts. The first section opens with a very clear description of the difference in standpoint between modern psychology on the one hand, and philosophy, physical science and physiology on the other; and contains a condensed account of the application of psychological methods to industrial efficiency, and to individual mental differences so far as they affect industrial efficiency. The second section deals with the application of psychology to nervous derangement. This topic is discussed from the standpoint of the psychology of feeling, just as the former was discussed from the standpoint of the psychology of intelligence. A few paragraphs are devoted to the application of psychology to advertisement and jurisprudence. And the remainder of the section is mainly concerned with shell-shock, discussed from a point of view that approximates towards that of psycho-analysis. Since it is in this field that Colonel Myers' work has recently lain, the discussion, though all too brief, is of especial interest.

It is at once significant, opportune, and encouraging that one of the leaders of pure experimental psychology in England should thus lend the weight of his authority to its application to practical problems of industry and medicine.

Studies in Psychology. By Colleagues and Former Students of Edward Bradford Titchener. Worcester, Mass. Louie N. Wilson. 1917. Pp. 337.

Experiments in Psychical Research. By John Edgar Coover. Leland Stanford Junior University Publications. Psychical Research Monograph. No. 1. 1917. Pp. xxiv + 641. $3.50.

Child Training. By V. M. Hillyer. London: Duckworth and Co. 1915. Pp. xxxix + 299. 5*s*. net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

November 24, 1917.	The Use of Fables as Tests of the Appreciation of Moral Values, by W. J. Anderson.
	The Concept of "Ownership" in relation to the Sentiments, by M. E. Wakefield.
January 26, 1918.	Destructiveness and Superstition, by M. D. Eder.
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	The Psychology of a Bird—Facts and Experiments, by F. B. Kirkman.

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THE PSYCHOLOGICAL INTERPRETATION OF SENSE DATA.

By JOHN LAIRD.

§ 1. *Sense data (or sense presentations).*
§ 2. *Inadequacy of the theory that sense data do not belong to the subject-matter of psychology.*
§ 3. *Falsity of the view that presentations are fictions.*
§ 4. *Assumptions in this paper.*
§ 5. *Difficulties involved in the description of sense data.*
§ 6. *Descriptive analysis of visual sense data with special reference to their 'meaning.'*
§ 7. *'Complication.'*
§ 8. *The bodily part of these sense data.*
§ 9. *Analysis of organic sense data.*
§ 10. *Analysis of sense data other than organic or visual.*
§ 11. *The development of sense data.*
§ 12. *Conclusion.*

§ 1. THE term 'sense datum' was introduced into English philosophy by writers of a metaphysical bent who believed that a dangerous ambiguity lurked in the usual term 'sensation,' and consequently felt the need for a new technical expression. A 'sensation,' they maintained, must be analysed into two elements, the mental act of sensing and the immediate object of this act. The sensation of yellow, for example, includes two things, the act of apprehending yellow and the yellow perceived[1] through this act. The act itself is not yellow at all. On the other hand the sense datum presented to it is most emphatically yellow.

The distinction thus drawn is of great importance for the metaphysical problems arising out of perception, especially in connexion with the ancient and perplexing dispute between realists and idealists on the question whether the *esse* of sensible things is their *percipi*. It is clear that a sensation is a mental event; but when sensations are analysed

[1] I have not attempted to distinguish between sensation and perception in this paper. The differences between them are not, I think, relevant to my argument unless (possibly) in the case of those entirely hypothetical entities, 'pure sensations.'

into mental acts and objects sensed, there are strong grounds for maintaining that only the acts of sensing are mental and that the objects sensed are not mental in any important way. Such metaphysical problems, however, will not be considered in this paper. The distinction which these writers emphasize so strongly has a psychological side to it as well as a metaphysical. Indeed, it is essentially a psychological distinction entailing important metaphysical consequences; and it raises a number of fundamental questions in the psychological theory of sense presentations. These theoretical questions cannot be neglected by any psychology which is more than an interesting collection of special observations or a set of rules of laboratory technique; and an attempt will be made to deal with some of them in this paper.

Psychologists, in a word, must set themselves to explain precisely what is meant by sense presentations or sense data. It is not enough to point to examples, and to say that such familiar facts as tasting a plum or seeing a rainbow are all that is meant. Again, it is not sufficient to refer to the sense organs on the one hand and to introspection on the other, in the vague hope that a prolonged oscillation between these two points of view will somehow evolve a satisfactory compromise and avoid the necessity for formulating a logically tenable theory. Neither the question of what presentations *are*, nor the question of what they *appear as*, can be left to chance. Indeed, according to the implications of some theories, sense data are not, properly speaking, part of the subject-matter of psychology, while according to other authors there are no such things as presentations.

§ 2. The grounds for the first of these contentions may be briefly recounted. Psychology, it is said, deals with the mind. The mind is a continuum of acts, such as the acts of believing, supposing, perceiving, sensing, choosing, desiring, and the like. These 'acts' or 'enjoyments' refer to objects; and objects are physical, not mental. The detailed consideration of them, therefore, should be left to the physical sciences. Psychology has to deal with the continuum of mental acts.

Even granting, however, that this continuum of acts is the true and proper domain of psychology, a psychology without presentations would be neither feasible nor desirable. The reason is that the acts of the mind have very little observable content, and certainly too poor a content to supply a sufficient empirical basis for the solution of many psychological problems. If indeed, as on Reid's theory of perception, sweet and bitter, hot and cold, purple and scarlet could be regarded as mental acts or as parts of them, this difficulty would not arise. The theory we are discussing, however, distinguishes absolutely between the acts of attending, discriminating and apprehending on the one hand, and the objects attended to, discriminated or apprehended on the other. It is plain, despite Reid, that sweet and purple appear on the object side of this antithesis. Consequently the observable characteristics of the mental acts are correspondingly reduced.

Some writers, indeed, speak as if this mental continuum were either wholly diaphanous, or else qualitatively the same throughout its texture. They investigate introspectively the sensing of blue and the sensing of green, and find that while the presentations 'blue' and 'green' are readily distinguishable, the acts of sensing them present no difference that can be detected. It is quite illegitimate, however, to construct a general theory implying the qualitative identity of all mental processes upon the basis of mere inability to find a distinction in the highly abstract and difficult feat of introspection required in these instances. Qualitative differences clearly appear in other cases. The process of doubting, for example, can be observed by introspection to differ qualitatively from the process of believing although both are mental acts of cognition, and the distinctions thus apparent between the various modalities of cognition are still more evident when cognition is compared with some other mental process such as conation.

None the less, it is plain that these very general distinctions between mental acts would afford a very slender basis indeed for most of the detailed investigations of psychology. If the sensing of blue cannot be distinguished from the sensing of green although the sense data blue and green are so manifestly different, the whole psychology of these acts of sensing would be summed up in a few words, supposing that psychology were restricted to them. In that case such questions as the development of sensory acuity, the growth of the idea of space, the acquirement of meaning in association and 'complication,' would be left without the rudiments of a possible answer. Thus even a psychology defined by the three assumptions that psychology is the science of the mind, that the mind is a continuum of acts, and that every act has a distinct existence from its object, would be forced to go further afield. It would have to evolve a theory of presentations.

The truth is that the cognitive acts of the mind do not exist or have any meaning except as directed to some object. A thought is nothing unless it is the thought of something. This statement must not be taken to imply that apprehension is identical with that which is apprehended or that it is absurd to introduce a little tiresome chatter about act and object into psychological theory. On any theory there is an important difference between the process of discriminating, and the blue and yellow, soft and rough which are discriminated. On any theory the difficulty of supposing the mind to be yellow or rough and the impossibility of denying that acts of apprehension are truly part of its being remain significant. The legitimate conclusion is only that it is hopeless to attempt to deal with cognitive processes in complete abstraction from their objects. The being of thoughts essentially requires a reference to objects, and the being of any particular thought requires or rather *is* reference to its specific object. Thus thought and object must be considered in relation to one another if either the function or the meaning of thought is adequately appreciated. And the object, considered specifically as the correlate of some act of apprehension, is just what is meant by a presentation.

§ 3. Some writers, it is true, attempt to dispense with presentations altogether. The only genuine facts in the case, they maintain, are the mind on the one hand and the objective world on the other. Unless presentations are identical with one or other of these they are mere fictions, and if any part of psychological theory is based upon these fictions, psychology itself is built upon sand.

This theory, as it stands, is plainly inadequate. No one can reasonably maintain that when a man and a dog sniff the same piece of meat the direct olfactory presentations are precisely identical for the pair, that a man's breakfast tastes the same whether he has a cold in the head or not, or that a colour-blind man and a normal woman perceive precisely the same coloured patch when they look at geraniums in flower. It may be true that both of them refer to the same objective world, and that, broadly speaking, they draw the same kind of distinctions within it. But there is a difference in the way in which this world appears to each of them, and this distinction is vital for psychology.

It is not necessary, and it would probably be false, for psychologists to maintain that these presentations form a distinct class of entities which are neither minds nor physical things but partake of the nature of both. All that need be supposed is the plain matter of fact that whenever there is perception something definite appears to the mind and that this appearance differs for different subjects according to the character, peculiarities and degree of development of their several minds. Such appearances may in all cases be *part* of the objective world. In that case the partial revelations of the world differ for each percipient, and the further investigation of this question may be left to metaphysics.

The important point for the psychologist to remember is that all these appearances are facts which he is bound to take at their face value. His primary duty is to describe, classify and relate them to the best of his ability. Things are what they are, no doubt. That is an axiom of metaphysics which is not often challenged intentionally. For psychology the corresponding axiom is that presentations are as they appear; and the task of tracing the history of mental development rests primarily upon the comparison and correlation of presentations.

§ 4. After this preliminary discussion it is advisable to indicate some of the main assumptions of this paper. While the terms 'act' and 'object' may not have been happily chosen (and both of them may be pertinently criticised in several regards), the distinction they are intended to express is both true in itself and fundamental in psychological theory. In particular the process of apprehension, at every level of cognition, is distinct from the object to which it refers, although its function and character are summed up in the fact that it has a specific reference to a specific object which appears. This analysis, indeed, may be more convincing in the case of judgment than in that of sensing; but in the last resort all cognition must have the same fundamental character, and it is therefore legitimate to argue from the clear case of judging to the relatively more obscure instance of sensing.

It is needless, however, to enter into these questions meticulously in the present connexion, since the purpose of this paper is to investigate, not acts of cognition, but certain characteristics of the objects which appear to them and particularly of a limited class of those objects. This limited class consists of the objects with which we are directly acquainted in sensation and perception. These objects may be called sense presentations or sense data. The word 'presentation' and the word 'object' itself are intended to describe a plain matter of fact. This fact is that the knowing mind (and, in particular, the perceiving mind) is confronted, when it knows or perceives, with something which appears to it. This appearance must not be identified either with physical objects in the case of perception or with a whole system of objective truth in the more general case of knowledge. It is possible, indeed, that any given sense presentation is precisely identical with some part or aspect of physical reality, and it seems plain that any proposition which is truly known is therefore identical with part of the system of truths which would come within the ken of omniscience. But psychology is concerned first and foremost with what appears precisely in the way in which it appears to this or the other individual mind at any given time. It is bound to consider presentations at their face value, whatever species of cognition be considered.

§ 5. The manifest consequence of these assumptions is that the first requisite for the interpretation of sense presentations is a careful description of them. This requisite, on a superficial view, appears to be very easily satisfied, at any rate in the case in which any particular person sets out to describe his own sense presentations. The presentation is precisely what appears to him. How then is there any room for hesitation or mistake in describing it with complete accuracy? A clammy surface, a grating noise, a localised visual presentation, *e.g.* the spark from a rocket, indubitably appear to us. It ought to be easy to describe them with a little care.

The task of describing these appearances, however, is not at all easy. In the first place there are inevitable difficulties connected with the use of language. The words we use in describing what we see or hear are commonly employed to refer to things and not to the appearances of things. The plain man's idea of a 'thing,' it is true, is different from the scientific conception of a physical object such as an atom or a molecule. The 'thing' for him contains secondary qualities as well as primary, and it is never so small as the billionth part of the *minimum visibile.* On the other hand, he supposes it at least to be something enduring, though it appears only intermittently to him. Language, then, is fitted to describe *continuants* rather than sense presentations, and even phrases like 'a patch of colour' naturally suggest this meaning. The patch is regarded as a 'thing' like the patch on a garment or on a lady's cheek. Such expressions, therefore, are apt to suggest a misconception of presentations which ought to be avoided at all hazards. Sense presentations are not continuants. If they were they would form an intermediate class of entities screening the mind from objective reality, and this view finds no support in psychological description, not to speak of the havoc it plays in the theory of knowledge.

We must use words, however, and perhaps we may guard against this difficulty sufficiently if we explain at the outset that our descriptions of presentations are not meant to convey the implication that these are even potentially continuants. A more serious difficulty results from the fact that all descriptions involve concepts or universals. Thus in the description 'a coloured shape' the universal 'colour' and the universal 'shape' are implied, and if we had to suppose a separate presented counterpart for each separate universal our description would be a complete failure.

This difficulty is so serious in the eyes of some philosophers that they conclude that all psychological descriptions are merely inaccurate makeshifts, since the conceptual terms employed are palpably inadequate to describe the fluid continuous character of the appearances. There is no need to draw this conclusion, however. If in describing a presented coloured shape we were forced to maintain that there are *two* presentations in the case, the presented colour *and* the presented shape, we should certainly be maintaining an absurdity. There is only one presentation, the coloured shape. But it would be a mistake to suppose that the description implies that there are two.

On the other hand, an analogous mistake is very common indeed. Let us consider the instance of a 'localised visual sense datum,' *e.g.* the sparks at a firework display. It is clear that such visual sense data are always localised presentations, and that this localisation implies certain relations. The localisation is relative to the other portions of the presented visual field, and to the presented body of the percipient. The percipient, for example, describes this sense datum as situated in front of and above him, at such and such a distance from the Grand Stand, and to the right of some neighbouring coruscation in the firework display.

This relation to other parts of the presented field is part of the meaning of the localised sense datum to the percipient. It would be a grave error to suppose, however, that this meaning can be separated, except analytically, from the other elements in the visual sense datum. There are not two presentations in the case, the visual sense datum and the meaning. There is only one presentation, and it has colour, shape and meaning, being accurately described as a localised visual sense datum. The meaning, in other words, belongs to the presentation in precisely the same sense as the colour and the shape.

§ 6. This conclusion is so important that it is necessary to draw out its implications at some length. For convenience' sake it is advisable to restrict the discussion to the sense of sight in the first instance, and provisionally to neglect the question of development.

Everyone can verify for himself the substantial accuracy of the current psychological descriptions in terms of the focus and the margin of the visual field. We select a part of the visual field for attentive scrutiny at any moment, and strive to envisage it as clearly as possible. The area so selected is of varying size relatively to the rest of the field, and it always shades gradually into the margin. Thus when a patch of colour is selected as an obvious instance of a sense datum, it is usually regarded as part of the discriminated central area of the visual field.

The choice of such examples is legitimate and even necessary since the centre of the visual field is certainly presented, and there is justice in the claim that the clearest examples of presentations should be chosen as illustrations. At the same time the centre of the visual field is not presented in isolation, and consequently the patches of colour aforesaid are only parts of any total momentary presentation and not the whole of it. This circumstance affects the description of visual presentations. It does not, of course, affect the accuracy of the statement that distinctive coloured shapes with clear-cut boundaries are frequently presented. On the other hand it affects part of the meaning which literally and necessarily belongs to the presentation. Even logical analysis implies a reference to its logical setting. *A fortiori* in psychological analysis the presented setting is required for an adequate description of any portion of the presented field.

Thus a reference to the rest of the visual field is obviously implied whenever there is a localised visual sense datum, and there are no unlocalised visual sense data. Any visual sense datum which is attentively scrutinised is part of a wider whole, and this fact is always part of its meaning. This circumstance also affects the description of the whole visual field at any moment. In the first place, there are no abrupt recognisable limits to the margin. We know, of course, that we cannot see with the back of our heads, but even after the most attentive survey we cannot be quite certain of the precise point at which the visual presentation ceases. In the second place, the margin itself has its meaning for us just as truly as the centre, and the significance of the margin is a characteristic of the total momentary presentation. On some theories, indeed, the margin or 'fringe' is supposed to be the sole repository of meaning. That is a mistake. But the marginal meaning counts, and the principal point to notice in the present case is that part of the intrinsic meaning of the margin is the fact that it is felt to belong to a wider continuum which extends beyond its boundaries. The total visual field at any moment is felt to be a mere fragment, and this meaning is as truly a part of its presented character as redness is part of the character of a cherry.

Very similar arguments hold when temporal meaning is considered instead of spatial. It is convenient to speak, as we have done, of a momentary presentation, but elementary reflection shows that here again there is a very real danger of misleading simplification. Although the usual accounts of the 'specious present' are obscure in many particulars they show to demonstration that the least perceptible unit of time has a sensible duration. A momentary presentation, therefore, has at least a 'no more' and a 'not yet' meaning which cannot be prescinded from it without manifest mutilation. Again, the temporal boundaries of the momentary presentation are not fixed, any more than its spatial limits. One moment merges into the next, and the appreciation of any moment implies the apprehension of this continuous passage of time. There is no such thing as an absolutely unchanging appearance. However steadily we fix our gaze there is a continuous fluctuation in the presentation. Scarcely a second can elapse without some change in it.

These fluctuations in visual presentations depend primarily upon the changing adjustment of the sense organs and upon the mobile eagerness of attention in concentrating now on one feature and anon upon another feature in the presented complex. Whatever point is fixed upon, however, is apprehended as part of a continuum, both temporal and spatial. Concentration upon some aspects and relative neglect of others, fluctuation and fragmentariness belong to the very being of sense presentations.

This conclusion may be stated otherwise by saying that all visual presentations at the level of adult perception are as much *signs* as *facts*. It is legitimate, indeed, to distinguish analytically between the various moments of this significance. But every presentation actually apprehended has a certain total significance which belongs to it in the same sense as its quality or intensity.

At this point an objection may occur to the reader. In the earlier argument of this paper a distinction was drawn between the 'thing' of common sense, and the presentations appearing to Smith or Jones. The subsequent argument, it may seem, tends to suggest the abolition of this distinction. Is not 'thinghood' part of the meaning of sense presentations?

The objection thus stated requires a closer analysis of the implications of 'meaning.' 'Meaning' in much psychological writing is wrapped in impenetrable obscurity. Thus for example, it is often supposed to be wholly mental, and to be fully explained by a reference to mental retentiveness. These implications are certainly present in the sense that the development of the apprehension of meaning requires retentiveness on the part of the mind, and that the extent to which the mind is capable of apprehending meaning depends upon the degree of its retentiveness. On the other hand there is a purely objective side to meaning, and this objective side is all that is required for the analysis of presentations as these are understood in the present discussion.

When a picture is said to signify or mean its original, the objective fact is that the picture and its original have certain common characteristics and relationships. These, in the last analysis, are discovered by the mind on comparing the two. When this discovery has been made, in some particular cases a number of possibilities arise when the picture is presented without its original. In the first place the presentation of the picture may lead us to think of the original itself. It is not necessary, however, that the original should be reinstated. The picture still has representative significance if the total presentation before the mind is the picture characterized by the implication or meaning that it is related to the original in various ways. This apprehension of the picture as a term of relationship may and frequently does occur without any apprehension of the other terms connected with it by these relations. More generally a 'meaning' of this kind can be understood by analogy when the terms represented have never been apprehended at all. Thus we may know that a photograph represents some one although we may never have seen the person portrayed in it. The photograph, in this case, has all the presented meaning of a photograph. It is not a mere shape on a glazed surface. But the term connected with the photograph by the relations implied in photographic representation need not be apprehended at all, even imaginatively.

This analysis may readily be applied to the spatial and temporal meaning which forms part of all sense presentations. The 'thing' of common sense certainly possesses colour, shape and other sensory qualities. On the other hand it is a continuant, presented intermittently. The solution is that the presentation of the thing consists of certain fluctuating appearances each having meaning and, in part, a common meaning. The presentation, however, does not include the term to which this common meaning refers. Continuants are not presented in their entirety. No one, for example, can see the whole surface of a sphere all at once. On the other hand the 'reference to a continuant' which is part of 'thinghood' belongs to the meaning of presentations. Thus, again, presentations differ for different individuals, and differ for the same individual at different times, although a necessary element in each of these different presentations may be a meaning which implies the existence of an abiding common object of reference.

The investigation of the problems thus arising leads beyond psychology. Psychology is concerned with the world of presented meanings, not with the implications of these meanings, and this limitation to presented meanings has certain consequences which should be noted. The preceding argument would be wholly misleading if the spatial and temporal meanings under discussion were interpreted in an abstract or scientific fashion. Only a few trained intellects are capable of pondering intelligently upon the theory of space and time, and although the general principles of physics are presented to these intellects, they are presented as free ideas detached from sensory presentations. Even the trained thinker does not think of pure mathematics or metageometry when he is scanning a ditch which he has to jump, or prodding with his dibble to plant a cabbage. It is this sort of spatial meaning which pertains to sense presentations, and one of the chief difficulties in the case is that the description of this meaning is inevitably expressed too abstractly. When we turn our attention to it we are bound to think of the theory of space rather than of the spatial meaning actually presented. We apprehend what this is when we do not ask. When we ask, we usually think of something else.

§ 7. These explanations may suffice to pave the way for the consideration of a further question arising out of the analysis of visual sense data. This is the fact of 'complication.' We say that ice looks slippery or that a cushion looks soft. Slipperiness, clearly, is part of the meaning of the ice-presentation, and softness of the cushion-presentation. Neither slipperiness nor softness however is the direct object of vision proper, and the language of these statements consequently requires some explanation. If the preceding argument has been correct, the language is perfectly accurate. The presented cushion really does look soft. That is as truly part of its meaning as the fact that it is localised above a chair. On the other hand, it is not necessary to maintain that there is any explicit revival in these instances of the tactual and organic sense data specifically denoted by softness or slipperiness, or even that these are present as the conclusions of 'unconscious inferences.' The meaning may very well be present without the presence of the terms indicated through it. No doubt there is often a memory or a dim presentation of these data from other senses. The slippery look of the ice frequently revives them, and often arouses emotions as well. This, however, seems to be an exceptional case. In most instances of visual 'complication' tactual and other elements do not seem to be presented even dimly, although there is always a tendency for them to arise if we allow our minds to linger upon this tactual meaning.

§ 8. It would seem, then, that visual sense data have a non-visual meaning. This fact is neither strange nor surprising in itself, and it suggests another essential feature of the analysis of visual sense data. Up to the present we have considered the characteristics of visual sense data without any reference to the organism of the percipient. This was natural, since no one can see the interior of his body unless he is mortally wounded or in some similar condition which has no bearing on normal psychology. We can only see the presented surface of the body and presentations external to it.

On the other hand, it must always be remembered that the body is presented along with any sense data external to it, and in the case of sight these intra-organic sense data are so inseparably blended with the character and meaning of the extra-organic visual presentations that it is impossible to lose sight of them without being faithless to the facts. As is well known, the most significant of these intra-organic presentations for the analysis of vision are certain movements of ocular adjustment, and it is easy to prove that without them a localised visual sense datum would be a mere non-entity, incapable even of appearing. The 'external' sense datum, in a word, is always part of a complex of which intra-organic presentations form part, and these intra-organic presentations, particularly the kinaesthetic ones, are essential both to the description of the total presented complex and to the extra-organic part of it.

These statements, it must be remembered, are intended to apply to the presentation only. The organism here considered is the presented organism, just as the external object seen is an external presentation; and it is necessary to remember this in order to avoid unnecessary misunderstandings. When psychologists begin to draw upon the theories of geometrical or physiological optics for the benefit of their science, they quickly run upon a difficulty which even beginners notice. The artless tyro, indeed, frequently turns upon his teachers with words of the following import: "You set out to explain to me how and what I see, and you introduce me to a complicated set of theories concerning the angle of convergence, 'corresponding points' on the retina, and localisation in the occipital lobe of the brain. I am obliged to you, and I find your information very interesting. But I would venture to point out to you, with all respect, that I can see as well as you can, although I know nothing whatsoever of mathematics or of the physiological structure of my brain or of my eyes. May I ask, then, how I am able to see as well as you do when I am so utterly ignorant of every single factor that you mention in your explanation?"

It is clear that this objection has great weight. Indeed, it is much more serious than many psychologists are willing to admit. The fact is that the organism as conceived by the anatomist or the physiologist is not presented to the percipient. Strictly speaking, it is not a sense presentation at all, but a physical object of the type considered in the sciences of physics or chemistry. But in so far as it can be regarded as a sense presentation with any significant meaning, it is a sense presentation which the subject himself cannot have. The subject cannot see the pulpy mass which is called his brain, and he cannot touch his retina or his optic nerve. Accordingly, any explanation in terms either of physical stimuli or physiological conditions is, psychologically speaking, subordinate, however indispensable it may be. It may be required in order to explain the causes which make a certain psychological condition possible. It is not a description of psychological fact.

The only important sense in which the angle of convergence, ocular adjustment, and the like enter into and have a meaning for visual presentations is the sense in which the angle or the adjustment is presented. It is clear, of course, that they really are presented. Even physiologists admit the fact indirectly when they distinguish extero-ceptive receptors from proprio-ceptive and intero-ceptive ones. The subject, as we have seen, can neither see nor touch the interior of his organism. Perhaps, in strictness of language, he cannot even hear it. But it is presented to him none the less, and its presentation consists of organic sense data, including the kinaesthetic presentations aforesaid. These sense data are presented to the subject in precisely the same sense as extra-organic presentations. They are discriminated and attended to in the same way. They are spatial as the others are spatial. In a word, they have all the characteristic features of sense presentations, and this conclusion is very important since these intra-organic sense data are always part of the total complex revealed to any sense and are always required for the adequate description or interpretation of any sense presentation whatsoever.

Kinaesthetic sense data, then, play a vital part in all visual perception, although they are apt to be forgotten in hasty psychologizing. That is natural, since the percipient subject is usually so much concerned with their significance that he is apt to neglect their very existence although he certainly makes these ocular adjustments and is certainly aware of them, at least as part of the fringe of his total presentation. This reference to the fringe also solves one of the difficulties in the case. A neglected presentation, it may be plausibly argued, is not a presentation at all. It does not appear, just because it is neglected. This objection, however, is not really solid. The margin or fringe of any presentation is always neglected with varying degrees of neglect. If it were not at least partially neglected it would not be a margin. But this is a very different thing from saying that the margin does not appear. It appears most certainly, although it is not attentively discriminated. And its appearance is suffused with meaning.

§ 9. After this long discussion of visual presentations, a briefer reference to the other senses must suffice. It will be convenient to consider organic sense data first, because, if for no other reason, these organic sense data are always present to the conscious mind, and are always a significant part of the total complex presented to the mind even when attention is specifically directed to extra-organic presentations.

Some of the characteristic features of organic sense data, like nausea or toothache, seem to mark them out as a class apart, and even to raise a doubt concerning the question whether these presentations are properly described as objects at all. It is clear, however, that all sense data are upon the same footing. Either all are objects or none is; and enough has been said already to indicate the sense in which all presentations are objects. They are all apprehended. They all appear to the mind in their quality, intensity, extension and duration, and in so appearing they confront the mind with something which it contemplates. Thus despite their contrast with visual presentations, organic sense data have the same fundamental status in psychological theory.

The principal features of this contrast are usually supposed to be the subjectivity of organic sense data, their comparative meaninglessness, and their extreme vagueness. These questions require discussion.

As we have seen, no one can perceive the interior of another man's body in the way in which the other man himself perceives it. This circumstance implies that organic sense data are in a certain sense private and peculiar when compared with others. It is easy, however, to draw false inferences from this circumstance, and therefore necessary to point out the true inference precisely. The subjectivity of these sense data is only a subjectivity of restriction, and the fact that such presentations are restricted to a single mind proves nothing concerning their psychological status. If, for example, it so happened that only one man perceived a particular rainbow, this fact would not justify the inference that this rainbow was more subjective than other rainbows. Again, the circumstance that the total presentation revealed to any sense necessarily contains a part consisting of organic sense data shows how rash it would be to infer that there is any absolute difference between presentations in respect of subjectivity. This would be true quite irrespective of the familiar arguments which purport to prove that no sense presentations can be shared by different percipients. That problem is too intricate to be considered here. In outline, the answer seems to be that the total presentations appearing to any two minds can never be identical, that there may be some coincidence in respect of extra-organic presentations, but that the sense in which it is correct to say that two men perceive the same thing refers principally to the common meaning which belongs to their presentations.

Organic sense data like nausea or toothache are relatively devoid of any meaning which points beyond themselves. Indeed, there is a certain plausibility in the suggestion that the cognitive significance of sense presentations diminishes in proportion to the closeness of their connexion with the vital functions. The most vital organs are scarcely sensed at all, there is greater sensitivity on approaching the epidermis, and extra-organic presentations are more significant still. The organism adapts itself to the end of living. It is guided by presentations in important parts of this enterprise, but the life functions themselves, which are the end of all its seeking, are not presented with any characteristic meaning pointing beyond themselves. It is true that all these organic presentations have acquired a certain meaning in terms of the visible body which appears to other men and can be regarded in a mirror. But this may be a side issue, due primarily to the need of communicating with our fellows.

Be that as it may, the plain matter of fact is that the most meaningful sense presentations are those which guide active movements, particularly when the range of the sense, as in the instance of vision, gives the organism time for adjustment and delayed reaction. This fact has many interesting applications. It partially explains, for instance, why the meanings expressed in terms of the primary qualities are so fundamental both in the common sense notion of 'things' and in scientific explanations of the material world. Again, it is regarded as the corner stone of some conational psychologies, and of those self-styled 'psychologies' which substitute behaviour for the mind. The discussion of these theories would lead too far afield. It is enough to point out in the present connexion that cognition is the guide of all movement. The collapse of the arguments attempting to show that there is a special 'innervation sense' proves incidentally that all our movements are guided by the kinaesthetic sense data presented to us. Thus the analysis of kinaesthetic sense data reveals certain differences between them and other organic presentations. If the other organic sense data are relatively meaningless, kinaesthetic presentations are essentially charged with meaning both for the organism itself and for the external senses. It is unnecessary to give further illustrations of this, and it is also evident that kinaesthetic sense data are much clearer than other organic presentations.

'Clearness' and 'vagueness,' it is true, are terms which imply their own difficulties. It is scarcely doubtful, however, that a man's sense presentations become clearer as he awakes slowly after an anaesthetic, or, again, that visual or tactual extension is clearer than the 'extensity' or vague voluminousness of most organic sense data. This difference is due for the most part to a more definite apprehension of spatial meaning. But that is not the whole difference. The development of sensory discrimination implies more than the increasingly explicit recognition of relations. Qualitative differences in the presentations also emerge.

§ 10. Before considering development, however, we may briefly review the other senses on the lines of the preceding discussion. Taste and smell need not detain us long. They are really organic sense data with a very indefinite extra-organic meaning. Thus it is almost impossible to tell where a smell, as we say, comes from, unless we are aware of some other facts, such as the direction of the wind. The kinaesthetic sense data of sniffing have little significance of this kind. The smell appears to be diffused around us, as the particles causing it probably are in fact. Taste, again, so far as it can be distinguished from smell, has little or no meaning beyond itself, unless the way in which it leads to the mastication or rejection of food can be so regarded. No one supposes, for example, that a pear has a taste unless someone tastes it, and whatever meaning beyond the organism is usually attached to the sense of taste is probably an instance of 'complication' due to the fact that the organ which tastes also touches.

Temperature is as much an organic sense datum as extra-organic, and its meaning is also 'complicated' through its inseparable connexion with touch. A hot breath, indeed, is often like a physical obstacle, as the writers of 'shockers' of a spiritualistic cast duly bid us note. In that case, however, the breath is really a physical obstacle whose pressure touches us, and, indeed, might knock us down. Temperature, however, is not merely an organic sense datum although it can be sensed only in so far as there is contact with the organism. If we admit, as we must, that visual sense data are presented 'out there,' we must also admit that cold or warmth is presented outside the perceived surface of our bodies.

The analysis of sound is very difficult in this regard. Sounds are certainly localised outside the body with some accuracy as regards distance and direction, although direction to right or left is more accurately estimated than direction in front or behind. Thus, as a matter of psychological description, sounds are external presentations. At the same time their presented spatial character reveals important differences from that of visual or tactual presentations. Although the note of the harp in the orchestra appears to be to the left of the note of the violin, neither of the notes appears to have a shape. Sounds, as it were, seem to have position without area, and the volume of a sound appears to belong rather to the organic part of the total sound presentation than to the part localised at a distance. Indeed, it does not seem possible to carry this analysis further in the description of presented sounds, and the whole procedure is unnatural since the spatial meaning of sounds is so intimately blended with visual or tactual spatial meaning that this analysis itself runs counter to the normal manner of presentation.

Touch, of course, has greater cognitive significance than any other sense except sight. It has, indeed, a tang of reality about it which vision sometimes seems to lack almost entirely, and this tang of reality is itself part of the meaning of normal sense presentation. Since the sense of touch requires contact with the body, there is naturally a difficulty in distinguishing the organic part of the presentation from the external part, and it might even seem plausible to regard the bodily presentation as the essential part of the phenomenon, and the external reference as a meaning holding of the organic presentation. This analysis, however, is probably mistaken even with regard to softness or hardness, or to the contact, say, of the surface of the hand with the object touched.

Mere contact, however, has very little to do with the significance of touch, on account of the way in which that sense is employed for the purpose of exploring objects outside our bodies. This exploration is accomplished by moving the limbs and fingers, and the touch presentation is informed by these kinaesthetic sense data. In this respect there is a close analogy between touch and vision although the exploring organ is different and the touch horizon so much more circumscribed. Tactual space itself is very largely a system of meanings which, in the case of the blind, are at bottom time-meanings, and in the case of normal percipients are fused, by 'complication' with visual space. This statement, however, is not quite accurate. Visual and tactual space must certainly be distinguished, but neither of them is pure in normal experience. Even apart from the organic part of them, their presented meaning includes a blending from many different sensory sources.

§ 11. The question of development remains. Development necessarily implies continuity of process, and in the case of sense perception it is also necessary to account for the improvement in this continuous process. Both continuity and improvement, however, may easily be incorporated into an argument on the lines of the preceding discussion. Thus the development from crude extensity to definite perceived extension is essentially a process of the continuous acquirement of meaning on the part of spatial data by means of the kinaesthetic presentations connected with active movement. We may even suppose if we choose that all sense presentations are developed from a common blurred matrix, and that they come to acquire meaning as successive acts of attention do their work. This development of meaning, as we have seen, goes hand in hand with other differences as in the often-quoted example of the blacksmith who perceives many tints in the flame although the onlookers see only a uniform glow. The cases of the tea-taster or the letter-sorter would supply parallel instances for other senses. The most notable species of improvement in sense perception, however, consists in the fuller discovery of meaning, and the test of this is increased proficiency and accuracy in adjustment to the environment.

Unless the meaning of sense presentations is recognised to be part of their character, it is hard to see how presentations could be said to improve at all except in an 'outrageously Pickwickian' sense. Any theory, therefore, which is able to interpret the improvement of sense data in a fashion that is not Pickwickian is, *pro tanto*, preferable to others.

Our view, in a word, is that the most rudimentary mind must be supposed to be presented initially with a vague voluminous sensory mass within which it gradually comes to discriminate and recognise meanings. Even this initial vague presentation is an object with which the mind is confronted, although it is very different indeed from the 'things' of common sense or from the molecules of science, and although the mind at this stage could not, of course, contemplate the abstract notion of objectivity in any sense whatsoever. As experience grows, the mind comes to recognise the distinction between that part of the presented field which is the presented organism, and the parts of the field perceived outside the organism. The apprehension of this distinction is acquired in special connexion with the presentations of movement and with the significance of the perceived environment for movements. Organic sense presentations are always part of any presented field, and they are relatively constant in comparison with the presented environment.

On the other hand organic sense presentations are not the whole presented field, nor are the presentations outside the body properly regarded as meanings read into organic sense data. A combination of dubious psychology and indifferent metaphysics may indeed suggest that nothing but the body (or is it the brain?) can be directly presented to the mind, and that the external world is but an inferred or imaginative eject. Such theories have no basis in descriptive psychology. As a matter of description, at least, the mind must be presumed to apprehend whatever it appears to apprehend, and to do so with equal directness in all cases to which its inspection reaches. It reaches the Milky Way when it scans the heavens.

The meanings thus acquired are an integral part of any presentation, and they should be interpreted in a wholly objective sense, *i.e.* the meaning is just as objective as any other feature of the presentation. These presented meanings, however, must not be confused with the terms to which they point, since these terms need not be presented at all, or presented only to a subsequent act of reflective conception. The meanings which guide our movements are not of this order. They must be expressed, it is true, in terms of time, space or cause, but the presentational significance of these may be very different indeed from their significance in an abstract metaphysical discussion.

§ 12. Descriptive psychology has to steer a middle course between two extremes. On the one hand it has to avoid the theory that sense data are a congeries of apparently disconnected heterogeneous entities, momentary, perishing, and without significance beyond themselves. This is a misleading description of the presentations actually appearing, and, in particular, it neglects the margin of presentation. On the other hand psychology must avoid the error of maintaining that the world is presented to the subject in sense perception 'and there's an end on't.' The aim of this paper has been to explain the chief conceptions required in a consistent description which follows the middle course.

The world itself is never presented as a whole, nor is any part of it presented in the same way to different senses. Whatever is perceived is felt to be fragmentary, even if the rounded whole of the world is potentially inferable from the character of this fragmentariness. Again it is absurd to speak of the perception of a two-inch line in which one of the inches is tactual and the other inch visual, and it is equally absurd not to distinguish carefully and accurately between the infant's presentations and the man's. The infant, the animal, and the man may perhaps, in a very large sense, refer to the same world in every act of perception, at any rate if they are credited at every stage with the apprehension of inherited meanings. The world in this sense, however, is only a potential term of reference, and this term is not presented in sense perception. The most that can be claimed for it is that it is a logical implication of certain meanings which are genuine parts of these enormously different presentations.

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THE UNCONSCIOUS.

By CARVETH READ.

§ 1. *Repression and Unconsciousness as determined by Organic Structure.*

AMONG the contributions to the discussion of the question "Why is the 'Unconscious' unconscious?" recently published in this *Journal*[1] Dr Rivers's interested me very much, exciting some other ideas that have gradually been accumulating. He traced back the phenomena of repression to remote stages of animal development. In insects that undergo complete metamorphosis he found it in its extreme form; and pointed out that, as the experience of a caterpillar of (say) a butterfly is not only useless but, if conserved, would be injurious to the imago, it is advantageous to repress it by the dissociation that takes place between the earlier and later phases of its life. Something similar, though less thorough, must occur in the change from a tadpole into a frog. In the higher classes of vertebrates no external change is seen, such as happens to this amphibian; but even in man (as presumably in every vertebrate that shows much development of the neopallium) the protopathic nervous system with the coordination of instinctive actions through the basal ganglia has been overgrown and largely superseded by the epicritic system with the coordination of conduct through the cortex; and this implies some repression (or rather supersession) of the instinctive by the intelligent life. Here, however, the process is much less complete than in the foregoing examples: "the affective responses of the human infant" are "ready to appear at any period of life if conditions arise suitable to call

[1] IX. 236.

them forth." There has come into existence "a mechanism whereby certain parts of an instinctive mechanism which were useful could be utilised, while other parts detrimental to welfare could be suppressed or dissociated." For example, as to the appreciation of space, "the reactions of the protopathic system are characterised by certain manifestations of radiation and reference to distant parts which are normally absent over those parts of the surface of the body where the earlier instinctive is controlled by the later epicritic system[1]."

It may be questioned whether the appreciation of space by the higher animals is a good example of the "repression" of an old instinctive life: for (1) the "crudeness and vagueness" of protopathic sensibility and its "immediacy and unmodifiability of response[2]"—characters displayed by it when temporarily uncovered (so to speak) by lesion affecting the epicritic system in an organism normally directed by this system—may not have belonged to it in animals in which it was the dominant system. We do not observe in fishes and amphibians any such failures of adjusted reaction as should be expected if their behaviour were directed by crude and vague sensibility. The guidance of their cutaneous senses must cooperate, or at least not conflict, with that derived from other senses, especially their eyes, which have the definite and discriminating character of the epicritic system. But (2), as to the appreciation of space, I venture to suggest that the clear perception of space in three dimensions, such as we exercise and share with the higher mammalia, is entirely a function of the cortex and is unknown to fishes; whose behaviour may be understood as consisting in specialised reactions to variously compounded stimuli. A physiologist might retort that our actions also are to be understood in that way; and that the *consciousness* of space is a mere extra. Very likely; but my reason for distinguishing our case from the fish's is that the impressions from different senses—tactile, articular, olfactory, optical—are not sufficiently coordinated in its old brain to give a 'perception of space' in our sense of the words. Birds probably perceive space by a cerebral apparatus somewhat different from ours; reptiles, too, in a rudimentary way. On the other hand, that active adaptation to the actual conditions of space without perspective—which is all that the old brain was capable of—is by no means a character of the old life that needs repression in our own, but is rather the foundation of our cognition of space, gives our perception that

[1] This *Journal*, IX. 241–2: referring to Dr Head's celebrated investigations described in *Brain*, 1905, 1908.

[2] This *Journal*, IX. 241.

profound meaning which, because it is so ancient and instinctive, makes all analysis seem superficial.

If the old perceptive system is incorporated and superseded rather than repressed, perhaps something similar happens to the old instinctive system. Some repression of the old instinctive life does no doubt occur in man, and also among other mammalian species according to the degree of their intelligence; of which there must be plain indication, could we read it, in the development of the neopallium; yet repression is a by-product of evolution. To understand this one must consider in what manner opposition and possible conflict exist between instinct and intelligence. Instinct, according to a strict definition, is the innate disposition of an organism to react as a whole to certain stimuli, without experience or instruction or awareness of an end, in a way useful to the individual or to the race. The reaction is immediate and unmodifiable[1]. Examples of such instinct are found in all classes of invertebrates, and have especially been studied in insects. All the mating activities of insects are of this type: there is no experience, no instruction, no foresight of the consequences. Similarly with all actions which an animal performs only once in its life: there cannot be a clearer case than the spinning of its cocoon by a caterpillar—say a silkworm. But when an action is repeated several times or even once in a lifetime, there is opportunity of profiting by experience; and there is reason to think that some animals not very high in the scale of organization (*e.g.* bees) do profit by experience: that is to say, in them instinct is enlightened by intelligence. This implies that their nervous system is not strictly organised at birth, but comprises alternative lines of association and reaction. In the higher Primates and particularly man, having a long protected youth passed in play, instincts (as distinguished from isolated impulsive movements, such as wriggling, crying, etc.) never develop into automatic activities; because the organism at birth is not ready for such activities, alternative association-paths are numerous, and all active dispositions come from an early age under the guidance of experience and display intelligence. Except, therefore, those instincts that conform to the definition, there is no natural opposition between instinct and intelligence: they are capable of cooperating; in a normal life they necessarily cooperate—instinct determining the general scope of the ends and supplying the energy, intelligence defining the ends and selecting the means to them.

[1] This definition is very disputable, but is here used to mark the starting-point of the following discussion.

But they do not always cooperate. When a sudden demand is made upon a man in novel circumstances, or when a great access of emotion disturbs him, the result is often an action, or series of actions, due neither to experience nor to instruction and showing very little intelligence. It is usual to call such actions instinctive; and they probably indicate an ancient organization that remains capable of functioning in some sort, though now normally controlled by intelligence. Occasionally a man suddenly called upon to act in a novel situation responds with greater neatness, speed and precision than he could do deliberately; but generally the old instinctive organization has become much impaired for want of selective preservation during all the generations of intelligent life. It was once well adapted to very simple conditions and subserved immediate ends; but the development of the higher animals and man requires an adaptation to more and more complex conditions and to ends more and more remote, such as can only be subserved by increasing intelligence. In this sense, instinct (so far as it functions independently) and intelligence are opposed and liable to be in conflict. And it seems reasonable to suppose that insurrections of instinct represent an old life that was coordinated, with very little intelligence, through the basal ganglia, and that normal activities in which instinct is guided by intelligence belong to the new life coordinated through the cortex. The new life involves, occasionally, some repression or inhibition of old activities, but generally proceeds by a redirection and 'sublimation' of their energy. How is this effected?

In Dr Rivers's examples of repression and dissociation in insects and frogs very remarkable changes occur in the organization of those animals. When a caterpillar, having passed through the chrysalis stage, emerges as a butterfly, hardly anything remains of its former structure, except its central nervous system. The senses are different: ocelli have been replaced by compound eyes with thousands of facets; the antennae have appeared; the muscle and articular senses must be greatly altered; and in correspondence with all these changes the brain develops. At the same time all reactions are necessarily different, because of alterations in the organs of nutrition and locomotion and the development of sex. Indeed, one might hesitate in deciding whether, in such a case, we have to do with one individual or with alternate generations asexual and sexual, as in ferns. Less extensive and thorough is the transformation of a tadpole into a frog; but in both insect and amphibian the advances gained upon the old life and its experience are brought about by changes which make that life impossible. Following this clue, if we are to explain the parallel change in man (and, in degree, in the higher mammals), we must look for a change of structure capable of carrying out those relations of the old instinctive to the new intelligent life which may be observed in him.

In the higher animals, the sensory nerves, which in the lower (say in fishes) terminate in the basal ganglia, are extended into the cortex, and terminate there in special regions; and similarly the purposes subserved by instincts come to be represented in the cortex by structures whose functioning is known to us as ideas of ends or goal-ideas: instincts are incorporated in desires. But such ideas in man are not mere surrogates of instincts; for the most part they are indirect and comprehensive representations of the needs of human nature and of the conditions of human life. Thus there arises a secondary super-instinctive apparatus related to the higher cortical intelligence; whereby reactions are so arranged by comparison of various organic interests that they subserve not the immediate aim of any one instinct but the total good of the animal. In order then that this secondary system may work effectually there must arise such a structure that instinct-exciting stimuli shall normally arouse and bring into action the secondary before the primary system[1]; and that the secondary, when in action, shall drain the energy and thereby supersede the activity of the primary system. So far as the instinctive apparatus still persists as a quasi-independent system, it is normally dissociated and unconscious—normally: but when stimuli excite the emotions beyond a certain degree of intensity, the sudden generation of energy in the thalamus may exceed the capacity of the nervous channels to carry it off in intelligent action; it overflows into the primary instinctive apparatus, now impaired by disuse, excites random movements and confuses a man's behaviour. The disturbance may range from petty indecorums to maniacal rage. Such I take to be the structure, secondary, super-instinctive, which gradually, as it develops, enables the great change to be made from the life of fishes to the new life; and such is its present imperfection.

§ 2. *Dissociation determined by Attention.*

The change from instinctive to intelligent control involves a development of attention, such that it is less attracted by things or events that immediately gratify the instincts than by those which subserve the goal-

[1] Cf. Dr Head in *Brain*, 1911–12, 191. "The low intensity of the stimuli that can arouse the sensory cortex, and its quick reaction period, enable it to control the cumbersome mechanism of the thalamic centre."

ideas. In the attitude of spontaneous attention repression is normal and automatic: when the attitude is strongly set, we cease to hear or see anything irrelevant. In voluntary attention the thought of anything foreign to the object of our contemplation is suppressed; the suggestion of it is resisted. This attitude implies a mechanism, and by turning again to the lower animals we may get some hint of what it is like.

The perceptive powers of an animal often seem remarkably limited: it fails to see or hear—at least, it does not notice—sights or sounds, which it would seem that it must be aware of, if able to see or hear at all. Fabre tells an amusing story of the cigale, which with hosts of its species, male and female, sits on a tree in his garden and 'sings' aloud all the summer day. Since it makes so much noise, it may be supposed to hear it; yet it seems not to hear—for it is quite undisturbed by—whistling, hand-clapping, stone-banging. Fabre borrowed the "municipal artillery, iron boxes that are charged with gunpowder on the day of the patron saint," and exploded them under the tree, like a clap of thunder. The cigale sang on as if nothing had occurred. Examples of a characteristic so well known need not be multiplied. There is a common saying "deaf as an adder"; and in fact an adder does not notice shouting, banging, etc.; but at a slight rustle in the grass it is at once on the alert. The explanation seems to be not that an animal is incapable of seeing or hearing or smelling whatever is unnoticed, so far as the structures of its receptors are concerned, but that the central structure is such that only those sights or sounds or odours that are biologically interesting to the species (or when they are so) are capable of exciting reactions—which for us are the sole evidence of its sensibility. In our own case the double images that fall on our retinae from objects within or beyond the fixation point of the eyes are hardly ever noticed; though their existence can be shown by a very simple experiment. If the consciousness of them were not repressed vision would be confused. For normally convergent eyes, stimulation of the foveae must be prepotent over that of other areas of the retinae (except for sudden movements affecting the margin): whence perhaps the absence of foveae in the eyes of mammals that must be much on the alert for signs of danger or of prey[1]. Hence we may say that any impressions that are rarely of biological interest to an animal, or which would hinder and embarrass its activities, tend to be not perceived or not attended to.

In default of so much intelligence as makes possible a judicious choice

[1] It may rather be that their olfactory sense must not be too much subordinated. In Primates this sense weakens with the development of the foveae.

of objects to react to, an organism is limited automatically to the effective perception of such things as deeply concern its life. It is the narrowness of such a life that makes it possible to leave something to the play of intelligence, as in the hymenoptera. Such automatic limitation is incompatible with higher grades of intelligence; and for want of it we see that some of the higher animals are often dangerously, and sometimes fatally, misled by what is called 'curiosity'—a liability to be attracted by things irrelevant. Yet the scope of attention is everywhere more or less restricted. A very large proportion of the actions of the higher vertebrates are directed by interest in the things that are necessary to them, and involve neglect of all other things seen, heard or smelt; and even in savages and uneducated men (it may be surmised) the limitation of their interests (deplorable as it seems to the philosopher) is a safeguard against the distraction of meddling with things which, if they attempted to deal with them, they could not understand. Under usual conditions, habit, custom, and absorption in narrow interests are a natural protection. Hence, whilst in the lower grades of animal life attention is restricted by instinctive needs, in man it is restricted by the secondary system of ideas by which those needs are represented and controlled. This prevents associations being formed with whatever things or ideas seem irrelevant, and represses all tendencies to activity that are not believed to subserve the goal-ideas. The 'practical man' is he who makes a virtue of such restriction. Curiosity when successful 'pays' by widening the conditions of life that are known and therefore safe; but its success depends on intelligence and is favoured by a protected life—such as a monkey's in a tree.

We saw in § 1 that there must be a cerebral mechanism whereby presentations normally bring into activity the secondary system of goal-ideas, or purposes and desires, before the primary instinctive system; and that the secondary system, when aroused, drains the energy and supersedes the activity of the primary; and we now see that attention is in consciousness the index of such control. Whilst some of the higher mammalia, no doubt, act in some part of their lives under the influence of ideas of what they desire, the characteristic of human life is that in normal circumstances it is almost entirely controlled by such ideas; and, as it rises in culture, its controlling ideas become more comprehensive and their objects more remote. Goal-ideas, whether immediate or derivative, have in men of character a natural prepotency and persistency (compared with the senses) based on their biological utility: human life is possible on no other terms. Failure of such prepotency is want of character. So important is the control of conduct by ideas that nature has taken the risk of its excesses, and at every stage of culture we see many men of not the least penetrating understanding exaggerate the values of ideas, whether in superstition or in philosophy, to the neglect or contempt of the senses; and look how freely men die for ideas. Psychologists generally explain the power of ideas hedonically, saying that the affective tone of ideas is stronger than that of sensations: which, in my experience, is untrue. For me the affective tone of ideas is very rarely equal, or nearly equal, whether as pleasure or displeasure, to that of sensation and appetite: it is neither as intense nor as voluminous; though, certainly, it may be more enduring. The interest of goal-ideas, their power in fixing attention, is the sign of a biological control not specifically represented in consciousness by any proportionate hedonic experience. Negatively, however, there is affective support; for distraction from an interesting pursuit is highly disagreeable.

Ribot in his study of attention has shown[1] how many morbid or insane conditions may be considered as diseases of attention. Hypertrophy of attention appears in hypochondria, fixed ideas, ecstasy; atrophy of attention in intoxication, extreme fatigue, hysteria, delirium, mania. And he observes that morbid hypertrophy of attention can hardly be distinguished at the border from the profound rapture or abstraction of some poets and thinkers. We have seen that in ordinary life attention is circumscribed by certain ideas which represent the innate tendencies of the organism, and exclude more or less effectually whatever solicitations cannot be dealt with. As this attitude becomes morbid in relation to occupations that do not directly subserve life, it tends to repress the consciousness of even necessary ends and means, and its victim cannot live unless he is taken care of. Ideas that cannot come into consciousness are those that cannot be attended to; or if they come with difficulty they are attended to with difficulty; whilst compulsive ideas are those from which attention cannot be withdrawn.

§ 3. *Repression of Desires and Memories.*

So far we have been considering the repression or inhibition of primitive impulses and of solicitations that might distract an animal from the routine of life. At low stages of animal development there is a central mechanism which selects the stimuli to which conduct can be adjusted, and excludes the rest from effective perception. In the higher

[1] *The Psychology of Attention*, ch. III.

animals and in man, where such an automatic process would be incompatible with intelligence, similar results are obtained by the operation of attention under the influence (for the most part) of customary ideas. It is likewise by the operation of attention that we effectuate the repression and dissociation of many disturbing central processes. We live by ideas, but our ideas are often at war amongst themselves. Some stand for comprehensive purposes that call for concentrated and sustained endeavour and give to our short career something like order and unity; others deter us from endeavour or allure us to by-ends, and threaten to reduce our little system to anarchy. This is not a conflict of instinct with reason. Not primary instinct but inflamed imagination is the enemy of our peace—imagination that often has plenty of 'reasons' at command, though its real strength is derived from the incorporation of instinct. Fears and desires of all kinds live in our imagination; and, no doubt, the most prevailing disturber of the peace is sexual desire. In the interest of an orderly life many fears and desires must be repressed so far as to take up no more than a certain proportion of time and energy; and their repression is a problem of attention. First one assumes, toward any one of them that happens to be insistent, an attitude—sometimes expressed in gross movements or gestures—of 'putting aside' or 'turning away,' which is incompatible with the expression of the intrusive desire: then one takes up some task or diversion; and, if this work is interesting enough, the trouble is got rid of; but, if not, the trouble returns. The most powerful of all diversions is the scientific study of the disturbing fears or desires themselves[1], in order to understand their grounds and functions in human life and society. So much the better if this remedy can be supported by 'sublimation,' diversion of their energy into channels compatible with, or subservient to, the main current of one's life, transferring their heat to other imaginations which, because they are in harmony with social requirements and one's own integrity, we call 'reason.' As such attitudes and diversions become habitual, the disturbing ideas have less and less hold on consciousness, though they may never disappear. Moreover, all conflict implies some waste of energy; and, if prolonged, and only partially successful, it leaves a man baffled, stunted, conventionalised and ineffectual, or pestered with eccentricities that bring us to the border of psychiatry. The absence of repression has for the most part obvious consequences; yet has sometimes been compatible with personal and social eminence. Reckless expansion may be less enfeebling than inward conflict. No moralist can praise reckless-

[1] Spinoza, *Ethica*, v. iii. and vi.

ness; but I cannot persuade myself that Mirabeau or Byron would have become a greater orator or poet by practising self-control. This is why the home of creative art has so often been in Bohemia.

The repression of memories follows the same course as the exclusion of desires. One often puts aside or turns away from disagreeable or irrelevant memories, not with any wish to forget them altogether, but merely because they are disturbing and one wants to go on with work or to go to sleep. The process, whether deliberate or impulsive, consists in this: first one checks the current of disagreeable thoughts by assuming an attitude—sometimes expressed by gross movements or gestures—of 'putting aside' or 'turning away': then one thinks of something else; and, if this something else is interesting enough, the troublesome ideas are got rid of; but, if the new current of thought fails, the troublesome ones are apt to return. The attitude of repression operates, I conceive, by counteracting the motor expression (which includes language) of the disagreeable memories; and the new current of thought implies a diversion of energy, and therefore the quiescence of the neural systems involved with the disagreeable memories. No train of reflexion can maintain itself if its motor expression is totally suppressed; and it does not seem fanciful to compare this fact with the failure of stimuli to arouse perception when they have no specific connexion with an animal's normal activities.

To free oneself in this way temporarily from unpleasant ideas is very common and perfectly harmless. If at any time it becomes useful (as it may) to dwell upon the memories thus discarded, they can be recalled, surveyed, and whatever useful lesson they comprise may be extracted. Moreover, unpleasant memories, thus recalled with a serious purpose, lose much of their unpleasant quality. Emotion, says Spinoza[1], can only be counteracted by another and stronger emotion; but by such means it can be counteracted: the affective tone of a serious purpose in the study of one's own experience can completely overpower that of the experience itself, whether pleasant or unpleasant. He who shrinks from such necessary study loses the opportunity of learning this truth. On the other hand, one may try (whether deliberately or impulsively) to forget something entirely; and then one pursues the same method as above described, and renews the effort again and again whenever the objectionable idea appears, until all currents of thought go round it; it is deprived of all expression, prevented from forming fresh associations, is isolated or dissociated, and at last forgotten—perhaps it seems to have been com-

[1] *Ethica,* IV. vii.

pletely forgotten; but in obliviscence there are all degrees. Such repressions of unpleasant experience are, probably, always unhealthy; they are refusals to learn by experience, a sort of mental self-mutilation; and in adults they imply moral cowardice, or (if that be too severe a word) at least weakness and unfitness for life in this world. Moreover, the repression of a memory does not include the destruction or undoing of the structural modification of the cortex on whose activity it depends: this remains, and there remains the possibility that it may be re-excited under certain conditions by appropriate stimuli. It is also conceivable that it should be re-excited without enabling the memory itself to break into the present personal consciousness, manifesting its activity in indirect ways mental and physical, such as Freud and his disciples have described; that it should influence dreams, be recoverable by hypnosis and discoverable by psycho-analysis; that its restoration by therapeutic measures should be resisted by the same motives that suppressed it, and that by its restoration a patient should be greatly benefited.

§ 4. *Psychological and Physiological Explanations.*

Psychiatrists of the Freudian school avoid physiological hypotheses; they urge that mental phenomena must be explained entirely in mental terms by mental antecedents; that a great many cases of insanity display on *post-mortem* examination no cerebral lesion or abnormality—and so on. As to cerebral lesions, it would be absurd to suppose that such changes of structure as may be necessary to the isolation or 'side-tracking' of a neural system with which a certain memory-complex is correlative could be observed in the brain of a dead man—at least, in the present state of science. But are we not justified in inferring that if a memory-system is dissociated so is its neural correlative? And this implies some (however minute) structural modification. The supposed contrast between structural and functional derangement is not (as I understand it) fundamental, but refers to the curative treatment appropriate in each case. A fault is functional when it can best be cured by functional exercise; but such a method of cure is possible because the exercise restores the structure to its healthy condition. A healthy structure functions in a healthy way.

As to the desirability of explaining mental phenomena entirely in mental terms, undoubtedly we ought to put together the series and groups of mental phenomena as closely as possible, describing the texture of the mind as of one material; but to make a science of psychology, or of psychiatry, independently of the animal body and nervous system is not possible; and nothing impossible is desirable. Freudians do not observe their own rule. Dr Bernard Hart, for example, in his brilliant article on "The Conception of the Subconscious" contributed to Dr Morton Prince's collection of essays on *Subconscious Phenomena*, writes that "the general conceptions underlying Freud's teaching...may perhaps be described in our own terminology as follows....These complexes [of unconscious ideas, etc.] are regarded as possessing both kinetic and potential energy, and thus are capable of influencing the flow of phenomenal consciousness according to certain definite laws[1]." And Dr Ernest Jones in this *Journal*[2] says "From the fact of this active opposition [to the resurrection of buried complexes on the patient's part], which can be overcome only by the expenditure of a considerable amount of psychical energy, Freud infers that the obstacle in question is not static, but dynamic in nature, *i.e.* that some force is in operation, etc." It may be asked—where in these passages are any physiological terms employed? There are none; but 'force' and 'energy' are physical terms, and the only objection to inserting physiological terms in a psychological series is that, in fact, physiological terms are physical. To speak of any conscious process as having energy (*a*) is a false description of it, (*b*) makes the word 'energy' ambiguous and impairs its usefulness in physics, and (*c*) suggests to any exact mind that it is a short-hand reference to the correlative neural process to which energy is appropriate. Acknowledged or not, physiological ideas always influence psychological discussions. Every mental series,—*vitalreihe* (I think Avenarius called it), or 'fulfilment of a wish,'—begins with sensation, which has no known mental antecedent or explanation[3], and ends with volition, which can only be manifested by muscular contractions. Apart from movement we know nothing of any other mind (nor, I suspect, of our own), yet movements can never be the effect of impulses or ideas considered merely as consciousness. In psycho-analysis by Jung's association-method all responses and their significant variations are physiological changes that cannot be referred to the activity of mental complexes without 'confounding the categories.' The explanation of the mind always begins with physical stimulation of the sense organs and ends with movements of the body: is it possible to neglect the intervening segment of the reflex

[1] P. 130. [2] IX. 248.

[3] This hardly needs qualification by reason of the exceptional, though interesting, cases in which sensations are the consequence of ideas; nor for the sake of Pampsychism—according to which every stimulus to a sensory nerve and the nerve current excited by it must be conceived to have their own sort of consciousness, which liberates psychic potential as the nerve current liberates physical potential. This is not Psychology but Ontology.

arc? To explain a phenomenon is to assign all the necessary conditions, and amongst the necessary conditions of any mental phenomenon is its neural correlative. Therefore, there cannot be an independent science of Psychology. This is for me an inevitable way of thinking; but it may be one of my many defects to be incapable of accepting as adequate an explanation of anything without a statement of its physical conditions, and to regard a 'pure' psychological account of any event as a good beginning. It is true that very often we cannot definitely get much further.

Perhaps the determination not to deviate into physiology has instigated the attempt to account for several common phenomena in a needlessly paradoxical way: as, for example, by attributing many curious failures of memory, errors of speaking and writing, misspelling and so forth to repression induced by an unconscious aversion to names, letters, sounds, because they have become associated with some disagreeable buried complex. That many failures to remember, or to observe relevant facts, or to feel and acknowledge the cogency of reasoning, admit of this explanation, seems to me very true, and important to bear in mind; but why stretch it to include trifles that may be much more simply dealt with? Dr Ernest Jones, writing in this *Journal*[1], argues that "all forgetting is due in part to repression"; and that the repression may be due either to the hedonic principle to avoid the pain of remembering, or to the utilitarian principle to avoid the embarrassment of useless memories—adding (what seems true) that the intrusion of irrelevant ideas is "a mild variety of *Unlust*." He hardly claims that this can be proved; nor do I see how it can be disproved; and, besides, the qualification "in part" reduces the hypothesis to such modest dimensions that it hardly calls for disproof. But as to such errors as the forgetfulness of names or bad spelling (in one who should know better) there may, I think, be no psychological cause. If we consider the mind as correlative with the functioning of the brain there is an obvious ground for such failures: namely, that the brain is not the only machine that never slips or jams, or suffers from friction, or gets out of repair: fatigue, for example, is entirely physiological. Forgetfulness of names, errors of spelling, are just such faults as may be expected sometimes to occur from the physical imperfections of the organ. I have never been good at remembering names, and my spelling has always been infected with individualism; so that, if Freud's doctrine is true, I might probably be shown to have had, some time or other, a quarrel with many of my friends

[1] VIII. 45–6.

and with every letter of the alphabet. But there is a certain pace at which I write most fluently and correctly: attempting to increase the speed, the characters sprawl; letters, syllables, whole words fall out—because the machinery is out of gear. Errors occurring at my ordinary pace of writing indicate the peculiar weaknesses of this machine. My commoner errors seem to be reducible to the imperfect formation of habit; which is physiological if anything is. As it is reasonable to think that slight cerebral hitches and slips occur, it is reasonable to allow for their consequences, and it is not reasonable to insist on neglecting them. They are represented in personal consciousness only by the faults and perplexities they occasion. Beginning with these trifles, a graduated series of memory-losses could probably be exhibited as due to physical conditions, until it came to a climax in Hanna's case, reported by Dr Boris Sidis in his *Multiple Personality*.

§ 5. *The Concept of the Unconscious.*

There is great difficulty in defining the condition of the Unconscious. Dr Bernard Hart, in his book *The Psychology of Insanity* and in his article on "The Conception of the Subconscious," urges (if I rightly understand him) that the Unconscious is to be considered merely as a scientific concept, not necessarily denoting anything actual, but devised to explain our phenomenal experience in so far as, by accepting it, we are able to deduce that experience. "This train of thought is the analogue of that underlying all the great constructions of physical science—the atomic theory, the wave theory of light, the law of gravity, and the modern theory of Mendelian heredity[1]." It is the nature of science, he says, to end in such conceptions: and so it is, provided the conceptions be good ones—the laws (as of gravity) true, and the entities (as the atom) real. The concept of the unconscious posits an imagined entity; and so far it resembles that of the atom. But my impression is that chemists would be very dissatisfied with the concept of an atom as imaginary only: a large majority of them (I believe) take it to denote a reality, and have the sane and healthy conviction that, if it is not real, it can never explain real phenomena. Similarly, if the unconscious is not real, it can never explain any phenomena of the human mind, normal or abnormal, such as dreams or psychasthenia. Dr Bernard Hart often compares the concept of the unconscious with that of the ether: "An unconscious idea is a phenomenal impossibility just as a weightless frictionless ether is a

[1] *Subconscious Phenomena*, 131; cf. *The Pscyh. of Insanity*, 15–19.

physical phenomenal impossibility[1]." There is not enough agreement about the ether to make it a satisfactory example of scientific concepts; but we may say confidently that, whilst a weightless frictionless ether is a phenomenal impossibility, it is not (in itself) an impossible or contradictory concept; whereas an "unconscious idea" is (in the words) a contradictory, and therefore impossible, concept. The region of concepts, far from being above the level of contradiction, is the only one in which contradiction can occur, and where, occurring, it is fatal. He says that memory is a psychological conception constructed to fill up the gaps in the phenomenal psychic series, not a phenomenon: "we only experience the recurrence of a certain mental process, but assume, to satisfy our demand for continuity, that it has in some way existed during the interval, and we invent the conception of memory to explain this continued existence." The reader, says Dr Hart, may "prefer to adopt the physiological point of view, and to regard memory as the conservation of traces in the brain": but "nervous energy," "permeability of paths," "conserved trace" are not phenomena; they are, like "memory," concepts[2]. Well: I am that reader; and I prefer the physiological concepts because they are good ones, agreeing with other physical concepts; and I reject 'memory' (as a "continued existence" of mental functions) because it is a bad concept, not scientifically constructed but borrowed from popular thought (the "storehouse of memory"), and involving a contradiction not merely verbal, namely, the assuming that mental processes, in their nature transient, can possibly have a continued existence. If we consider that a state of consciousness is always a process, that every perception, idea, feeling or wish appears, develops, disappears and is succeeded by others, it is plain that states of consciousness have no substantive existence like things in space; they have a certain duration in process, but in the intervals of their presence no perduration. Perduration can never be a psychological predicate.

Something, however, must be assumed to be relatively permanent or perdurable in order to explain the reproduction of memories and desires; and, unless we posit the soul as a subject of permanent dispositions, there is no resource but to fall back upon the nervous system as a subject of permanent modifications—traces, paths, groupings, clusters, systems, integrations, or what not. Imperfect as such notions may be, something of the kind, as a hypothetical relatively-permanent ground, is indispensable: and it has the merit of being a constant motive to the investigation of cerebral physiology, which has in the past owed so much to the

[1] *Op. cit.* 132.

[2] *Op. cit.* 1234.

stimulus and direction of psychology. Any state of mind that is not present has no real but only a potential existence; that is, it is dependent on the excitation of certain 'paths,' and 'clusters' of cells in the brain. Even Dr Morton Prince's term "dormant" to describe memories not present is open to the objection that only a thing that exists can sleep, and that such memories have no psychical existence; their neural correlatives do exist and, when not functioning, may be said to be dormant. Many difficulties are obviated if we conceive of the dissociated unconcious as neural dispositions. Such dispositions, if they are not excited, do not function; and, so far, they are in the same case as the neural dispositions that are correlative with all the memories or possible ideas that are not present to us; that is to say, they are in a state of potential. But they differ from these associated dispositions in not being capable of functioning in such a way that their correlative mental complexes can directly enter self-consciousness (or, in some cases, entering, are still regarded as alien), but can only indirectly affect it, symbolically or pathologically. Again, the contradiction in terms exhibited by such an expression as 'unconscious consciousness' may be seen to be merely verbal, if by 'unconscious' we mean unrelated (or alien) to self-consciousness—the consciousness that for the time is correlative with the central control of the bodily organs (or most of them). Such an unconscious consciousness is in fact a 'co-consciousness' (in Dr Morton Prince's happy phrase); and, if the possibility of cerebral dissociation be granted, its existence along with the dominant consciousness in a given organism is not more difficult to conceive of than the co-consciousness of distinct organisms.

The unconscious in some sense is necessarily the source of all conscious processes. Not only wit and genius, dreams and lunacy, but equally dullness and commonplace well up for ever from that dark abyss. Our interests or circumstances fix attention upon a certain matter, and presently there occur to us ideas of some sort. We never, except in rote-memory, know what they will be, can never think what we shall think. If a philosophical Monist says our ideas come from the eternal substance, manifested alike in mind and body, I praise him (of course), but add that, for scientific purposes, we want something that lies open to investigation, and, therefore, point to the nervous system, as that in whose ongoings we hope to find a correspondence with mental processes which will connect them with the system of the sciences, and without which they are all in the air.

§ 6. *Hedonism or Utility?*

Turning to the question discussed in the recent symposium "Why is the 'Unconscious' unconscious?"—it seems to me that (1) as to the instinctive life, though superseded in some of its tendencies, and in that measure usually unconscious, it cannot justly be said as a whole to have become unconscious: rather it has been enlightened, in our normal condition, by being taken up into the secondary intelligent-instinctive apparatus. This has been the work of development under natural selection; and Dr Rivers rightly refers it to utility.

But (2) as to forgotten experiences—that is experiences not recoverable under ordinary conditions (though possibly they may, in some sense, have been registered), one may distinguish four classes: (*a*) those that are forgotten through neglect because they are unimportant, having no special significance for our progressive life, so that there has never been any need to recall them—commonplaces varying little from day to day that demand no attention, such as where I went last Friday week, what coat I wore on the 16th of last January, and so on; which, at most, contribute vanishing increments to abstract or cumulative ideas. Perhaps 99 per cent. of our forgetting is of this kind. Some occurrences were possible only in circumstances that cannot be repeated, and such were many experiences of infancy. But (*b*) these last may really have been forgotten for another reason, namely, that the brain had not yet attained the determinate plasticity that was necessary to their persistent registration: it was, no doubt, plastic in the sense of being easily modifiable, but not in the sense of acquiring the new growth that is necessary to preserve the modification. Memory is not at its best in our earliest years: it would be most disadvantageous if it were so. The position, therefore, is in some measure similar to that of old age, when recent events are forgotten because plasticity has been lost; that is, the power of growth which registers experiences has been lost. In all these cases there is utility (except old age, for which I see no use): to have all sorts of trifles frequently coming to mind would waste energy and embarrass our present activities, as Dr Ernest Jones recognises, and (as he says) it would also be very disagreeable: they must be forgotten for the same reason as errors made in forming technical skill are forgotten when automacy has been attained. (*c*) The temporary forgetfulness of names, incorrect spelling, etc., seem to me explicable, in general, by what may be called organic disrepair, due to many slight causes that have not yet been investigated, and having nothing to do with either use or pleasure. And under the same head must come the serious cases of loss of memory from such causes as alcoholic poisoning, fever, violent concussion, which derange the mechanism on a more extensive scale. But there has, doubtless, been (until recently) a most regrettable failure to observe and consider the importance of (*d*) the forgetting that is due to repression, whether spontaneous or deliberate, and that seems to be due to two chief causes: (i) hedonistic shrinking from intrinsically disagreeable memories in the case of children, weak-minded people, unstable, fastidious and morbidly sensitive people; and (ii) hedonistic rejection of that which has become disagreeable through education or personal development (whether by self-training or the realisation of inherited tendencies) under cultural or social principles of behaviour—according to Plato's maxim, that it is the part of education to make what is good agreeable and what is bad disagreeable. This latter hedonistic repression blindly aims at social utility, but commits the costly error of seeking refuge in ignorance instead of in understanding.

The nature of the unconscious has been so involved with pathology, and has therefore fallen so much into the hands of professional men (who assuredly have special advantages in discussing it), that I only offer these reflexions on it because it may not perhaps be useless to show how the matter looks, in its normal aspects, to an unprofessional student. The therapeutic value of psycho-analytic methods is so great that one ought to try to free them (if possible) from the influence of ideas and associations which often raise a prejudice against them in the minds of both professionals and laity. Is it not possible to assimilate to modern Psychology the valuable passages of Freudian speculation without the strange apparatus that encumbers it? And, if so, will not someone better qualified than myself by a knowledge of Psychiatry undertake this genial task?

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THE ACQUISITION OF MOTOR HABITS[1].

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1. STATEMENT OF PROBLEM.

THE present study of the acquisition of motor habits is concerned with a series of experiments on the maze-running habits of rats. The experiments were planned to show whether rats are hindered or helped in the acquisition of a habit by previous habits of a similar kind. This question was raised by certain conflicting results and opinions on the subject which have been published recently.

The first results bearing directly upon the problem are those of Richardson. While studying the rôle of the different senses in the rat's acquisition of motor habits, she noted incidentally that two rats, who had previously acquired habits, gave abnormally low time records for learning the Sawdust Puzzle Box. This led her to make further experiments which gave the following results: "The comparison of the time records and of the learning curves of each group of untrained rats with a trained group corresponding in age, variety and condition (normal or defective), shows that in every instance...the trained animals made uniformly better records than the corresponding groups of untrained rats[2]."

[1] This study is part of a thesis approved for the Master of Arts degree in the University of London. The writer wishes to express her thanks to Dr Edgell for constant help and advice throughout its preparation.

[2] *Psychol. Rev.* XII. 114.

In 1911 Bogardus and Henke carried out a series of experiments to determine the function of the tactual sensations of the white rat and "to determine the effect of the running of previous mazes upon the learning of subsequent alterations of the original maze by opening and closing definite pathways[1]." They designed the maze which is named *E* (Fig. 1) in the present work. From the results of opening and shutting different passages after the maze had been learned in its original form, they concluded that previous learning is advantageous or disadvantageous according to circumstances, but that on the whole the disadvantages of the old habits rather overshadow the advantages. The analysis of the results is interesting, as showing how alterations in different parts of the course have different effects. An alteration near the beginning is much less disturbing to the rat than one farther on, and alterations which involve entry into former blind alleys present greater difficulty than those which involve a short circuit.

During the last ten years there have been several other studies of rat behaviour in the maze; but they have been concerned chiefly with the statistical aspect of the subject; and are not therefore relevant to the present study. The only one which bears upon it directly is that described in the *Psychological Studies from the Bedford College Laboratory.* The object of this research was to study learning and relearning in mice and rats. The results showed incidentally that animals which are practised learn to solve a new problem more quickly than do animals which are unpractised[2]. Another study, which has been published more recently than this, appears at first sight to lead to a contradictory conclusion. Walter S. Hunter in an article on "The Interference of Auditory Habits in the White Rat[3]" concludes that "habit interference occurs in the white rat between a first habit and the formation of a second one[4]." This result, however, is not necessarily in conflict with the others which have been quoted because Hunter's problem was of a very special character. The first habit which his rats had to learn was to go to the right for handclaps, to the left for silence. When they had learnt this, they had to acquire contradictory habits such as turning to the left for tuning fork. It is obvious that problems such as this, which involves attaching a certain meaning to a stimulus, then learning to attach the very reverse significance to it, are not on a level with the ordinary acquisition of motor habits. In fact it seems questionable

[1] *J. of Anim. Behav.* I. 125.

[2] *Psychological Studies from the Bedford College Laboratory*, 1915, I. 10.

[3] *J. of Anim. Behav.* VII. 49.

[4] *Ibid.* 65.

whether the term 'habit' should be employed at all in such a case as this. The term Hunter uses—'auditory habit'—certainly calls for explanation. The rat did not form a habit of going any one way; he learnt which way he ought to go when he received a specific stimulus. The problem is one of the acquirement of meaning. The results of Hunter's first group of experiments, which show that the rat did learn to attach meaning to the alternative signs, are extremely interesting.

This brief survey of the work bearing on the subject affords evidence that, on the whole, the formation of habits facilitates the acquisition of other habits resembling them. Writers have, however, tended to ignore this and to regard the process as extremely automatic. Thus Smith says that the maze habits of rats are automatic in character and that "rats do not learn mazes with any greater facility after having had experience in previous mazes[1]." In another place she says, "The acquirement of modification in the form of a habit appears to render the establishment of subsequent modification of a similar character more difficult[2]." The experiments which are described in the next section were planned to test this statement and to analyse, if possible, the factors involved.

2. DESCRIPTION OF METHOD.

Mazes were chosen for this study because they involve a kind of activity perfectly natural to the rat, and have the additional advantage of admitting of endless variety without necessarily involving any novel activity; whereas it is difficult to make a series of puzzle boxes of even approximately equal difficulty.

The original plan of experiment was to use pairs of mazes, to let one rat learn one of these and then to test him and an unpractised 'control' rat in the other. While the first rat was learning his maze, the control rat was taken out of his cage every day and made to find his food in different places. This was done to ensure that the control rat's learning was not hindered by the novelty of being handled, and of running outside his cage. This plan was abandoned after the first series of experiments, because it meant that for a large part of the time only half the rats were supplying records, and the control rats who were not learning mazes required more time and attention than the others. The general plan adopted afterwards was to let a pair of rats each learn a different maze. Then they were changed over and each learnt the maze which had been learnt by the other. This affords a comparison between the

[1] *Mind in Animals*, Cambridge, 1915, 41. [2] *Ibid.* 46.

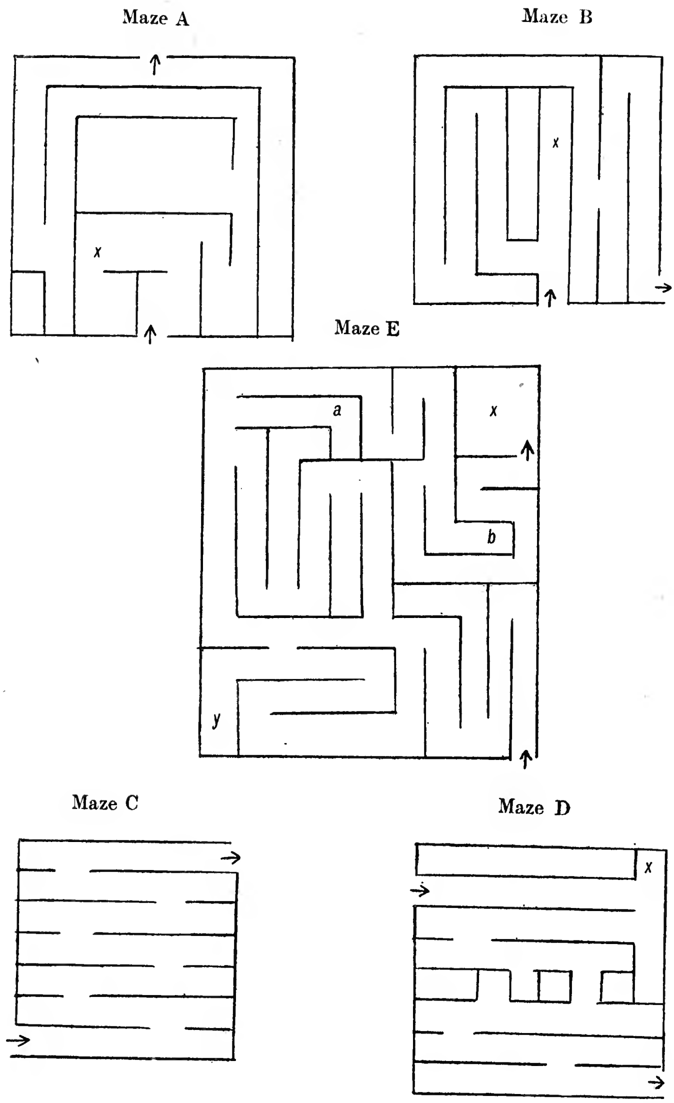


Fig. 1. Plan of Mazes A, B, C, D and E.

learning of each maze by a practised, and by an unpractised rat, and also between the work of the same rat practised and unpractised. In the first instance care was taken to have pairs of mazes with the same number of turns and possibilities of error, but it is not certain that this ensures their being of equal difficulty for the rat.

Fig. 1 shows the mazes which were used. *A* and *B*, and *C* and *D*, respectively, were planned as pairs. Experience showed, however, that all four were of approximately equal difficulty. The last series of experiments was performed with a copy of the maze used by Bogardus and Henke—Maze *E*, Fig. 1. This maze is much more difficult than any of the others, a comparison between it and them being therefore impossible. It was learned by six practised and six unpractised rats, and when they had all learnt it to varying degrees, the position of the food was altered from x to y, while the rats were still free to run to x. As will be seen in a later section, the results for this maze were more suggestive than those for any other.

All the mazes were made of wood. The walls were six inches high, and the alleys uniformly four and a half inches wide. The mazes were scrubbed at frequent intervals with a very strong solution of Jeyes' Fluid, and the floor and walls were covered with clean grease-proof paper every time a rat ran in the maze, except in the case of maze *E*, where the paper was omitted and the maze was rubbed over with Jeyes' Fluid after each rat had run in it. The paper and Jeyes' Fluid were used to guard against the rat finding his way to the food by following his or other rats' tracks. As, however, it is a matter of controversy whether any method completely removes a rat's tracks, the argument for his not using this method of finding his way will be based on his behaviour, and will be considered in the next section.

The rats were carried in the hand from the cage, and placed in the entrance to the maze. At the end of their journey they found a little food which they were allowed to eat at the exit, and then they received their food for the day immediately afterwards in their cage. They had only one trial on each day. The rat's course was traced on a diagram of the maze while he was running and the time from entry to exit was taken with a stop-watch. The cages in which the rats lived were large and composed chiefly of wire on which the rats climbed most of their time. This kept them lively and nimble. All the rats used were males, and they were not put into the maze unless they were in good health.

Throughout the whole of the experiments there were only four rats which behaved abnormally in the maze. The first of these, *M*, showed great fear both of the operator and of the maze. Experiments were begun with him in the hope that he would lose his fear. At first he behaved very wildly, and tried persistently to climb out. Then he went to sleep on two or three successive occasions. One day, when probably he was particularly hungry, he ran about and found the food, and it looked as if he might learn the maze, but a day or two afterwards he was found dead in his bed box. This was the only rat to die suddenly without showing any sign of illness.

The second rat that behaved abnormally was rat *J*. From the first he took a very long time to run the maze and his learning curve is very irregular. It was noticed that when he found the food he did not eat it. Later, he showed signs of ill-health, and was not made to run any more mazes. In the other two rats there was no indication of illness to account for the behaviour, but in each case the rat showed little or no tendency to run about in the maze. The first of these, rat *I*, found the food once, but on the following days he settled down to sleep directly he was put into the maze. As he showed no sign of illness the experiments were continued. It had been noticed that rats tend to follow a slowly moving object, and this seemed a possible way of leading him to the food. The operator's hand was therefore drawn slowly away from him, and he was thus led gradually through the maze. After this, he behaved quite normally and his learning curves are comparable with those of the other rats. The fourth case was similar to this, only that on four successive occasions the rat did not find the food in half-an-hour. On the fifth occasion he was led through by hand with the same result as with the previous rat.

The twenty-two rats, whose records form the bulk of the following results, showed no disturbance at being put into the maze, and after a very few experiments they strained to get into it when they were brought near. If on any particular day a rat showed signs of being frightened, he was removed and tested later, or the experiment was omitted for that day. At first, the only signs of fear recognised were starting or running wildly, but later experience suggested that the position habits mentioned by different experimenters might be a sign of fear or some other abnormal mental condition. The rat, for instance, might persist in trying to jump up on to the partition, however many times he were checked. If left in the maze, he was likely to acquire a permanent habit of doing this, but if lifted out and given a little food or merely stroked, he usually behaved quite normally on his return. After this was discovered, a rat was removed from the maze at the very first sign of a position habit.

The position habit would be an interesting subject for investigation. It seems to be a form of circular reaction, and if it is allowed to go on for long, the rat's eyes have a curious 'glazed' look, much like those of a person mesmerised.

3. STATEMENT OF RESULTS.

(i) PRELIMINARY CONSIDERATIONS.

A number of papers have been written recently about the relative merits of the different ways of recording the rat's learning in the maze. It can be recorded in time taken, in errors made, or by a combination of the two. Furthermore it is possible to rate errors in a variety of ways. No doubt the way in which the results are expressed must depend on the object in view. If all that is needed is an indication of the rate and degree of improvement, the time record is adequate and the curve which it gives will correspond very closely with the error curve. The time curve has the advantage over the error curve that it does not involve any arbitrary judgment on the part of the experimenter. In counting errors, on the other hand, he must decide to count all mistakes as equal or to make distinctions between them. Whichever he does will more or less gravely distort the results.

The main objects of the present research are to ascertain whether practised rats learn a new maze more quickly than unpractised rats; and whether there are any characteristic differences between the behaviour of practised and of unpractised rats in a new maze. For the first of these purposes time records are adequate. For the second, consideration of the different kinds of errors made is necessary. Errors will be considered in two groups, those which consisted in entries into blind alleys and those which consisted in returns on the same path. Within each of these groups no distinctions will be made, because, while it is certain that entry into one blind alley is not always on a level with entry into another, it is not possible for the onlooker to rate the difference. In the same way it is impossible to say whether one return on the path should be considered of different value from another. In maze *E* a third kind of error was possible, viz., running a longer route to the food than was necessary. The instances of this will be shown in the curves for maze *E* (Table I).

As the object of the present study is the acquisition of motor habits, the records for the first few occasions on which a rat succeeds in running a maze will be all that it is necessary to consider. In most cases the rats continue to run in a maze until they have done so on at least three successive days without error. The records show that after this point the rat makes only occasional mistakes if any, and the time taken decreases to a minimum at which it remains. The fact that some rats find the food on the first day on which they are put into the maze, while others do not find it for two or three days, introduces a difficulty. If the learning is counted from the first day, the rats who do not find the food are placed at a disadvantage because until they have found it there is no incentive to learning to run the maze. On the other hand, if the learning be counted from the first time each rat finds the food, the rats who do not find it for the first two or three days are at an advantage because, during those days which do not count in their record, they are running about and becoming familiar with the maze and its intricacies. In the results that follow, the second alternative will be accepted—the learning process will in every case be supposed to start with the first day on which food is found. This will be done because it throws the advantage on to the side of the unpractised rats, who were the ones, if any, not to find the food on the first occasion. This will make any superiority shown by the practised rats more significant.

In carrying out the experiments great care was taken to ensure that the rats, whose results were to be compared, should be as far as possible on the same footing. For this reason only males were used. The experiments were performed at the same time each day. The rats compared were usually of the same age, from the same family, and working at the same time of year. None of the results is from a rat under three months or over twelve months old.

It is obvious that the records would be of very little use as showing learning if it were possible that the rats found their way to the food once by chance, and then on later occasions followed their own tracks. For this reason it is an important preliminary to any study of learning such as the present to establish that tracking by scent is out of the question. To ensure this the mazes were lined with paper and scrubbed with a solution of Jeyes' Fluid, as has been described in the preceding section. Some experimenters, however, appear to doubt whether any precautions are completely successful in removing the rat's trail from the maze. It is, therefore, necessary to show from actual behaviour that tracking cannot explain the rat's learning in the present research.

For this there seems to be abundant evidence. One of the best records for learning a series of mazes was given by rat *T*, who was the first to enter them after they came from the carpenter. This is well seen in Table IV, where the individual records may be compared.

The table shows also that the learning curves for different rats at the same stage of practice are extraordinarily similar, which would not be the case if an unpractised rat tracked a practised rat, or any one rat tracked his own previous course. In the latter case shortening of the path would be unexplained. In the former much greater irregularity than is shown in the records would result. Apart from these particular considerations, it is to be noted that the problem of the present work is one which would be affected least by tracking. Tracking might account for habituation, but it could not account for the transference of improvement in learning from one problem to another. Indeed, this transference, if established, might be taken as in itself confirmatory of Small's[1] and Watson's[2] conclusion that olfactory clues are not essential to the rat in running the maze.

On the other clues which the rats may use it is unnecessary, for the purpose of the present study, to dwell. The rats may have discriminated the passages by visual, tactual, kinaesthetic or distance clues, although it is difficult to see how they could use visual or tactual clues when the mazes were lined with paper, the surface of which would be different every day. The important question for the present enquiry is not which clues the rats used, but whether they learned to use them more effectively with practice.

(ii) Statistical Summary of Results.

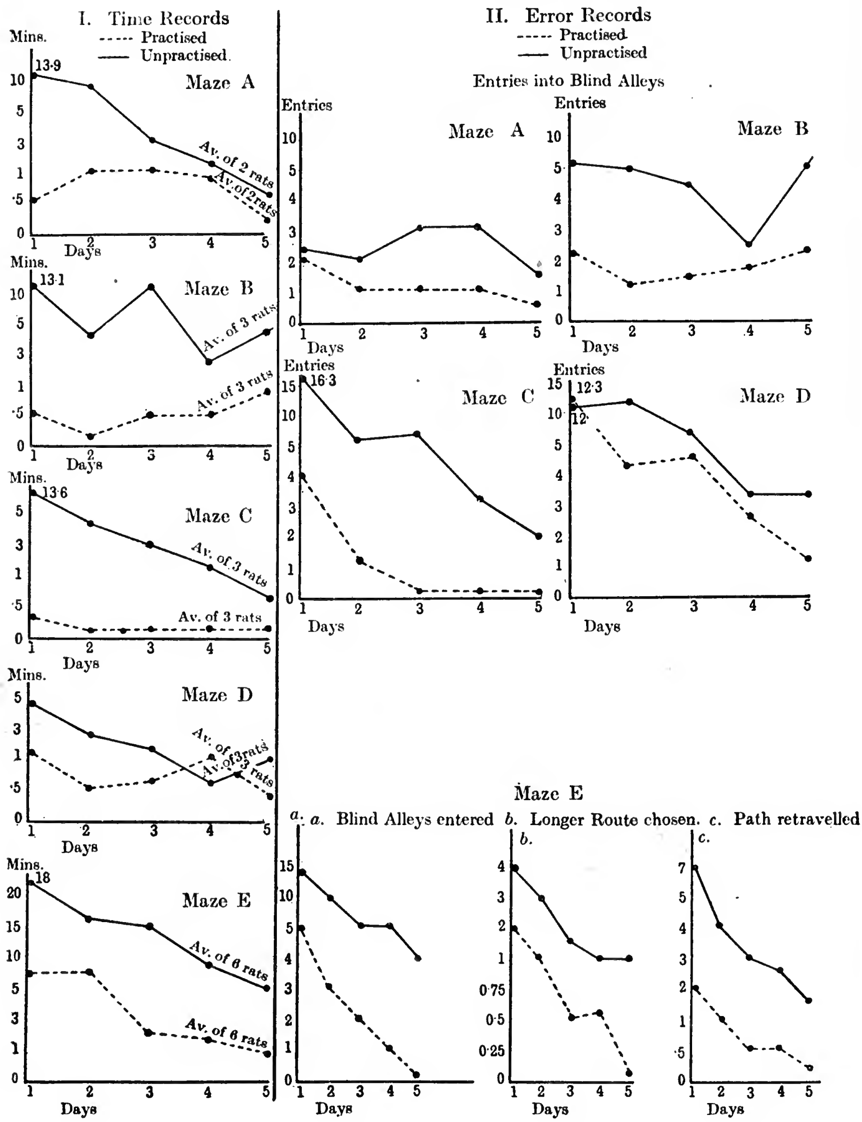
(*a*) *Records of Practised and Unpractised Rats for the same maze.*

The graphs in Table I represent the learning curves of groups of rats practised and unpractised. There were three rats in each group except in the practised and unpractised groups for maze *A* in each of which there were only two; and in the group for maze *E* in each of which there were six. The records are founded on the work of twenty-two rats, most of which figure as unpractised for one maze and practised for another—a fact which makes it improbable that any difference between the two should depend on individual differences in the animals. The practised rats had sometimes run one maze previously, and sometimes more than one. The curves show a constant and considerable difference between the work of the practised and the unpractised. The difference between the time taken by practised and by unpractised rats is much greater than the difference in time for different mazes, in spite of the fact that the path to be travelled in them varied from 11 ft. to 31 ft. It

[1] *Amer. J. of Psychol.* XII. 213. [2] *Psychol. Rev., Monogr. Suppl.* VIII. 91.

Table I.

Learning of Mazes by Practised and Unpractised Rats.



is evident that the improvement in learning may be due to the more rapid running of the practised rats, to their making fewer errors, or to a combination of these two factors. That they ran more quickly is certain; any one who has experimented with rats can tell whether a given rat is practised in maze-running by the rate at which he runs.

The error curves in Table I indicate that the practised rat enters blind alleys less often than the unpractised. Not only so, but the more mazes he has learned the more this is the case. Thus the greatest difference between practised and unpractised occurs in the case of mazes *C* and *E*. The practised rats for both these mazes had learnt more mazes previously than the practised rats for any other maze. The three practised rats for maze *C* had learnt three other mazes; the six for maze *E* had learnt from four to seven previously. The difference in the number of blind alleys entered by practised and unpractised rats is not confined to any one part of the learning process. If it were characteristic of only the first day or two, it might be that the greater emotional disturbance of the unpractised rats caused them to run about more blindly even though they did not show signs of fear. The curves show, however, that the difference is at least as great on the fifth as on the first day.

In maze *E* the rats had to choose between a shorter and a longer path in two cases. Curves *b* show the average number of times each group ran over the longer course. The practised rats learnt to choose the shorter path much more quickly than did the unpractised. The rats returned on their paths more often in maze *E* than in the others; but here again the practised rats wasted much less time than the unpractised, as is shown by the curves *c* for maze *E*.

The effect of particular motor habits may be studied from three crucial instances which the mazes afforded. Rats which learned *B* after *A* might be expected to show a particular tendency to overlook the first turn to the left and run along alley *x*; rats which learned *A* after *B* might show a tendency to turn round to the left at the first opportunity; rats which learned *D* after *C* might turn to the left into *x* (Fig. 1). The following figures show that, on the whole, the practised rats who might be expected to show these survivals of old motor habits did not enter the blind alleys in question nearly as often as the unpractised.

TABLE II.

1. *Entries into x in Maze B.*

(*a*) Totals for 7 rats who immediately before had learnt maze *A*.

Days	1	2	3	4	5
	18	6	4	4	4

Total entries 36

(*b*) Totals for 7 unpractised rats.

Days	1	2	3	4	5
	27	16	12	10	17

Total entries 82

2. *Entries into x in Maze A.*

(*a*) Totals for 3 rats who immediately before had learnt maze *B*.

Days	1	2	3	4	5
	7	3	1	0	0

Total entries 11

(*b*) Totals for 3 unpractised rats.

Days	1	2	3	4	5
	7	6	0	3	3

Total entries 19

3. *Entries into x in Maze D.*

(*a*) Totals for 6 rats who immediately before had learnt maze *C*.

Days	1	2	3	4	5
	8	3	5	3	0

Total entries 19

(*b*) Totals for 6 relatively unpractised rats[1].

Days	1	2	3	4	5
	9	7	2	1	1

Total entries 20

(*b*) *Records showing the effect of altering the position of the food in Maze E.*

When the rats who had learned maze *E* had mastered it in varying degrees, the position of the food was altered from x to y (Fig. 1). The results show that the rats who had formed the most perfect habit of running the maze in its original form were as a group the quickest to adapt themselves to the altered position of the food. The following table shows this in the total time taken by the rats for all the five occasions on which they ran in the altered maze.

The way in which the rats behaved after the alteration in the position of the food is interesting. There was only one rat, *C*, who never learnt to run directly to the food at y. He was a very quick runner and dashed round to the original food box and usually tried to climb the partition and get out on to the table when he did not find the food. Then he would

[1] Three of these rats had run a few times in a maze which would favour turning in the right direction.

dash back to y. It is to be noted that when the rats found no food at x they returned and explored blind alleys which they had previously learnt to avoid. Another significant feature is the way in which rats *I*, *XI*, and *IV* ran a step or two of their old route and then returned to where they had found food more recently.

TABLE III.

Rat	No. of mazes run previous to *E*	No. of correct runs in original maze *E*	Total time for 5 days in modified *E*
B	7	8	6·45 mins.
III	4	5	2·35 ,,
I	7	4	3·43 ,,
C	4	3	7·2 ,,
IV	4	3	12·1 ,,
X	0	5	25·4 ,,
10	0	3	14·6 ,,
Y	0	2	11·9 ,,
Z	0	0	55·8 ,,
XI	0	0	32·0 ,,

(*c*) *Records showing the work of each Rat in the order of Performance.*

These records may be divided into two groups. The first is composed of a small number of rats who worked in 1916 (Table IV). There were only a few working at a time and none of them learnt more than four mazes. Rats *T* and *P* have the longest records, and a glance at the curves of their learning shows how few errors they made in the later mazes. Rat J_2 learnt only one maze, but his record is given for comparison with other unpractised rats' curves. Table V shows the time curve of the other group. It was composed of eight rats working under particularly uniform conditions. It will be seen that rats *A*, *B*, *I*, *II* learnt the same number of mazes, but that *A*, *B*, and *I*, *II*, respectively, learnt each pair of mazes in reverse order. This was to determine whether differences in learning were due to differences in the mazes. Rats *C*, *D*, *III*, *IV* learnt fewer mazes than the others and spent longer at them. The intention was to see whether those who spent a longer time at one maze were hindered in learning another. Apparently they were not, but it must be admitted that the difference in the time spent was small.

(iii) GENERAL CHARACTERISTICS OF THE RATS' LEARNING.

The records that have been under consideration show that rats improve with practice in their ability to acquire motor habits, and that any hindrances to learning which may be offered by the survival of old

TABLE IV.

Records of Errors made by Rats in Learning a Series of Mazes.

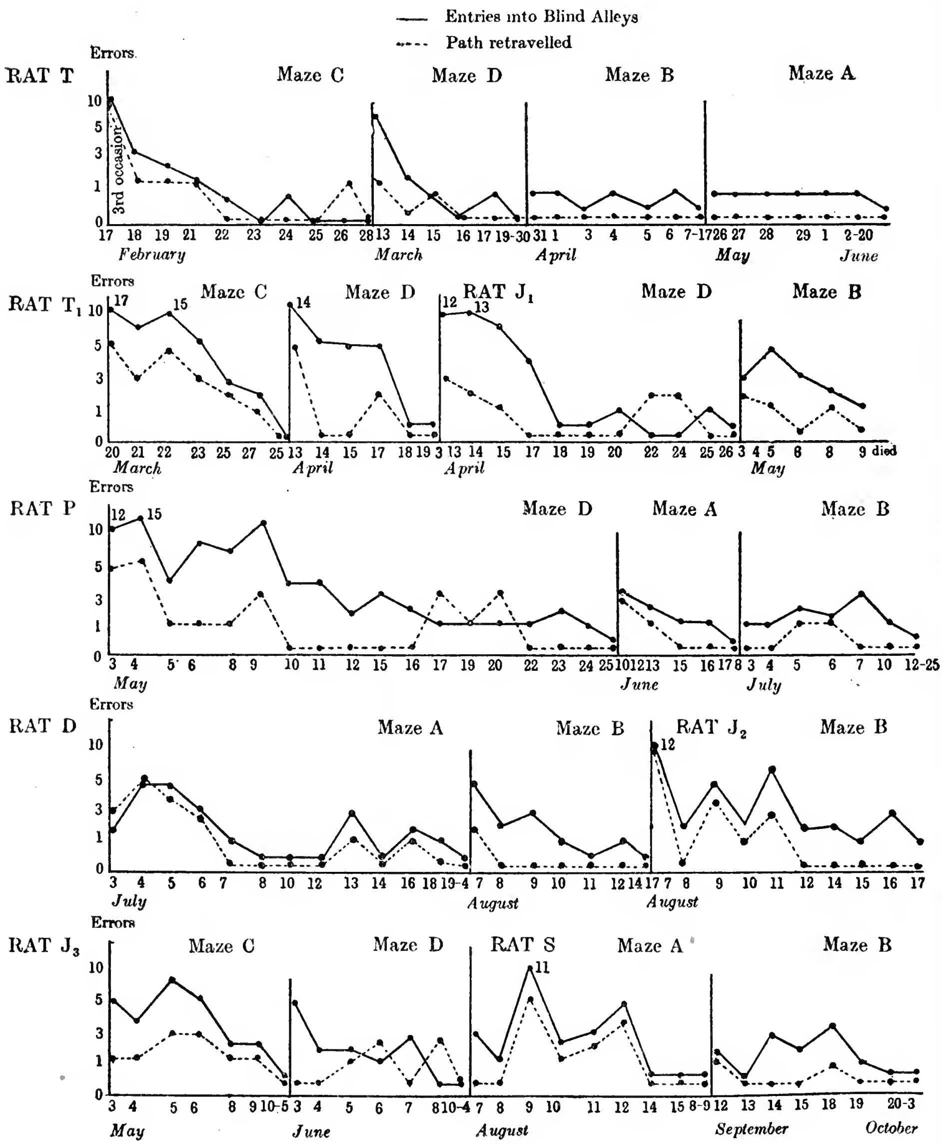
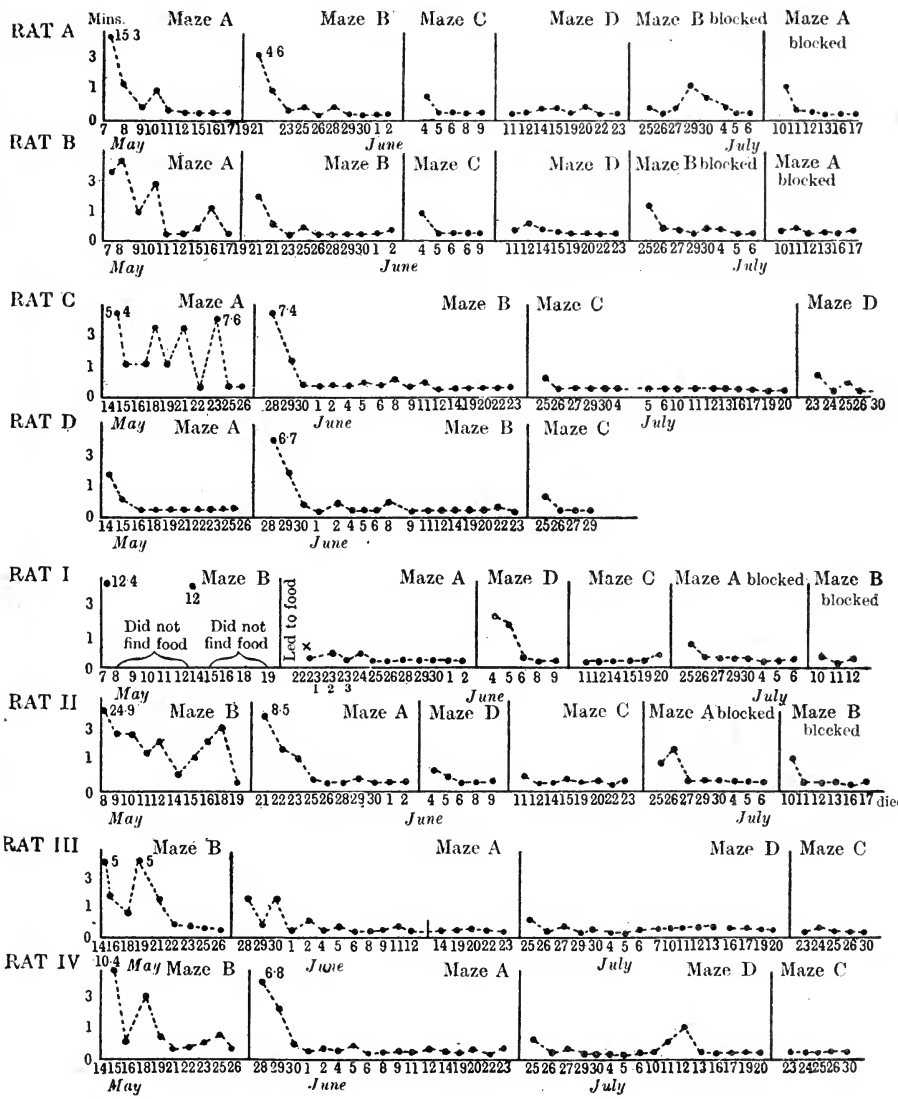


TABLE V.

Time Records of Individual Rats Learning.



× First occasion after being led to food.

habits are more than counterbalanced by the mastery which the practised rats gain over the general situation. Analysis of the records has shown that this improvement is due to several factors. The practised rat runs more quickly; he enters blind alleys less often; he very seldom returns on his path; if there is a choice of paths, he learns, more quickly than the unpractised rat, to choose the shorter one. There is another characteristic of the practised rat's behaviour which it is not easy to establish statistically, but which becomes apparent in a series of experiments. The practised rat seems much less upset by making a mistake than the unpractised rat. This characteristic is probably one reason why he seldom returns on his path. It is well exemplified in the running of maze *E*. Here, after the six practised rats had learnt the correct path, they would sometimes run into blind alley *a* or *b*. Without a moment's delay they leapt the partition and continued on their path. If the unpractised rats made the same mistake, they always ran out of the alley and often wasted time running back and forth, or right back to the starting-point. This difference suggests the analogy of the beginner and the accomplished pianist. When the former has learnt a piece of music by heart, one mistake may oblige him to go over the whole piece again, whereas the latter glosses over his mistake and finishes with very little disturbance.

Despite this characteristic, there are signs that the practised rat is affected by unusual features in his path. He would sometimes start if the paper bulged or if ends of it stuck out, and he showed signs of disturbance when the cover was left off the maze. When the practised rats were learning maze *E* which had a much longer course than any of the other mazes, they crouched and ran very slowly when they had traversed a distance greater than that at which they were accustomed to find the food.

4. THEORETICAL IMPLICATION OF RESULTS.

Writers on the maze habits of rats have, on the whole, agreed in regarding the learning as a process of 'stamping in' right reactions. The results given in the last section challenge such a view. If, as Thorndike maintains, the rat's experiences are stamped in upon his organism quite mechanically so that he 'feels like doing this' and 'not doing that' without any appreciation of the situation, there is no reason why he should improve in learning from one maze to another, except in the matter of running more quickly. The fact that with fewer trials the rat avoids running into blind alleys together with other facts already mentioned, makes it apparent that the situation has 'meaning.' Meaning must always be ideational in character. It points beyond the immediately given and involves a non-sensational element. The ideational factor may be of the simplest kind; it is almost certainly 'tied' with, and 'implicit' in the sense data. In his chapter on "Laws and Hypotheses for Behaviour[1]," Thorndike describes what he considers to be the explanation of the learning. He formulates two laws. The Law of Effect is that: "Of several responses made to the same situation those which are accompanied or closely followed by satisfaction to the animal will, other things being equal, be more firmly connected with the situation; so that, when it recurs, they will be more likely to recur; those which are accompanied or closely followed by discomfort to the animal will, other things being equal, have their connections with that situation weakened so that, when it recurs, they will be less likely to occur. The greater the satisfaction or discomfort, the greater the strengthening or weakening of the bond." The Law of Exercise is that: "Any response to a situation will, other things being equal, be more strongly connected with the situation in proportion to the number of times it has been connected with that situation and to the average vigour and duration of the connection[2]."

Presumably the rat's behaviour might be expressed in the terms of Thorndike's theory by saying that the animal is driven to act by the discomfort of his situation. This action is accompanied by feelings of activity, which Thorndike calls 'impulse,' and the action is continued until satisfaction is reached. On later occasions satisfaction is attained more quickly because the rat "feels like doing certain things and not doing certain others." Thorndike expresses this hypothesis in his review of Small's work—"An animal may feel like going down a certain path, or feel like hesitating, or feel like doing one thing where he could do either of two or more and still have no images or ideational consciousness whatever[3]." To use the 'open sesame' of this theory, the right reaction becomes 'stamped in.' It is not of great importance to ask why Thorndike in his earlier writings so carefully insisted on the psychic character of impulse when it is merely a feeling which accompanies innervation and is not even Man Friday to the mental Robinson Crusoe of satisfaction and dissatisfaction. The question which demands an answer is how we can conceive of this 'feeling like' taking certain paths, etc., as the resultant of purely physiological changes.

Stout says that Thorndike "in his eagerness to reject the operation

[1] *Animal Intelligence*, New York, 1911, 241. [2] *Ibid.* 244.

[3] *Psychol. Rev.* VIII. 644.

of free ideas has run to the opposite extreme[1]." The difficulty to which this gives rise is nowhere better illustrated than in Thorndike's treatment of impulse and ideo-motor action. Dwelling, though he does, on the impotence of ideas to produce action, nevertheless in his definition of impulse he says that the act "in this aspect of being felt as to be done or as doing is in the animals the important thing[2]." This may be true enough, but action cannot be 'felt as to be done' except through ideational elements. No twitchings of muscles, or other physiological preparations, are sufficient apart from the prospective reference and the consciousness of meaning. Thus, from Thorndike's own argument, the ideational element is 'the important thing.'

The Behaviourist School does not appear to realise the fact on which the *a priori* claim for the presence of ideas is based. It is the fact that the animal's action is influenced in an appropriate manner by something which is not a state of his organism. This distinction between being something and having the something as an object is possible only in cognition. Wherever the distinction can be made, and it can be made in the simplest cases of impulse, there purely mechanical explanations are out of court.

The mnemic or biological theories of memory might be supposed to overcome this difficulty and to show, without necessitating reference to non-physical factors, how experience tells on present behaviour. This is the purpose of Semon's theory. He holds that every excitation brings about a change in the 'energetic situation' of the organism. This change has a lasting effect, the trace of which is called an 'engram.' It is physical in nature. The engrams of all the different excitations received during any one 'energetic situation' tend to be associated in such a way that the partial return of the situation may excite any or all of them. This action is called 'ecphoric.' When a stimulus is repeated, it does not merely excite an already existent engram; it creates a second. The two are conceived as vibrating together—a condition which Semon calls 'homophony.' If the two are very much alike the homophony is congruent; if they have important differences, it is incongruent. To this relation of congruence or incongruence the organism may react in a specific manner.

It is difficult to apply Semon's theory to the case of the rat because the incongruence is initiated by his own action and exists between only a part of the original, and the second engram. If, for instance, a rat has

[1] *Manual of Psychology*, London, 1913, 382.
[2] *Psychol. Rev.* VIII. 37.

run into blind alley x one or more times, we may say in Semon's terminology that the various stimuli which he receives from the maze have affected him engraphically. The next time, perhaps, he avoids the blind alley. On this occasion the stimuli act ecphorically on the previous engram and the rat's behaviour follows accordingly, but at the critical point a new reaction appears. There is no congruence or incongruence in the engrams concerned to bring about the change, except in relation to an often far-distant object, namely, the food. Semon states this difficulty[1] in general terms and gives an answer which, it seems, amounts to the admission that his theory cannot explain it. This means that his attempt to explain such behaviour as the rat's in purely physical terms breaks down at the same point as Thorndike's.

Washburn, who agrees with Thorndike's view, alleges, as special reasons for denying that any kind of ideational process is present, that the elimination of useless movements is slow; that the method is not such as to involve images; that some of the mistakes made by the rats are such as would be least likely to occur if the animals depended on any kind of image[2]. These objections will be considered in order.

The view that the possession of ideas should lead to the immediate elimination of useless movements, once a problem has been solved, is frequently put forward. But it does not hold even for human beings. The idea of how a problem has been solved the first time may be very vague, and may help merely in the selection of likely actions and in the elimination of some of the errors on the next occasion. Or again, even though the idea has been clear in the first solution, in the second performance attention may stray for a time, or the idea of the correct solution may be difficult to recall. It seems then wholly illegitimate to argue that because the rat, after having solved the problem once, does not necessarily do it without mistakes the next time, he experiences nothing corresponding to ideas.

Washburn has brought as an argument against the view that rats depend on images, the fact that it is very difficult to conceive of an image of the desired kind which would be sufficiently definite to help. The conceivability of these images appears to be largely dependent on individual experience. It is a subject into which definite experimental inquiry would be both interesting and valuable. A human problem which comes very near to the rat's in threading the maze is finding one's way about in underground railway labyrinths. Some individuals find this

[1] *Die Mnemie*, 3rd ed., Leipsic, 1911, 384.

[2] Washburn, *The Animal Mind*, New York, 1908, 225 ff.

very difficult and rely to the end on the written signs; but others appear to have kinaesthetic images and images of orientation which guide them in familiar places and mislead them in strange ones. In the case of rats who spend their lives in subterranean windings, possibly it is just these kinaesthetic images and images of orientation which are clear, while their visual images have the blurred undiscriminated character which *we* associate with images of organic sensations.

It is difficult to discuss Washburn's third argument, based on the alleged fact that rats continue to make mistakes which would be the very first to be eliminated if they had images. She quotes only one instance, the case of Small's rats who persisted in taking the wrong turn at the beginning. Small suggests that this was just because the turn came at the beginning before the rats had settled down to work, as it were. The suggestion seems plausible. Although the first choice might be expected to make a strong impression as being the first, greater emotional disturbance and the lack of incitement at the beginning may be counteracting influences. For the rest the results do not show the inexplicable survivals of useless movements of which Washburn speaks.

If we now seek to define more closely the psychological elements which, as we have maintained, are necessarily involved in the rat's behaviour, we are led to some such analysis as the following. On the first occasion the rat runs about either from his impulse to explore or from his want of food. Sooner or later food is found. Usually one experience of finding food is sufficient to form an association between being put into the maze and food. That the association is soon firmly fixed is shown by the way in which the rats struggle to get to the maze when they are held near it. Once the association has been formed, the idea of food which is aroused by the perception of the maze acts as a regulative factor. It is impossible to say what form the idea of food takes. The form is immaterial as long as it is sufficiently definite to act as an incentive, a reference to a beyond.

When the rat comes to a critical point, let us say on the first occasion of omitting a useless run, it does not seem possible to say what are the factors which determine his choice. In a fully developed consciousness there might be the recognition of the passage as that first turning on the right, or that passage with the dirty finger-mark on the right-hand side. But it would be violating the Law of Parsimony to credit the rat with such discrimination. He is more likely to judge that 'this-turn-coming-within-the-distance-I-have-just-run' does not mean food. Such colouring of his perception by a revival of past experience is a case of 'acquirement of meaning.' In some cases it seems as if the rat must discriminate by extra-organic clues, and it is of course possible that he always does so. An instance, in which it seems necessary to suppose that he thus discriminates, is that of choosing the direct route to the food in its altered position in maze E. In that case he has no organic clue to determine him to enter x without running round to y. Some aspect in the perception of the entrance must have become associated with the idea of the food so strongly as to inhibit the habitual course.

Hunter's results, which have been quoted in an earlier section, are conclusive in this matter. We cannot suppose any organic state of the rat to correspond to a 'tuning fork' or to 'silence,' and Hunter's rats showed discrimination even between different auditory stimuli. Signs of a similar kind of discrimination have been reported in the *Bedford College Psychological Studies*. A rat which had learnt the Sawdust Puzzle Box and the Spring Door Puzzle Box appeared to discriminate between them by stretching up as if testing the height of the box. "The behaviour was interesting as although it might be insignificant, just one of the many trial movements, it suggested the rudiments of a practical comparison between x and not x. x-ness being in terms of muscular experience and sufficiently distinct in the complex of experience to influence behaviour[1]."

5. CONCLUSIONS.

The experimental results which have been described in this paper seem to show conclusively that in his acquisition of motor habits the rat is not a mere machine. If the analysis based on these results is correct, it does not seem possible that the rat's acquisition of one motor habit should ever interfere seriously with his acquisition of another. Habituation will, of course, count for something. The rat will be likely to try to run his accustomed course in the new maze, but a very few experiences will suffice to cure him of this. If it be objected that in the experiments which have been described the rat was not given sufficiently long at any one maze for the running to become automatic, it may be admitted in reply that more work on this subject is desirable. If a rat runs in the same maze every day for a year, will he then take longer to learn a new one? A consideration of the results so far obtained would, I think, make it appear improbable that he would take longer than an

[1] *Loc. cit.* I. 8.

unpractised rat, but it is very likely that he would take longer than a rat which had learnt several mazes in that time. Nevertheless, whatever the result of this further experiment, it would not prove that the rat is any more of a machine than a man is. A year to a rat's organism is probably as twenty years to a human being. A man who had gone to the station by a certain route every day for twenty years would find it extremely difficult to become habituated to a new route.

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THE PROOF OR DISPROOF OF THE EXISTENCE OF GENERAL ABILITY.

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1. Introduction.

The object of this paper is to investigate the significance of the coefficient of partial correlation, and to examine into the validity of some reasoning based on its use, especially in experimental psychology and pedagogy, and particularly with regard to the problem of the existence or non-existence of General Ability. In these sciences a very common type of argument consists in drawing conclusions as to the mechanism producing correlations, from a comparison of entire and partial correlation coefficients. If, for example, $r_{12\cdot3}$ (the partial correlation between two mental tests 1 and 2 with regard to a third test 3) is less than the entire coefficient r_{12}, then conclusions are often drawn, from the difference of the two, as to the amount of ability general to the three tests 1, 2, and 3.

Let us take a particular instance. Suppose that a number of subjects have been tested in (1) *Touch Discrimination* (Spatial Threshold), by means of an aesthesiometer, and in (2) *Card Sorting*, in which playing cards have to be dealt into piles according to the colour of their backs: and suppose that the correlation of Touch Discrimination with Card Sorting, $r_{12} = 0{\cdot}28$.

Next let the subjects be tested in (3) the *Dotting Test*, by means of Mr McDougall's apparatus, and let the further correlations be

Touch Discrimination with Dotting, $r_{13} = 0{\cdot}4$,
Sorting with Dotting, $r_{23} = 0{\cdot}7$.

The partial correlation of Touch with Sorting, for constant ability in Dotting, can then be found by the well-known formula

$$r_{12\cdot3} = \frac{r_{12} - r_{13}r_{23}}{\sqrt{(1 - r_{13}^2)(1 - r_{23}^2)}}$$
$$= \frac{0{\cdot}28 - 0{\cdot}7 \times 0{\cdot}4}{\sqrt{(1 - 0{\cdot}49)(1 - 0{\cdot}16)}},$$

and turns out to be zero. The correct conclusion from this is that if we could have an immense population of which the subjects tested are a fair sample, and if we could select from that population subjects with identical ability in Dotting, then this selected group would show no correlation between Touch Discrimination and Sorting, provided the correlations are all normal or pseudo-normal. And, on this assumption, it will be so, irrespective of whether the selected subjects are all good, or all bad, or all indifferent at Dotting, provided that they are all equal[1].

There is, however, a great and usually irresistible temptation to go beyond this and to make more or less explicit deductions as to the underlying mechanism producing the correlations. The argument is somewhat as follows. Since the partial correlation coefficient gives us the correlation for constant ability in Dotting, the influence of the factors at play in the Dotting Test has been eliminated; whereupon the correlation between Touch Discrimination and Sorting has fallen to zero. Therefore the correlation 0·28 must have been due to these factors, which are thus general to all three tests.

Another form of the argument is to say that since the correlation for Dotting constant is zero, the original correlation between Touch Discrimination and Sorting is due to the correlation of each with Dotting. A particularly naïve form operates with circles like Euler's diagrams in a text-book of logic. There are all stages of the fallacy, and sometimes the conclusions drawn are capable of a correct, though liable to an incorrect, interpretation.

The method of this paper is to apply the reasoning in question to phenomena in which the mechanism producing the correlation is known

[1] If the correlations be skew, then these statements have only a kind of average meaning, which has been very clearly explained by Mr Udny Yule in the *Proc. Roy. Soc.* for 1907. This is not the difficulty we are concerned with in this present paper.

a priori, namely to dice throws. If it is seen to be fallacious in this case, it must *a fortiori* be fallacious when applied to less simple data. The reverse statement is of course not necessarily true; reasoning valid for dice throws is not necessarily valid for mental phenomena. Moreover, it must be borne in mind that a conclusion invalid from the given premisses is not necessarily untrue. It is the *validity of certain reasonings* concerning the existence of General Ability which is here investigated, not the actual existence or non-existence of General Ability.

The conclusions of this paper are mainly negative, viz. that the interpretations of mental phenomena based on the use of the partial correlation coefficient are for the most part unwarranted. A result of a positive character is also attained, inasmuch as it is shown that, under certain circumstances and on certain assumptions, the existence of a factor common to three correlated variables can be deduced with certainty from the correlation coefficients, provided the latter are correct. Further inquiry into this point is reserved for another paper.

2. THREE CORRELATED VARIABLES REPRESENTED BY DICE THROWS.

Let n red dice, n blue, n yellow, and n white dice be thrown, and let the variable x be given by the combined red and white, y by the combined yellow and white, and z by the combined blue and white scores, as in Fig. 1. That is to say, there is a general factor (the white dice), common to all three variables, which causes all the correlations between them. These correlations are

$$r_{xy} = r_{yz} = r_{zx} = \tfrac{1}{2}.$$

The partial correlations are, by the well-known formula,

$$r_{xy \cdot z} = r_{yz \cdot x} = r_{zx \cdot y} = (\tfrac{1}{2} - \tfrac{1}{2} \cdot \tfrac{1}{2})/\sqrt{(1 - (\tfrac{1}{2})^2)(1 - (\tfrac{1}{2})^2)} = \tfrac{1}{3}.$$

It is not permissible, however, to reverse this statement, and to assume that in every case where $r_{xy} = r_{yz} = r_{zx} = \frac{1}{2}$ the correlations are formed solely by the action of a general factor. In fact, identically the same values can be produced without any general factor at all. Let n purple, n green, and n orange coloured dice be thrown, and let the variable x consist of the purple and orange, y of the orange and green, and z of the green and purple scores combined, as in Fig. 2.

Here there is no general factor whatever. The connexion of x with y (through the orange dice) is entirely independent of the connexion of x with z (through the purple dice).

Yet the correlations, both partial and entire, are exactly the same as in the first arrangement, viz.

$$r_{xy} = r_{yz} = r_{zx} = \tfrac{1}{2},$$
$$r_{xy \cdot z} = r_{yz \cdot x} = r_{zx \cdot y} = \tfrac{1}{3}.$$

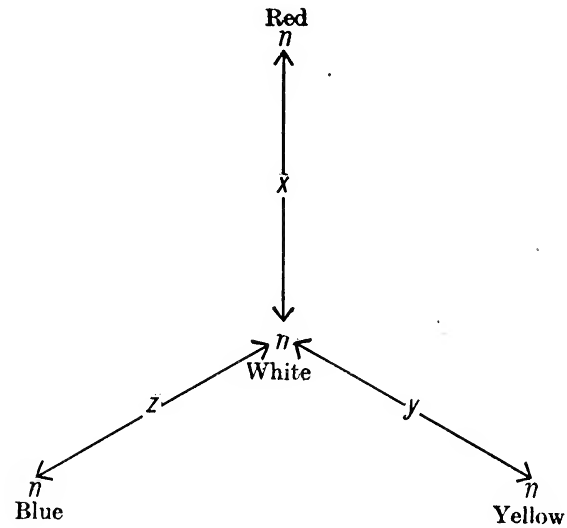

Fig. 1.

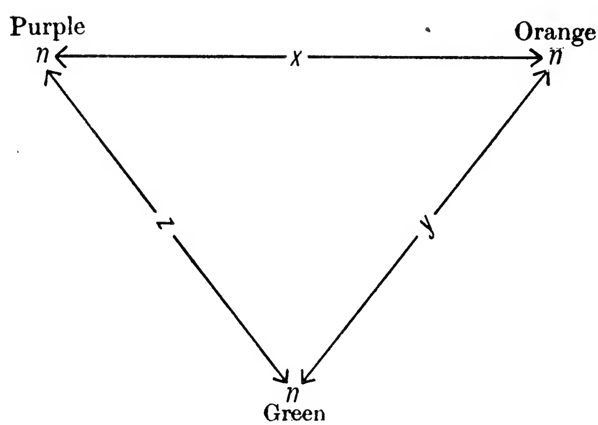

Fig. 2.

Clearly, therefore, if we only know of three variables x, y, and z, formed of dice throws, that their correlations are as above, we cannot say with certainty whether a general factor exists or no. Let us now consider a more general arrangement of dice, with numbers of the different colours, viz. W white, R red, B blue, Y yellow, P purple, G green, and O orange, thus:

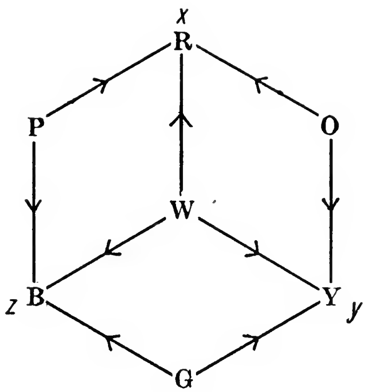

Fig. 3.

x consisting of the scores of the W, R, P, O; y of the W, Y, O, G, and z of the W, B, G, P dice.

In this arrangement we shall call W a *general factor*, it being common to all three variables; R, Y and B *specific factors*, they being unique to x, y and z respectively; and O, G and P *group factors*, since each runs through a group of (two) variables.

The theoretical values of the correlation between any two of the variables, say x and y, can be found by means of the formula[1]:

$$r_{xy} = \frac{\text{Number of dice common to } x \text{ and } y}{\text{Geometrical mean of total dice in } x \text{ and in } y}.$$

For example, if x is the score of $9n$ dice, and y the score of $4n$ dice, $2n$ being common, the correlation is

$$r = \frac{2n}{\sqrt{9n \times 4n}} = \frac{2}{6} = 0{\cdot}33.$$

[1] This formula was proved by me in This *Journal*, 1916, VIII. 275, in ignorance of any former statement of it. Professor Spearman showed (*ibid.* 282) that it is deducible from a formula of his concerning correlation of sums or differences. I have since noticed that Professor Spearman gives the following clear expression of the formula (*Amer. J. of Psychol.* 1904, XV. 75): "The correlation is always the geometrical mean between the two shares." I do not however agree with the application he there proceeds to make. The formula can also be directly deduced from Bravais, and in several other ways. It assumes that the elements all have the same standard deviation, as dice have.

The general arrangement of dice shown in the above figure includes the two special cases already considered, viz.:

$$(1)\quad P = G = O = \text{zero},\quad R = B = Y = W = n;$$

and

$$(2)\quad P = G = O = n,\qquad R = B = Y = W = \text{zero};$$

which both give $r_{xy} = r_{yz} = r_{zx} = \frac{1}{2}$*.

Of the infinite arrangements possible with this diagram, an infinite number (of a lower order) can in general be constructed to produce any given set of positive correlations between x, y and z. Moreover, all these possible ways of producing the required correlations are, in our ignorance, equally likely to have been those used by the person making the arrangement of dice, although they are, it is true, not equally probable as chance occurrences. From the correlations, therefore, we cannot in general deduce what proportion the white dice (*i.e.* the particular colour representing the general factor) bear to the others, for this proportion can vary between wide limits, and give exactly the same correlations. The most we could conceivably do would be to give the 'expectation' of the proportion of white dice. The meaning of this would be, that if a very large number of arrangements of dice were examined, each giving the required set of correlation coefficients, and *if we assume that these arrangements of dice are not formed on any plan*, beyond that they all agree in the correlations they produce, then the average proportion of white dice would be that named as the 'expectation' thereof. But if there is any reason to think that the large number of cases examined are all of much the same pattern—as there would be were they all natural phenomena of the same sort—then the 'expectation' of the proportion of white dice becomes useless and meaningless. We cannot conclude anything which is of any definite value in constructing the pattern, except give limits within which it must lie.

This brings us to the problem:—are there any values of r_{xy}, r_{yz} and r_{zx} which make it certain (having regard to their probable errors) that at any rate *some* general factor, some number of white dice, exists? The answer is that this is so if the correlations are large enough. For example, if we take the special case of equality of the three coefficients,

$$r_{xy} = r_{yz} = r_{zx} = r,$$

* If he be so minded, the reader can make thousands of other dice patterns, all giving the correlations $r_{xy} = r_{yz} = r_{zx} = \frac{1}{2}$. One group is given by

$$R = B = Y = W = kn,\quad P = G = O = sn.$$

But such a high degree of symmetry is unnecessary. For example, another pattern giving the same correlations is

$$R = 91n,\ Y = 91n,\ B = 54n,\ P = 78n,\ G = 78n,\ O = 91n,\ W = 78n.$$

The number of white dice present ranges between the two extreme cases given in the text then up to $r = \frac{1}{2}$ the correlations can be imitated by various numbers of red, blue, yellow, purple, green and orange dice without any white dice. But as soon as the common value r rises above $\frac{1}{2}$, some white dice are necessary. In this case therefore the proof of the existence of (at any rate) some amount of general factor reduces to the examination of the probable error of r, to see if r is indisputably greater than $\frac{1}{2}$.

In the more general case the matter is not so simple, the three values of r differing from one another. The more detailed examination of this case is reserved for treatment elsewhere. It will be found however that if the quantity

$$r_{xy}^2 + r_{yz}^2 + r_{zx}^2 + 2r_{xy}r_{yz}r_{zx}$$

is indisputably greater than unity, then some white dice, some general factor, may be postulated with certainty[1]. It may not be out of place to remind ourselves again that this, though true of the arrangements of dice we are considering, may not be true in the same sense of other phenomena, e.g. biological or mental phenomena.

The following two examples illustrate the above principles.

Example A.

Three variables, composed of overlapping dice throws, give correlations as follow:

$$r_{xy} = 0{\cdot}32, \quad r_{yz} = 0{\cdot}33, \quad r_{zx} = 0{\cdot}54.$$

Are any dice common to the three variables?

In this case we find:

$$r_{xy}^2 + r_{yz}^2 + r_{zx}^2 + 2r_{xy}r_{yz}r_{zx} = 0{\cdot}56, \text{ i.e. } < 1.$$

From this we conclude that these correlations can be imitated either with or without a general factor of white dice. The following arrangements of dice do actually produce these correlations.

Case (1).	$R = 19n,\ B = 17n,\ Y = 85n,$	(specific factors),
	$P = G = O =$ zero,	(no group factors),
	$W = 21n,$	(a general factor).
Case (2).	$R = 15n,\ B = 15n,\ Y = 16n,$	(specific factors),
	$P = 47n,\ G = 25n,\ O = 24n,$	(group factors),
	$W =$ zero,	(no general factor).

[1] A rough guide is the *average* value of the three r's: if this is greater than $\frac{1}{2}$ some general factor certainly exists. The exact condition however is that given in the text. It is proved in the Appendix to this paper. Note that all this only applies to correlations produced by overlapping dice throws, or by some sufficiently similar mechanism.

Example B.

Three variables, composed of overlapping dice throws, give correlations as follow:

$$r_{xy} = 0{\cdot}72, \quad r_{yz} = 0{\cdot}77, \quad r_{zx} = 0{\cdot}67.$$

Are any dice common to the three variables?

In this case we have

$$r_{xy}^2 + r_{yz}^2 + r_{zx}^2 + 2r_{xy}r_{yz}r_{zx} = 1{\cdot}93, \text{ i.e. } > 1.$$

We conclude therefore that some white dice common to the three variables are present, *i.e.* that there is a general factor. The following arrangements of dice do actually produce these correlations, the general factor being a minimum in one and a maximum in the other.

Case (1).	$R = 156n,\ B = 104n,\ Y = 52n,$	(specific factors),
	$P = G = O =$ zero,	(no group factors),
	$W = 260n,$	(general factor).
Case (2).	$R = B = Y =$ zero,	(no specific factors),
	$P = 90n,\ G = 161n,\ O = 123n,$	(group factors),
	$W = 198n,$	(general factor).

We see then that if

$$r_{xy}^2 + r_{yz}^2 + r_{zx}^2 + 2r_{xy}r_{yz}r_{zx} > 1,$$

the presence of some white dice is certain. If the above quantity, which we shall call D, is equal to or less than unity, the presence of white dice is uncertain. Suppose we consider two cases in which

$$r_{xy} = 0{\cdot}8, \quad r_{yz} = 0{\cdot}4, \quad r_{zx} = 0{\cdot}1, \quad D = 0{\cdot}842,$$

and $$r_{xy} = 0{\cdot}2, \quad r_{yz} = 0{\cdot}2, \quad r_{zx} = 0{\cdot}1, \quad D = 0{\cdot}094 \text{ respectively.}$$

Can we in these two cases say anything as to the *probability* of the existence of some general factor?

The answer to this question is twofold. If we suppose that the person making the arrangements of dice has, among all the possible arrangements giving

$$r_{xy} = 0{\cdot}8, \quad r_{yz} = 0{\cdot}4, \quad r_{zx} = 0{\cdot}1, \quad D = 0{\cdot}842,$$

chosen one by chance selection, and if we suppose that the other arrangement giving

$$r_{xy} = 0{\cdot}2, \quad r_{yz} = 0{\cdot}2, \quad r_{zx} = 0{\cdot}1, \quad D = 0{\cdot}094$$

has similarly been chosen by chance selection, then it is much more probable that a general factor exists in the first than in the second case.

This probability will in fact rise and fall with D though it is not *measured* by D. Its value will be investigated in another paper.

But if the person making the arrangements of dice has any definite rules which he follows in making the patterns, then the above probability will have much less meaning. In the former instance it would express the average to be found in many cases; in the latter it will no longer express even such an average value, and will have no practical worth.

3. AN EXAMINATION INTO SOME TYPICAL CONCLUSIONS BASED ON A COMPARISON OF ENTIRE AND PARTIAL CORRELATION COEFFICIENTS IN PSYCHOLOGY AND PEDAGOGY.

An experimental psychologist finds for sixty-six boys the following correlations between three mental tests, called respectively the ER, the ANOS and the Motor Tests[1]:

		ER	ANOS	Motor
x	ER	.	·78	·53
y	ANOS	·78	.	·21
z	Motor	·53	·21	.

From these he finds the partial correlation between ANOS and Motor (for constant ability in the ER test) to be negative, viz. $-0{\cdot}38$, whence he concludes that "the original positive correlation between ANOS and Motor is due entirely to the correlation of each with ER."

Now if these correlations had been those of dice throws, the dice throws owing their correlation to overlapping as in Fig. 3, then this conclusion would be unwarranted. Let us write

x for the ER test,
y for the ANOS test,
and z for the Motor test;

then the conclusion drawn is that there are *no green dice*.

It is true that the above set of correlation coefficients *can* be imitated without any green dice. The following arrangement does so:

$$R = Y = \text{zero};\ B = 391n, \qquad \text{(specific factors)},$$
$$P = 242n,\ G = \text{zero},\ O = 254n, \qquad \text{(group factors)},$$
$$W = 113n, \qquad \text{(general factor)}.$$

[1] W. Brown, *The Essentials of Mental Measurement*, Cambridge, 1911, 121–4. The tests in question consist in cancelling letters in pages of print, viz. the letters E and R, the letters ANO and S, and, in the Motor test, all the letters.

I may add that I would have used the example which Dr Brown gives as an illustration when first introducing the subject of partial correlation, did it not contain a misprint or arithmetical slip.

But the same correlations can also be imitated by a pattern in which the green dice play the major part in producing the correlation between y (ANOS) and z (Motor). Such a pattern is the following:

$$\begin{array}{ll} R = Y = B = \text{zero}, & \text{(specific factors)}, \\ P = 286n,\ G = 81n,\ O = 600n, & \text{(group factors)}, \\ W = 33n, & \text{(general factor)}. \end{array}$$

Moreover, the same correlations can be very nearly, though not quite exactly, imitated by a dice pattern in which all the correlation between y (ANOS) and z (Motor) is due to green dice, and is not due at all to the correlation of each with x (ER). Such an arrangement is the following:

$$\begin{array}{ll} R = Y = B = \text{zero}, & \text{(specific factors)}, \\ P = 300n,\ G = 105n,\ O = 595n, & \text{(group factors)}, \\ W = \text{zero}, & \text{(general factor)}. \end{array}$$

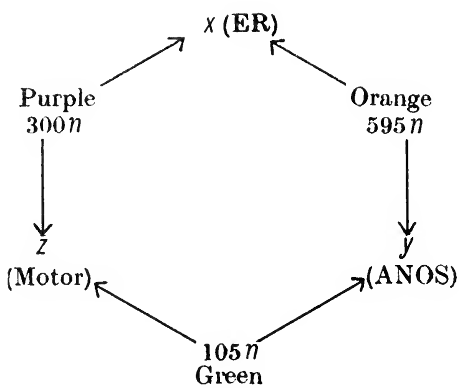

Fig. 4.

This pattern gives the correlations

	ER	ANOS	Motor
ER	.	·75	·50
ANOS	·75	.	·20
Motor	·50	·20	.

of which those actually found in the psychological experiment might well be samples. Here it is not the " original positive correlation between ANOS and Motor," but on the contrary the *negative partial correlation* which is " due to the correlation of each with ER."

There is, in fact, a whole family of dice patterns which will imitate these correlations exactly, and only of a small section of this family is it true to say that the correlation between y and z is due entirely to the correlations of each with x. This is because the quantity D is only slightly greater than unity, being equal to 1·107[1].

Another writer[2], in a pedagogical article, finds the correlations between three tests called respectively 'Nonsense Syllables,' 'Concrete Terms,' and 'Narrative,' to be as follow:

	x	y	z
x = Nonsense Syllables	.	·67	·43
y = Concrete Terms	·67	.	·55
z = Narrative	·43	·55	.

The partial correlation coefficients of these three tests, taken in pairs, the third test in each case being 'constant,' are:

$$r_{xy\cdot z} = \cdot 57,$$
$$r_{yz\cdot x} = \cdot 39,$$
$$r_{zx\cdot y} = \cdot 10.$$

From a comparison of these with the 'entire' coefficients, the writer concludes: (1) "Thus we see that the correlation between Narrative and Concrete Terms (·55) is due in part to the correlation of each with Nonsense Syllables, since when the influence of memory for Nonsense Syllables is made constant, the correlation falls from ·55 to ·39." (2) "Nonsense Syllables and Narrative are chiefly connected through their respective connexions with Concrete Terms, as that part of memory for Nonsense Syllables which is not common to memory for Concrete Terms is only connected to the extent of ·10 with that part of memory for Narrative which is not common to memory for Concrete Terms."

The quantity

$$D = r_{xy}^2 + r_{yz}^2 + r_{zx}^2 + 2r_{xy}r_{yz}r_{zx}$$

is in this case of the value 1·095 and is not significantly greater than

[1] In another and earlier account of these experiments (This *Journal*, 1910, III. 317) Dr Brown says of these correlations and partial correlations: "The relation here brought out is one very different from that of a central factor." This sentence is omitted in his book, and may perhaps therefore be considered as withdrawn. Apart from the question of its being in conflict with the immediately previous sentence, it is confuted by the fact that two of the dice patterns given in the text do contain a general factor. In fact, D being greater than unity, a central factor is necessary to imitate the correlations exactly with dice patterns.

[2] S. Wyatt, *J. of Exp. Ped.* 1914, II. 292–7.

unity[1]. The first part of the writer's conclusion would only have been certain had D been significantly greater than unity. The arrangement

$$\begin{array}{ll} R = Y = B = \text{zero}, & \text{(specific factors)}, \\ P = 40n,\ G = 55n,\ O = 70n, & \text{(group factors)}, \\ W = \text{zero}, & \text{(general factor)}, \end{array}$$

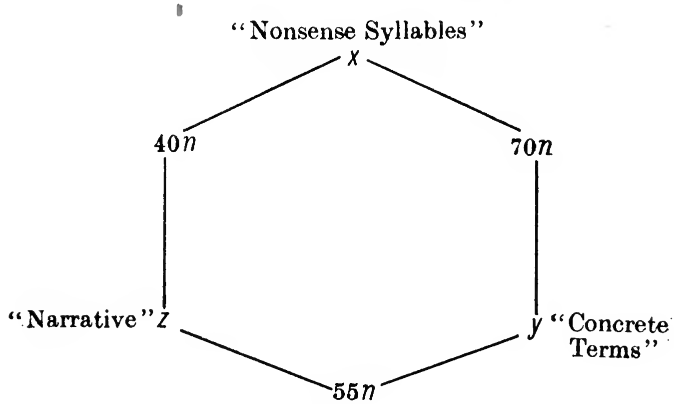

Fig. 5.

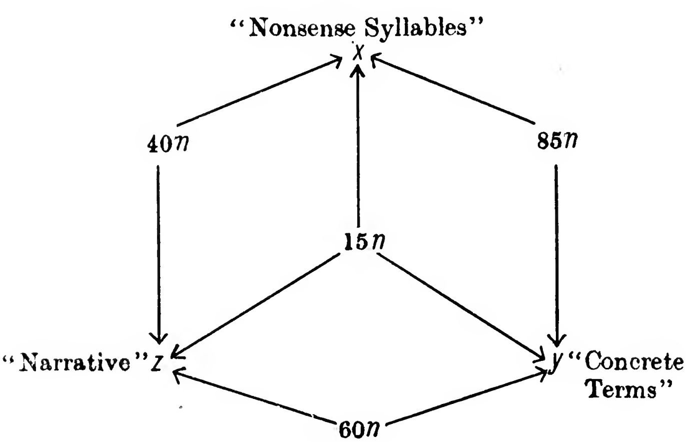

Fig. 6.

[1] It is in fact very far removed from being significantly greater than unity. Anyone calculating the probable error of D should remember that correlation coefficients are themselves correlated. The size of the probable error of D is not really an essential part of the argument of this paper and I am therefore not going into it: but it is in fact surprisingly large.

gives correlations of which those found might well be samples, and here the correlation between Narrative and Concrete Terms is not even in part due to the correlation of each with Nonsense Syllables.

An *exact* reproduction of the experimental r's is given by (*inter alia*)

$$\begin{array}{ll} R = B = Y = \text{zero}, & \text{(specific factors)}, \\ P = 40n,\ G = 60n,\ O = 85n, & \text{(group factors)}, \\ W = 15n, & \text{(general factor)}. \end{array}$$

Here the writer's first statement is true. But his second statement is not so, for Nonsense Syllables and Narrative are chiefly connected *independently* of their respective connexions with Concrete Terms. In the approximate pattern first given their connexion was wholly independent of Concrete Terms.

It will I think be clear from these examples that statements as to the underlying mechanism producing correlation, which are based on partial correlation coefficients[1], are very liable to be misleading unless given in the form of a probability. They are in fact usually invalid[2].

4. THE LOGICAL NATURE OF THE FALLACY.

The fallacy involved in the above typical examples consists in giving a possible explanation of the premisses as though it were the only possible explanation. In some cases this fallacy may be committed by the writers of the statements: in all it is nearly certain to be committed by many readers.

For instance, the conclusion cited on page 329 is that "the original positive correlation between ANOS and Motor is due entirely to the correlation of each with ER." The correct conclusion however is: "The original positive correlation between ANOS and Motor *may* be due entirely to the correlation of each with ER. But it may on the other hand not be due at all to the correlation of each with ER, but be direct.

[1] I hope it will not be thought that this article is in any way contrary to the statements as to the significance of the partial correlation coefficient made by Mr Udny Yule in the lucid memoirs in which he first introduced and afterwards generalized this coefficient (*Proc. Roy. Soc.* 1896 and 1907 respectively). Mr Yule makes no deductions (and rightly) as to the mechanism producing the correlations.

[2] And above all this there is further doubt introduced by the fact that correlations, even in objective physical phenomena, are often produced otherwise than by overlap. For example, the reader might consider the nature of the positive correlation which exists between the number of hearts in my hand and the number of spades in my partner's hand at whist. And in mental phenomena the correlations are no doubt still less tangible.

Each of these possibilities however is very improbable, if we have only the correlation coefficients to base our judgments on. It is far more likely that the original positive correlation is partly direct and partly due to the correlation of each with ER."

Fallacies are usually only possible in complex material, and are easily exposed if the argument is applied to simpler material. For this purpose I wish to draw an analogy between the type of argument exemplified in the cases quoted and one of Mr W. H. Winch's tests for reasoning, which runs as follows: "Less than half the girls in a school wore blue frocks, and less than half wore straw hats. Do you think there were any girls in the school who wore blue frocks as well as straw hats, or do you not, or can't you tell? You must say why you think so."

The strictly correct answer to Mr Winch's question is "You can't tell, because the blue-frock group and the straw-hat group *need* not overlap, being each less than half the school. On the other hand, they may overlap, indeed one may entirely include the other."

If p is the proportion of blue frocks, and p' of straw hats, then as long as $p + p'$ is not greater than unity, the answer is "You can't tell." As soon however as

$$p + p' > 1,$$

there are *certainly* some girls wearing both straw hats and blue frocks.

In both cases we may go further and say that the most probable proportion of girls wearing both is $p \times p'$, a prediction not of much value in practice, for there may be regulations in the school about wearing these articles. The combination might occur in the uniform of the seniors, or on the other hand the juniors might wear straw hats and the seniors' uniform might include blue frocks. "*You can't tell.*"

The analogous correlational question is: "The correlations between three tests taken in pairs are known to be r_{12}, r_{23}, and r_{31}. Do you think there is any ability general to the three tests, or do you not, or can't you tell? You must say why you think so."

The correct answer is "You can't tell, except when

$$r_{12}^2 + r_{23}^2 + r_{31}^2 + 2r_{12}r_{23}r_{31} > 1,$$

in which case, if the mental elements are additive like dice, there must be some general ability." The most probable amount of general ability present on certain further and equally unlikely assumptions can conceivably be given, but this quantity is like the most probable number of girls in both blue frocks and straw hats: it is of very little use, and for the same reason. Moreover, it is not given by correlational workers, many of whom commit the fallacy analogous to saying "all the girls who wore blue frocks wore straw hats," on learning that there were more straw hats than blue frocks.

5. CONCLUSIONS.

1. The comparison of a partial correlation coefficient $r_{12 \cdot 3}$ with an entire coefficient r_{12} is no sure guide to the extent to which the connexion of 1 and 2 is via 3.

2. If the correlations are produced by a mechanism such as overlapping dice throws, and if

$$r_{12}^2 + r_{23}^2 + r_{31}^2 + 2r_{12}r_{23}r_{31} > 1,$$

then at any rate *some* of the connexion of 1 and 2 *must* be via 3.

3. In this paper only the simplest case, viz. that of three variables, has been considered. There have been made however sweeping deductions as to the presence of General Ability in many forms of activity, based upon methods depending largely, if not entirely, on a similar misinterpretation of the methods of partial correlation.

6. APPENDIX.

(BY THE WRITER AND MR J. R. THOMPSON.)

Consider the Fig. 3 on page 325. If the group factors P, G and O are each zero, and also the general factor W, the correlations are all zero.

If P, G and O are now introduced, positive correlations appear, and these correlations can be increased by reducing R, B and Y. When these specific factors have become zero, the correlations have reached a limiting condition for group factors, beyond which they cannot rise without a general factor.

In this limiting case we have:

$$r_{xy} = \frac{O}{\sqrt{(P+O)(O+G)}} \quad \text{......................(1)},$$

$$r_{yz} = \frac{G}{\sqrt{(O+G)(G+P)}} \quad \text{......................(2)},$$

$$r_{zx} = \frac{P}{\sqrt{(G+P)(P+O)}} \quad \text{......................(3)}.$$

After a little manipulation these equations become:

$$-GP\frac{r_{xy}}{r_{yz}r_{zx}}+PO+OG=\text{zero},$$

$$GP-PO\frac{r_{yz}}{r_{zx}r_{xy}}+OG=\text{zero},$$

$$GP+PO-OG\frac{r_{zx}}{r_{xy}r_{yz}}=\text{zero}.$$

Since the solution $GP = PO = OG =$ zero is excluded, these equations can only be true under the condition

$$\begin{vmatrix} -\frac{r_{xy}}{r_{yz}r_{zx}} & 1 & 1 \\ 1 & -\frac{r_{yz}}{r_{zx}r_{xy}} & 1 \\ 1 & 1 & -\frac{r_{zx}}{r_{xy}r_{yz}} \end{vmatrix} = 0,$$

which reduces to

$$r_{xy}^2 + r_{yz}^2 + r_{zx}^2 + 2r_{xy}r_{yz}r_{zx} = 1 \text{ ....(4).}$$

This equation expresses a condition which is realizable when P, G and O have positive values and R, B, Y and W are zero. These are of course not the only cases where it is true; values of R, B, Y and W may be introduced in such a way as to leave the correlations unaltered at the values which satisfy the above equation. The point of interest is that it is true for all cases where group factors alone are present, and in such a case we have driven the correlations to the maximum values attainable without the aid of W the general factor. For brevity let us write D for the left side of the equation. If we wish D to be greater than unity the only course left is to introduce the general factor W, and on the other hand if we wish to make D less than unity the only course open to us is to introduce one or more of the specific factors R, B and Y. We do not say that there can be no W when D is less than unity, nor do we say that there can be no specific factors when D is greater than unity; such statements would be quite untrue. The correct inferences are that when D is greater than unity specifics *may* be present but a general factor *must* be present, and when D is less than unity a general factor *may* be present but specifics *must* be present.

In all this we are speaking of dice patterns like Fig. 3 only, or of correlation-producing mechanisms resembling this in the essential features.

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THE HIERARCHY OF ABILITIES.

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1. *Introduction.*
2. *Reply to an argument of Professor Spearman.*
3. *Some variants of my first hierarchy without a general factor.*
4. *A new theory of ability.*
5. *Conclusion.*

1. Introduction.

The object of this article is to investigate some of the ways in which hierarchical order can be produced among mental tests, other than by the action of a hypothetical general ability.

By hierarchical order is meant a property of the correlation coefficients of the tests in question, taken in pairs. If these are such that the equation

$$r_{su}/r_{sv} = r_{tu}/r_{tv}$$

is true for any four of the tests s, t, u, v, the r's being the correlation coefficients, then the hierarchy is said to be perfect. In practice of course only approximate satisfaction of this condition could be expected from experimental results. This equation is a more exact way of stating that if the tests are arranged in order, with the test which has the greatest total correlation at the head, then the order is called a perfect hierarchy if it is unchanged when, instead of the total correlation, the correlation with any particular member of the hierarchy is taken as criterion.

The result of the investigation is to confirm the statement already made[1], that there are many theories in addition to that of Professor Spearman[2], which will explain such hierarchical order as is actually found, and that the mathematical analysis of the data to hand is as yet unable to distinguish between these theories. The essence of all these theories is stated as conclusion.

[1] "A Hierarchy without a General Factor," This *Journal*, 1916, VIII. 271.

[2] "General Ability, its Existence and Nature" by B. Hart and C. Spearman, This *Journal*, 1912, V. 51.

2. Reply to an Argument of Professor Spearman.

In This *Journal* for 1916[1] I described sets of dice throws imitating the marks obtained in ten mental tests. There was no general factor in the dice throws, and yet they produced an excellent example of hierarchical order which easily passed Professor Spearman's criterion for proving the presence of a general factor. The arrangement of dice in question is described in Fig. 1 of that paper and the accompanying text.

In a note at the end of my paper, Professor Spearman advances as an argument in defence of his views the point that I have only proved that one single arrangement of my Fig. 1 gives a hierarchy, out of the huge number of possible arrangements, viz. 2^{360}. The chance of occurrence of my arrangement by mere accident is therefore negligible, and the chance of its occurrence in each of twenty investigations is still smaller.

Now in the first place nothing was further from my mind than to suggest that the particular illustrative arrangement of group factors in that figure explains all hierarchical order of correlation coefficients. If however this line of argument has to be dealt with, I must point out that the chance of any given general factor occurring in my Fig. 1 is *exactly the same* as the chance quoted by Professor Spearman for the occurrence of my illustrative arrangement.

Moreover, there exist millions and millions of other arrangements of my Fig. 1 which will produce hierarchical order among the correlation coefficients. *A few* of these can be obtained by shuffling the group factors of each test, while keeping in each the same total number of group factors as in my original arrangement. $5{\cdot}7 \times 10^{81}$ arrangements can thus be obtained, not including those which have a general factor, and most of these will produce as good a hierarchy as any psychological experiment has yet produced, and capable of being claimed as perfect by Professor Spearman's criterion. At least one-half will do so, but to be quite safe let us say one per cent. will do so.

On the other hand, the total number of possible ways of introducing ever so small a general factor into my Fig. 1 is 7×10^{10}. The number of hierarchy-producing arrangements immediately deducible from my original 'special arrangement' therefore exceeds the total possible cases of general factor at least 10^{69} times.

[1] *Loc. cit.*

In fact, although I only said in my 1916 article that the existence of a general factor was not proved, the truth really is that, on the evidence available (and even granting for the moment that the experimental hierarchies are as good as Professor Spearman's calculations make them out to be) the existence of such a factor is extraordinarily improbable, if this line of argument is to be followed out.

3. Some Variants of my first Hierarchy without a General Factor.

As I have said in the last section, variants of the arrangement given in 1916 can immediately be obtained by shuffling the group factors. The 36 group factors employed were distributed as follows:

Test	No. of group factors	No. of specific factors	Total
a	24	0	24
b	20	0	20
c	19	1	20
d	17	3	20
*e**	11	9	20
f	8	14	22
g	8	16	24
h	6	20	26
k	6	22	28
l	6	24	30

* I find in correcting the proof that test *e* in the previous article had 12 group factors. This slip however makes no difference to the argument.

Fig. 1 of my 1916 article identifies the group factors of each test. Let us however allow this to be decided by chance, only fixing the numbers of group factors as above, and drawing lots for which they are. An arrangement actually arrived at in a trial was that shown in Fig. 1 of the present article.

FIG. 1. One of the numerous variants of the figure given on page 277 of This *Journal*, 1916, VIII.

Test	Group factors involved
a	1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 14, 16, 18, 20, 22, 24, 25, 27, 28, 29, 34, 36
b	1, 2, 3, 6, 7, 9, 10, 12, 14, 15, 18, 19, 22, 23, 27, 30, 33, 34, 35, 36
c	2, 3, 5, 6, 7, 8, 9, 18, 19, 20, 21, 26, 27, 28, 29, 30, 32, 33, 34
d	1, 8, 9, 10, 11, 13, 14, 16, 18, 20, 23 26, 27, 29, 30, 31, 35,
e	1, 6, 14, 18, 19, 20, 21, 25, 27, 29, 36
f	1, 3, 6, 13, 19, 22, 23, 28,
g	2, 3, 7, 13, 14, 29, 31, 34,
h	14, 18, 27, 29, 33, 36
k	6, 22, 24, 28, 29, 31,
l	3, 6, 7, 13, 34, 36

The correlation coefficients produced by Fig. 1 are as follows (the theoretical values):

	a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*k*	*l*
a		64	55	50	40	22	25	20	19	22
b	64		55	45	34	29	23	22	08	20
c	55	55		40	34	19	23	18	13	16
d	50	45	40		29	14	18	18	08	04
e	40	34	34	29		09	09	21	08	08
f	22	29	19	14	09		09	00	12	12
g	25	23	23	18	09	09		08	08	15
h	20	22	18	18	21	00	08		04	04
k	19	08	13	08	08	12	08	04		03
l	22	20	16	04	08	12	15	04	03	

It will be seen at a glance that again the order is strongly hierarchical.

The total number of arrangements which could be found in this way is as has been already said $5{\cdot}7 \times 10^{81}$ although not all of these would give good hierarchies. By trying a few however the most non-mathematical reader can convince himself that the chance of not getting a hierarchy is very small. The worst conceivable case is that in which the set of selected group factors in each test is as it were consecutive, but starting from opposite ends of the row of group factors in alternate tests. The correlations in this case are such that the intercolumnar correlation oscillates between plus and minus unity. In fact the arrangement breaks up into two hierarchies, each caused by a general factor. A number of other arrangements, however, without actually containing two factors each general to one of the sub-hierarchies, but depending merely on combinations of group factors, will also arise imitating the above double hierarchy as closely as may be desired, so that the presence of these minor 'general' factors is not proved any more than the presence of the generalissimo factor by the ordinary hierarchy.

Excluding such special cases, which will occasionally arise, and excluding also all cases of a really general factor, there still remains a large majority, millions and millions in number, of hierarchy-producing arrangements of my original figure of 1916[1] which contain not a ghost of a general factor.

[1] The paper "A Hierarchy without a General Factor," although not printed until September 1916, was prepared in 1914, and appears on the agenda of the meeting of the British Psychological Society for March 20th, 1915. An abstract of the article was sent to the Secretary of the Society early in that month, and a longer account direct to Professor Spearman. On learning that the latter would be prevented by military duties from being present at the meeting, I asked permission to postpone my paper, and the postponement was announced by a slip sent out with the agenda. Full copies of the paper were then sent both to the Secretary and to Professor Spearman.

4. A New Theory of Ability.

It will be seen from all this what is the key to the formation of imitation tests which while containing no general factor will yet give excellent hierarchies. This key is given in what follows.

If desired, it can be expressed in the form of a psychological theory which accounts for the known facts of correlation as well as does the Theory of General Ability, and which is in my opinion more consonant with other facts. It is a wider theory, including the possibility of General Ability as a special case.

Let us suppose that the mind, in carrying out any activity such as a mental test, has two levels at which it can operate. The elements of activity at the lower level are entirely specific; but those at the higher level are such that they may come into play in more than one kind of activity, in more than one mental test. These elements are assumed to be additive like dice, and each to act on the 'all or none' principle, not being in fact further divisible.

The difference between the levels may be physiological, as between cortex and spinal cord, or it may be the difference between conscious and non-conscious, or what not. The theory may later be reduced to a less harsh dichotomy and there may be gradations from the one level to the other. Indeed the specific factors may be entirely omitted in what follows. *Leaving the rest to chance*, this theory will give hierarchical arrangement of correlation coefficients in the overwhelming majority of cases, if we assume that the elements are additive, as dice are additive, and are about equally variable from subject to subject, as dice are equally variable. It includes the possibility of obtaining results which are not hierarchies at all, though only very rarely, and the hierarchies obtained will vary in perfection. This seems to me to agree exactly with the facts.

To illustrate the theory, consider the following dice example. Let the number of tests we are going to use be ten, and their names *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *k*, *l*. Let us decide *by chance* how many elements from the upper level and how many from the lower level each of these tests contains. This was done by drawing cards from a well-shuffled pack composed of four ordinary packs, shuffling between each draw. The possible number of group factors or upper level elements in each test was therefore 13 (the Knave, Queen and King being counted as 11, 12 and 13). The same number of specific factors or lower level elements was possible. The actual draw was as here given:

Test	No. of upper level elements	No. of lower level elements	Total
a	12	5	17
b	10	10	20
c	10	13	23
d	4	9	13
c	4	8	12
f	13	13	26
g	6	13	19
h	6	4	10
k	13	11	24
l	11	1	12

The next step is to identify the group factors or upper level elements in each test. This also was done by chance. The suit *spades* was taken, and each card represented an element or factor; and drawings were made to identify the elements in each test. The result was as follows:

Test	Group factors or upper level elements
a	Ace, 2, 4, 5, 6, 7, 8, 9, 10, Knave, Queen, King
b	Ace, 2, 4, 5, 6, 7, 10, Knave, Queen, King
c	2, 3, 4, 5, 6, 7, 8, 9, Knave, King
d	3, 4, 7, Knave,
e	2, 8, 9, 10,
f	Ace, 2, 3, 4, 5, 6, 7, 8, 9, 10, Knave, Queen, King
g	Ace, 3, 6, 10, Knave, Queen,
h	Ace, 4, 7, 9, 10, King
k	Ace, 2, 3, 4, 5, 6, 7, 8, 9, 10, Knave, Queen, King
l	Ace, 2, 3, 4, 5, 7, 8, 10, Knave, Queen, King

From this we can find the number of elements common to each pair of tests. They are given in the following table:

	a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*k*	*l*
a		9	10	3	4	12	5	6	12	11
b	9		7	3	2	10	5	5	10	9
c	10	7		4	3	10	3	4	10	8
d	3	3	4		0	4	2	2	4	4
e	4	2	3	0		4	1	2	4	3
f	12	10	10	4	4		6	6	13	11
g	5	5	3	2	1	6		2	6	5
h	6	5	4	2	2	6	2		6	5
k	12	10	10	4	4	13	6	6		11
l	11	9	8	4	3	11	5	5	11	

If now dice are thrown, one for each element, both upper and lower, and are combined in the way demanded by the above tables, then the correlations found, if the experiment is sufficiently extended, will be those given by the formula

$$r_{ab} = \frac{\text{Number of common elements}}{\text{Geometrical mean of total elements in } a \text{ and } b},$$

and *mutatis mutandis* for the other pairs[1]. The correlations prove to be as follows:

	a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*k*	*l*
a		·49	·51	·20	·28	·57	·28	·46	·59	·77
b	·49		·33	·19	·13	·44	·26	·35	·46	·58
c	·51	·33		·23	·18	·41	·14	·26	·43	·48
d	·20	·19	·23		·00	·22	·13	·17	·23	·32
e	·28	·13	·18	·00		·23	·07	·18	·23	·25
f	·57	·44	·41	·22	·23		·27	·37	·52	·62
g	·28	·26	·14	·13	·07	·27		·14	·28	·33
h	·46	·35	·26	·17	·18	·37	·14		·39	·46
k	·59	·46	·43	·23	·23	·52	·28	·39		·65
l	·77	·58	·48	·32	·25	·62	·33	·46	·65	
Total	4·15	3·23	2·97	1·69	1·55	3·65	1·90	2·78	3·78	4·46

This is a hierarchy as perfect as any ever found in experimental psychology, as will be seen on placing the tests in order of total correlation thus:

	l	*a*	*k*	*f*	*b*	*c*	*h*	*g*	*d*	*e*
l		·77	·65	·62	·58	·48	·46	·33	·32	·25
a	·77		·59	·57	·49	·51	·46	·28	·20	·28
k	·65	·59		·52	·46	·43	·39	·28	·23	·23
f	·62	·57	·52		·44	·41	·37	·29	·22	·23
b	·58	·49	·46	·44		·33	·35	·26	·19	·13
c	·48	·51	·43	·41	·33		·26	·14	·23	·18
h	·46	·46	·39	·37	·35	·26		·14	·17	·18
g	·33	·28	·28	·29	·26	·14	·14		·13	·07
d	·32	·20	·23	·22	·19	·23	·17	·13		·00
e	·25	·28	·23	·23	·13	·18	·18	·07	·00	

This hierarchy follows from the hypothesis and assumptions given, leaving all other points to chance. If any reader cares to repeat the experiment, I may say for his information that the above only took an afternoon to complete. *Not* to get a hierarchy is like the chance of not getting a polygon closely resembling a Gauss curve on repeatedly throwing a dozen pennies.

5. CONCLUSION.

The following theory explains hierarchical order among correlation coefficients at least equally as well as does Professor Spearman's theory of General Ability.

[1] This *Journal*, 1916, VIII. 275.

The mind, in carrying out any activity such as a mental test, has two levels at which it can operate. The elements of activity at the lower level are entirely specific, but those at the higher level are such that they may come into play in different activities. Any activity is a sample of these elements. The elements are assumed to be additive like dice, and each to act on the 'all or none' principle, not being in fact further divisible.

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GENERAL ABILITY, CLEVERNESS AND PURPOSE.

By J. C. MAXWELL GARNETT.

1. *Introduction.*
2. *A 'single general factor' in dissimilar mental tests.*
3. *The cosine expression for correlation; and the consequent condition that three correlated variables should be completely expressible in terms of only two independent factors.*
4. *'Cleverness,' a group factor in tests of certain intellectual qualities; and its relation to 'General Ability.'*
5. *Group factors in tests of similar qualities.*
6. *'Purpose,' a group factor in tests of certain character qualities.*
7. *Summary of Conclusions.*

1. *Introduction.*

THE object of this paper is to show that, in addition to the 'single general factor' which along with specific factors tends (as Professor Spearman has shown) wholly to account for the correlations between any set of sufficiently diverse mental tests, there are other independent factors which also enter to a significant extent into tests of certain groups of similar qualities. Thus there are several intellectual qualities which may be regarded as compounded of 'g'—the 'general ability' which tends to occur without significant group factors in every set of sufficiently dissimilar tests—and of one other independent (group) factor which we shall find it convenient to describe as 'cleverness.' There is also, as Dr Webb has shown[1], a number of character qualities into which there enters, along with 'g,' another (group) factor which has much in common with 'purpose' and which is independent both of general ability and of cleverness. When the degrees in which any particular person possesses general ability, cleverness and this third (group) factor are known, his character is, in large measure, determined; although of course a very great number of other factors, independent of these three, may also enter into many of his mental qualities.

[1] This *Journal*, Monogr. Suppl. I. 3.

2. *A 'single general factor' in dissimilar mental tests.*

It has been shown that if $q_1, q_2, \ldots q_n$ be a number of correlated variables, as for example the measures of the mental qualities tested in such investigations as those of Mr Burt[1] or Dr Webb[2], each of which is distributed according to the normal law with the same standard deviation, then all these n inter-dependent q's may be regarded as made up of the sum of fractions of n or more independent variables $x_1, x_2, \ldots x_N$ (where $N \geqslant n$) each of which is distributed according to the normal law with the same standard deviation[3]. In other words, any one of the q's, say q_s, may be expressed as a linear function of the x's thus

$$q_s = {}_sl_1 . x_1 + {}_sl_2 . x_2 + \ldots + {}_sl_N . x_N \quad \ldots\ldots\ldots\ldots\ldots(1),$$

where the l's are numerical coefficients such that

$${}_sl_1^2 + {}_sl_2^2 + \ldots + {}_sl_N^2 = 1 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots(2).$$

(In order to avoid wearisome repetition, we state here that all the variables discussed in this paper are assumed to be distributed according to the normal law with the same standard deviation unless, in any particular case, the context explicitly provides to the contrary.)

It has further been shown that, if the correlations between the q's satisfy Professor Spearman's 'correlation between columns' conditions the differences, $q_1', q_2', \ldots q_n'$, between the n q's and a constant multiple k (which becomes zero when Mr Burt's conditions for a hierarchy are fulfilled) of an $\overline{n+1}$th variable, y, that is independent of the q's may be expressed in terms of $n+1$ independent x's of which one, say $x_{n+1} \equiv x_g$, is a single general factor, while all the other x's are specific factors[4]. In other words, if r_{st} be the correlation between q_s and q_t, and if when the r's be arranged in a correlation table thus

$$\begin{array}{ccccc} - & r_{12} & r_{13} & \cdots & r_{1n} \\ r_{12} & - & r_{23} & \cdots & r_{2n} \\ r_{13} & r_{23} & - & \cdots & r_{3n} \\ \vdots & \vdots & \vdots & & \vdots \\ r_{1n} & r_{2n} & r_{3n} & \cdots & - \end{array}$$

the correlation between every pair of columns is ± 1, we may write

$$q_s' \equiv (q_s - y . k)(1 + k^2)^{-\frac{1}{2}} = r'_{sg} . x_g + (1 - r'^2_{sg})^{\frac{1}{2}} . x_s \quad (s = 1, 2, \ldots n) \quad \ldots\ldots(3),$$

[1] This *Journal*, 1909–10, III. 94–177. [2] *Loc. cit.*

[3] Garnett, *Proc. R. S.*, 1919. Cf. also Bravais, *Mém. de l'Inst. de France*, 1846, IX. 260 *et seq.*

[4] Garnett, *loc. cit.*

where r'_{sg} is the coefficient of correlation between q_s' and x_g; so that x_g is a single general factor of the q''s and all the other x's are specific factors[1]. When Mr Burt's conditions for a hierarchy, namely

$$\frac{r_{su}}{r_{sv}} = \frac{r_{tu}}{r_{tv}}$$

for all values of s, t, u, v from 1 to n inclusive, are satisfied, $k = 0$ and equations (3) reduce to

$$q_s = r_{sg} \, . \, x_g + (1 - r^2_{sg})^{\frac{1}{2}} \, . \, x_s \quad (s = 1, 2, \ldots n) \qquad \ldots\ldots\ldots(4),$$

where r_{sg} is the coefficient of correlation between q_s and x_g. Under certain conditions, which are not fulfilled in the case of published results of mental tests examined by the present writer, r_{sg} in equations (4) or r'_{sg} in equations (3) would become imaginary.

It is easy to show that Mr Burt's conditions are equivalent to $\frac{1}{2}n(n-3)$ independent conditions. We note that when $n = 3$ no conditions need be satisfied by the correlations between the q's in order that the q's may be expressed in terms of four independent factors, one of which is a single general factor and the remainder are specific factors.

In what follows we shall use the term 'single general factor' to denote only that one of $n + 1$ independent factors which, when n correlated q's are expressed in terms of them alone, enters into each of the q's while the remaining n independent factors are specific, each entering into a different q. This definition of a single general factor is equivalent to saying that, if n correlated q's can be expressed by means of equations (4) in terms of $n + 1$ x's, so that all the r's are finite, then x_g is a single general factor.

That such a definition of a single general factor is necessary in order to avoid confusion is readily seen by observing that it is always possible, by choosing a sufficient number of independent factors (x's) in terms of which to express n given q's, so to express these q's that any desired number of x's will have finite coefficients in the right-hand sides of all the n equations (1) obtained by giving s all values from 1 to n in that equation. For example in equation (4) we might replace x_g by

$$\frac{1}{\sqrt{R}}(m_1 y_1 + m_2 y_2 + \ldots + m_k y_k), \text{ where } m_1{}^2 + m_2{}^2 + \ldots + m_k{}^2 = 1,$$

where all the y's are independent of each other and of $x_1, x_2, \ldots x_n$. There would then be k independent factors (y's) instead of only one (x_g) entering into each of the q's. Moreover, by another similar trans-

[1] Cf. Garnett, *loc. cit.*

formation it is possible to replace the right-hand sides of equations (4) by expressions into which there enter any desired number of group factors: namely, independent factors whose coefficients are finite in the expressions for two or more but not all of the q's. If, for example, we were to write

$$y_1 = r_{1g} \cdot x_g + \sqrt{1 - r_{1g}^2} \cdot x_1,$$
$$y_2 = \sqrt{1 - r_{1g}^2} \cdot x_g - r_{1g} \cdot x_1,$$

so that

$$x_g = r_{1g} \cdot y_1 + \sqrt{1 - r_{1g}^2} \cdot y_2,$$

and to substitute for x_g in the n equations (4), they would be replaced by n equations into the right-hand sides of which there entered $n + 1$ mutually independent variables $y_1, y_2, x_2, x_3, \ldots x_n$, but so that y_2 appeared to be a group factor entering into all but one of the q's.

3. *The cosine expression for correlation; and the consequent condition that three correlated variables should be completely expressible in terms of only two independent factors.*

It has been shown[1] that the correlation r_{st} between any two of the q's, q_s and q_t, expressed by means of equations (1), is given by

$$r_{st} = {}_sl_1 \cdot {}_tl_1 + {}_sl_2 \cdot {}_tl_2 + \ldots + {}_sl_N \cdot {}_tl_N \quad \ldots\ldots\ldots\ldots\ldots(5).$$

However great may be the number, N, of independent variables we can, by a linear transformation, choose new independent variables y_1, y_2, y_3, ... such that two variables, q_s and q_t, depend on two and two only among them, say y_1 and y_2. Equations (1) may then be replaced by

$$\left.\begin{aligned} q_s &= y_1 \cos\theta_s + y_2 \sin\theta_s \\ q_t &= y_1 \cos\theta_t + y_2 \sin\theta_t \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(6),$$

so that

$$r_{st} = \cos(\theta_s \sim \theta_t) \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(7).$$

This equation may be interpreted by saying that, if we measure the y_1 and y_2 of any subject along two axes Oy_1 and Oy_2 at right angles to one another, and plot the point P (y_1, y_2) corresponding to that subject so that y_1 and y_2 are the projections of OP on Oy_1 and on Oy_2 respectively, and if we then draw a line Oq_s, which we may call the axis of q_s, making with Oy_1 an angle θ_s (measured in the direction from Oy_1 to Oy_2), the degree in which the subject possesses the quality measured by q_s will be equal to the projection of OP on Oq_s. Moreover, the

[1] Garnett, *Proc. R. S.*, 1919. Cf. also Bravais, *Mém. de l'Inst. de France*, 1846, IX. 271.

correlation between q_s and q_t is equal to the cosine of the angle q_sOq_t; so that this correlation represents the average deviation in q_s (or q_t) corresponding to unit deviation in q_t (or q_s), affording us a very simple geometrical conception of the measure of correlation.

The condition that three qualities q_1, q_2 and q_3 should be expressible in terms of two and only two independent factors follows at once. It is the condition that Oq_1, Oq_2 and Oq_3 should lie in a plane; or that the sum of the angles $q_2\hat{O}q_3$, $q_3\hat{O}q_1$ and $q_1\hat{O}q_2$ should be zero; or that

$$\cos^{-1} r_{23} + \cos^{-1} r_{31} + \cos^{-1} r_{12} = 0 \quad\ldots\ldots\ldots\ldots\ldots\ldots(8);$$

or that

$$r_{23}^2 + r_{31}^2 + r_{12}^2 - 2r_{23}r_{31}r_{12} = 1 \quad\ldots\ldots\ldots\ldots\ldots\ldots(9).$$

4. *'Cleverness,' a group factor in tests of certain intellectual qualities; and its relation to 'general ability.'*

Dr Webb, in his paper on "Character and Intelligence[1]," describes an investigation of forty-eight mental qualities. The subjects of his enquiry were ninety-eight men students (average age twenty-one) at a Training College during the last six months of their second year of training (January—July, 1912), and a similar group of ninety-six students during the corresponding period of the following year (January—July, 1913). Dr Webb also investigated a similar number of mental qualities of four groups of London schoolboys, with whom we shall be only once again concerned. Of the students' forty-eight mental qualities investigated, forty-three were estimated by pairs of prefects who acted as judges; to each pair a group of twenty (or nineteen) students was assigned. The measurements of the qualities were so chosen as to give the same constant (standard deviation) to the frequency distribution of each. The forty-three mental qualities estimated by the prefects included degree of sense of Humour (No. 8) and two described by Dr Webb as intellectual qualities, namely, Quickness of apprehension (No. 35) and Originality of ideas (No. 38).

Among the five qualities not estimated by the prefects, two were objectively measured, namely Examinational Ability and Professor Spearman's single general factor whose measure is g*. The manner in which 'g' was measured is fully described in Dr Webb's paper. Following

[1] *Loc. cit.*

* It will be remembered that g, like the measures of the other qualities with which we are concerned, is distributed according to the normal law and may have any value, $-\infty$ to $+\infty$, its mean value being zero. For the future we shall briefly write 'g' in place of 'the quality of which the measure is g.'

Dr Hart and Professor Spearman, Dr Webb described '*g*' as a 'General Factor of Intellective Energy[1]'; but Dr Webb produced no additional evidence for regarding g as a measure of intellective energy rather than as, for example, a measure of power of voluntarily concentrating nervous energy, or, what amounts to the same thing, voluntarily concentrating attention.

Now it has been shown[2] that the correlations between the three qualities, '*g*,' Humour and Originality satisfy equation (8) within the limits of probable error, while the correlations of Quickness with any pair of the other three qualities very nearly do so; so that '*g*,' as measured experimentally, together with Humour and Originality, as estimated in a very large number of cases by Dr Webb's collaborators, is compounded of two, and only two, independent factors, while Quickness consists almost, if not quite, of some combination of the same independent factors. We may therefore construct a diagram (Figure 1) by drawing lines Og, Oh, Oo in one plane, making the following angles with each other:

$$\begin{aligned} gOo &= \cos^{-1} r_{go} = 100°, \\ hOo &= \cos^{-1} r_{ho} = \ \ 38°, \\ oOg &= \cos^{-1} r_{og} = \ \ 62°. \end{aligned}$$

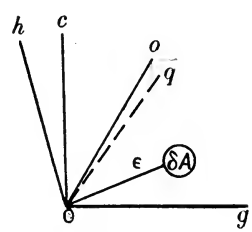

Fig. 1.

And we may add to the diagram another line Oq, very nearly coincident[3] with Oo.

If the training college students who formed the subjects of Dr Webb's investigation constitute a fair sample of adult Englishmen, the '*g*,' sense of Humour, Originality, and (probably) Quickness of apprehension of any Englishman can be represented by a single point (P, say) on the plane of the diagram—the 'intellectual plane,' as it has been

[1] *Loc. cit.* 37.

[2] Garnett, *loc. cit.*

[3] The correction coefficient between Humour and Originality given in Dr Webb's table is $r_{qo} = 1{\cdot}04$.

called; for if P be determined so that the projection of OP on any two of the axes, Og, Oq, Oo or Oh, measures the corresponding two qualities of the subject in question, it follows (as we saw in § 3 above) that the degrees in which the same subject possesses the remaining two qualities will be measured by the projections of OP on the remaining two axes; and the proportion of Englishmen whose intellectual qualities (if for the moment we may confine this term to the four qualities just named) are represented by points lying within any small area δA, will be equal to the volume of a cylinder having its axis perpendicular to δA, its base on δA, and bounded at its other extremity by the surface

$$z = \frac{1}{2\pi\sigma^2} e^{-\frac{1}{2\sigma^2}(x^2+y^2)} = \frac{1}{2\pi\sigma^2} e^{-\frac{\epsilon^2}{2\sigma^2}} \quad \dots\dots\dots\dots\dots(10),$$

where x and y are the measures of the two independent variables of which each of the four qualities is compounded, and where

$$\epsilon^2 = x^2 + y^2;$$

so that ϵ measures the degree in which the subjects represented by points in δA are *exceptional*. We observe that since equation (10) is independent of the particular axes chosen, the number of individuals represented by points in δA is independent of the orientation of the radius joining O to δA.

The solid bounded on one side by the surface whose equation is given in (10) above, and on the other by the plane $z = 0$, is a useful conception when we have to consider qualities dependent upon only two independent variables measured along Ox and Oy. For example, the number of men whose Originality lies between o and $o + \delta o$ is equal to the volume of this solid included between the two planes which are at right angles to Oo (and therefore to the plane gOh), and which cut Oo at distances o and $o + \delta o$ from the origin.

Now imagine the small area δA to move, in the plane of the diagram, at right angles to Og (or to Oh, oı to Oo). It is clear that the projections of the radius $(O\,.\,\delta A)$ on the lines Oh and Oo (or on Og and Oo, or on Oh and Og) vary in strict proportion to one another. In other words, if any one of the three qualities 'g,' Humour, or Originality be kept constant, the other two qualities will be strictly proportional to each other. That is to say, the partial correlation between Humour and Originality, with 'g' constant, should be ± 1; between Originality and 'g,' with Humour constant, should be ± 1; and between 'g' and Humour, with Originality constant, should be ± 1. Actually, the partial correlations in question, obtained by Yule's formula from the total correlations given above, are 1·00, 1·0*, $-$ 0·98, thus verifying all our anticipations within the probable error.

The nature of the two independent qualities or factors which, combined in different proportions according to equation (5), make up each of the four intellectual qualities 'g,' Humour, Originality and Quickness, has been thus discussed[1]:

Since the equation (10) is independent of the particular pair of perpendicular axes chosen in the 'intellectual' plane, we may choose Og as one axis. What, then, is the other axis, represented by a line Oc at right angles to Og? Evidently, since the Humour axis makes with it an angle of only some 10°, it is very nearly identical with Humour.

Now Mr McDougall has suggested that the process of 'reproduction by similars,' or, as William James[2] called it 'association by similarity,' is due to "a partial identity of the complex neural systems involved in the perception of two objects. Each system consists of many sub-systems, and one or more of these sub-systems is common to the two. When the one system is excited, its excitement spreads, not, as is most commonly the case, through some association-path previously established by some temporal contiguity, but from the sub-system, which forms also part of another system, radiates itself through that other system. In the commonplace type of mind this process comparatively rarely occurs. It would seem that in the brains of such persons neural systems tend to become circumscribed and individualized, whereas in a higher type of brain the neural systems are more complexly interwoven, sub-systems becoming freely associated with many principal systems. In a brain so constituted, reproduction of similars will frequently occur, causing the dull chain of simple redintegration, the serial reproduction of impressions associated by temporal contiguity, to be broken across. The possessor of a brain so constituted will never be a commonplace person; he may be a crank or an original thinker, or merely a wit[3]."

There is no evident reason why such a constitution of brain should have anything to do with capacity voluntarily to concentrate attention, the capacity which Binet and others have identified with the quality whose measure is g. Now the quality independent of 'g' for which we are seeking is very closely connected with Wit or Humour, and also closely connected with Originality. Its correlation with Humour is, as we see from our diagram (Fig. 1) cos 10° or 0·98 and its correlation with Originality is cos 28° or 0·88. Let us call it Cleverness. Then Cleverness is defined as a quality which is independent of 'g' but which, combined with 'g' in different proportions, wholly constitutes Humour or Originality, and wholly or mainly constitutes Quickness of apprehension. Being closely connected with Humour and with Originality, Cleverness as thus defined is also closely connected with the form of brain constitution described by Mr McDougall, in the passage we have

* The actual figure is 1·65, but the correlation cannot, of course, exceed +1. If, for example, $\cos^{-1} r_{oh}$ were ·70 instead of ·79 a difference approximately equal to the probable error, the partial coefficient $r_{og.h}$ would become 1·19.

[1] Garnett, *loc. cit.*

[2] *Principles of Psychology*, I. 578.

[3] *Physiological Psychology*, 139.

quoted. Moreover, so far as we can see, this form of brain constitution, like Cleverness as just defined, is independent of 'g.' We have therefore grounds for identifying it with Cleverness.

Let us now consider our diagram (Fig. 1) in the light of this suggestion.

Still having regard only to the five intellectual qualities named in the diagram, we observe that people may be intellectually exceptional in any number of different ways: the radius from O to the small area δA, while remaining of length ϵ, may make any angle θ with any one of the fixed axes, say Og. If ϵ remain large and θ increase, we start with men having great 'g'—great 'Ability,' perhaps great power of concentrating attention—but only average Cleverness. Such men will, according to the diagram, possess much more than average Originality and Quickness, but less than the average sense of Humour. As θ increases, ϵ remaining constant, δA will come to Oq and soon after to Oo, when θ is about 60°; for $gOq = \cos^{-1} r_{gq} \equiv \cos^{-1} 0{\cdot}53$, and $gOo = \cos^{-1} r_{go} = \cos^{-1} 0{\cdot}47$, according to Dr Webb's table. When, therefore, for a given degree of intellectual exceptionality, Quickness or Originality is greatest, Cleverness is about $\sqrt{3}$ times as great as 'g,' while Sense of Humour is well above the average. As θ continues to increase, the rotating radius brings δA to Oc. Then Cleverness is at a maximum (ϵ being given); Sense of Humour is nearly at a maximum, for the angle cOh is only about 10°; Originality and Quickness are much above the average; but Ability ('g') is only equal to the average. Finally, when δA comes to lie on Oh (so that Sense of Humour[1], or Wit, is the most exceptional quality of the exceptional men represented by points in δA), Cleverness, Originality and Quickness are all much above the average, but Ability is slightly below the average. This does not, of course, mean that very able men (men with very high 'g') may not have a great Sense of Humour; but only that, the greater their Ability ('g'), the greater must be their Cleverness to produce a given degree of Sense of Humour.

The distinction between Ability ('g') and Cleverness has been emphasised by Dr Mercier in an essay on "Cleverness and Capability[2]." He maintains that these two qualities "are quite different from one another, and may be developed to very different degrees in the same person," as the point representing that person in our diagram moves

[1] We here assume the correlation $r_{gh} = -{\cdot}17$, which is about double the probable error, to be a significant figure; so that the correlation between Ability and Humour is low and negative.

[2] *Human Temperament*, 1917.

from the neighbourhood of *Og* to the neighbourhood of *Oc*. Dr Mercier is further of opinion that "capability ['*g*']...may be inculcated by a proper training, but no training will make a stupid person clever[1]." Dr Mercier goes on: "Capability can be acquired, and it should be one of the main objects of education to see that it is acquired." The clever man of science "is fertile in hypotheses." "The clever shopman amuses his customers"; he is, in fact, a wit. "Capable people concentrate their attention on the matter in hand, think it out in all its bearings...." "Clever [but incapable] people are apt to make mistakes and go wrong because their attention is discursive." "From this lack of concentration it results that they do not think matters out."

William James, after pointing out that there are two stages in reasoning, the first of which—association by similarity—merely operates to call up cognate thoughts (Cleverness), and in the second of which attention is concentrated ('*g*') upon the bond of identity between these cognate thoughts, adds: "So men of genius may be divided into two main sorts, those who notice the bond and those who merely obey it. The first are the abstract reasoners properly so called, the men of science and philosophers—the analysts, in a word; the latter are the poets, the critics—the artists, in a word, the men of intuitions[2]." According to this classification, great men of science and philosophers would be represented by points lying far from the origin but near to *Og*, while equally great poets and artists would be represented by points lying equally far from the origin but near to *Oc*. The importance to the poet of having a large ratio of *c* to *g* was emphasized, although in other words (!), by Schiller in a letter written in 1788, and quoted by Professor Freud[3]. Schiller wrote (to a friend who complained of his own lack of creativeness): "The reason for your complaint lies, it seems to me, in the constraint which your intelligence imposes upon your imagination."

We may note in this connexion that the word 'genius' is more commonly used to denote exceptional Cleverness than exceptional '*g*.' Thus William James writes:

> Geniuses are, by common consent, considered to differ from ordinary minds by an unusual development of association by similarity....And as the genius is to the vulgarian, so the vulgar human mind is to the intelligence of a brute[4].

[1] The view that '*g*' is educable is inconsistent with that expressed by Mr Burt (This *Journal*, III. 176); but Mr Burt's argument that '*g*' is innate seems to me inconclusive. On the contrary, there seems to me to be a considerable balance of evidence that '*g*' is educable, although its educability may be innate.

[2] *Loc. cit.* II. 361.

[3] *The Interpretation of Dreams*, English edition, 85, 86.

[4] *Loc. cit.* II. 348.

Again, in another place William James says that

Geniuses are commonly believed to excel other men in their power of sustained attention. In most of them, it is to be feared, the so-called 'power' is of the passive sort. Their ideas coruscate, every subject branches infinitely before their fertile minds, and so for hours they may be rapt. But it is their genius making them attentive, not their attention making geniuses of them....It is probable that genius tends actually to prevent a man from acquiring habits of voluntary attention, and that moderate intellectual endowments [moderate c] are the soil in which we may best expect, here as elsewhere, the virtues of the will ['g'][1], strictly so called, to thrive. But, whether the attention come by grace of genius or by dint of will, the longer one does attend to a topic the more mastery of it one has. And the faculty of voluntarily bringing back a wandering attention, over and over again, is the very root of judgment, character and will[2].

At the same time, it is probable that genius, as the word is commonly used, is more directly measured by $\epsilon = \sqrt{g^2 + c^2}$ than by g (measuring general Ability or capacity to concentrate attention) alone, or even by c (measuring Cleverness, as we have defined it, or tendency to associate by similarity) alone.

If the independence of 'g' and 'c' is confirmed by further investigation, and if of these two 'g' alone be educable (although its educability may be innate), the distinction between 'g' and 'c' should have important consequences for education.

The correlation of c (the measure of Cleverness, as we have defined it) with the forty-eight qualities investigated by Dr Webb may be calculated as follows. Let us take the general factor measured by g, and Cleverness measured by c, as two of the independent variables in terms of which any of Dr Webb's qualities may be expressed. Then the measure of any of his qualities is given by equation (1), which now becomes:

$$q = r_{gq} \cdot g + r_{cq} \cdot c + l_3 x_3 + \ldots + l_N x_N \quad \ldots\ldots\ldots\ldots\ldots (11),$$

where $x_3, x_4, \ldots x_N$ are the remaining independent variables. Any of the qualities represented in our diagram, say Humour, is given by

$$h = r_{gh} \cdot g + r_{ch} \cdot c = r_{gh} \cdot g + (\sqrt{1 - r_{gh}^2}) \cdot c,$$

so that, according to equation (5),

$$r_{qh} = r_{gq} \cdot r_{gh} + r_{cq} \cdot \sqrt{1 - r_{gh}^2},$$

[1] In identifying 'g' with 'will strictly so called,' it is necessary clearly to distinguish the momentary effort of *will* that we identify with 'g' from the persistence of *purpose* that in English is often called by the same name. For example, when in the Lord's Prayer we say 'Thy will be done,' we mean 'Thy purposes be fulfilled.'

[2] *Loc. cit.* I. 423–4.

from which we obtain

$$r_{cq} = \frac{r_{qh} - r_{gq} \cdot r_{gh}}{\sqrt{1 - r_{gh}^2}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots(12).$$

All the r's on the right-hand side of this equation are given in Dr Webb's table of corrected coefficients for the students. If we substitute Dr Webb's values and calculate r_{cq} for all values of q, except $q = g$ and $q = h$, we obtain the following series of coefficients of correlation:

TABLE I.

No. in Dr Webb's schedule	Name of Quality	Correlation with 'Cleverness'
1	General tendency to be cheerful (as opposed to being depressed and low-spirited) ...	·97
2	Tendency to *quick* oscillation between cheerfulness and depression (as opposed to permanence of mood) ...	–·05
3	Occasional liability to extreme depression ...	–·57
4	Readiness to become angry ...	·15
5	Readiness to recover from anger ...	·33
6	Occasional liability to extreme anger ...	–·18
7	Degree of aesthetic feeling (love of the beautiful for its own sake)	·39
8	Degree of sense of humour ...	·985*
9	Desire to excel at performances (whether at work, play or otherwise) in which the person has his chief interest ...	·46
10	Desire to impose his own will on other people (as opposed to tolerance) ...	·58
11	Eagerness for admiration ...	·18
12	Belief in his own powers ...	·32
13	Esteem of himself as a whole ...	·30
14	Offensive manifestation of this self-esteem (superciliousness) ...	·12
15	Fondness for large social gatherings ...	·85
16	Fondness for small circle of intimate friends ...	·01
17	Impulsive kindness (to be distinguished from No. 18) ...	·50
18	Tendency to do kindnesses on principle ...	·40
19	Degree of corporate spirit (in whatever body interest is taken) ...	·68
20	Trustworthiness (keeping his word or engagement, performing his believed duty) ...	·07
21	Conscientiousness (keenness of interest in the goodness and wickedness of actions) ...	–·05
22	Interest in religious beliefs and ceremonies (regardless of denomination) ...	–·39
23	Readiness to accept the sentiments of his associates ...	–·29
24	Desire to be liked by his associates ...	·56
25	Wideness of his influence ...	·67

* This figure cannot be calculated from equation (12). The equivalent formula $r_{ch} = \frac{r_{oh} - r_{gh} \cdot r_{go}}{\sqrt{1 - r_{go}^2}}$ has therefore been used.

TABLE I (*continued*).

No. in Dr Webb's schedule	Name of Quality	Correlation with 'Cleverness'
26	Intensity of his influence on his special intimates	·84
27	Degree of 'tact' in getting on with people	·60
28	Extent of mental work bestowed upon usual studies	−·01
29	Extent of mental work bestowed upon pleasures (games, etc.) ...	·31
30	Degree of bodily activity during business hours	·64
31	Degree of bodily activity in pursuit of pleasures (games, etc.) ...	·37
32	Degree with which he works with distant objects in view (as opposed to living 'from hand to mouth')	−·07
33	Tendency *not* to abandon tasks in the face of obstacles	·39
34	Tendency *not* to abandon tasks from mere changeability	−·06
35	Quickness of apprehension	·95
36	Profoundness of apprehension	·59
37	Soundness of common-sense	·51
38	Originality of ideas	·88
39	Pure-mindedness (extent to which he shuns telling or hearing stories of immoral meaning)	−·45
40	Power of getting through mental work *rapidly*	·59
41	*Physique*, estimated by visiting doctor and lecturer in physical exercises	·16
42	General excellence of character, estimated by lecturers	·02
43	Estimate of general excellence of character, supplied by each prefect	·33
44	Examinational ability	·30
45	Athletics, estimated by captains and by a member of the college staff	·27
46	Experimental tests of intelligence, furnishing the correlations of 'g' with the forty-seven other qualities, as explained on pp. 35 to 38 of Dr Webb's paper	·00*
47	Degree of strength of will	·41
48	Degree of excitability (as opposed to being phlegmatic)	·22

Inspection of these figures indicates that Cleverness may be recognised in practice—as, for example, when interviewing a candidate for an appointment, to whose general Ability ('g') testimonials or examination results bear witness—by noting his sense of humour, general tendency to cheerfulness (which is perhaps difficult to judge on the occasion of such an interview!), or quickness of apprehension.

* The correlation between the independent variables c and g cannot be calculated from equation (12). That its value is zero follows from our definition of c.

5. *Group factors in tests of similar qualities.*

It is important to observe that, if Mr Burt's conditions for a hierarchy were completely satisfied by every set of mental tests, g would be a single general factor within the meaning of our definition; and that, if Professor Spearman's correlation between columns conditions were completely satisfied, g would be a single general factor of the q''s in equations (3). But the fact is that, while Professor Spearman's conditions are approximately satisfied by every set of sufficiently diverse tests, they are not satisfied by a set of tests of very similar qualities, such for example as ability to translate Latin and knowledge of Latin grammar. The absence of a single general factor within the meaning of our definition—or, in other words, the inevitable presence of group factors,—when the tests relate to a set of such similar qualities, has been repeatedly mentioned throughout the literature of this subject beginning with Professor Spearman's paper of 1904[1]. Thus when a set of tests relates to sufficiently dissimilar qualities the results of the tests may be approximately expressed by means of equations (3) or, if Mr Burt's conditions are satisfied, equations (4) containing on the right-hand sides a single general factor $g \equiv x_g$ (and therefore no group factors) together with specific factors, x_s ($s = 1, 2, \dots n$); and, in so far as these approximate expressions give correlations that differ from those experimentally determined by more than the probable error, the necessary small correction may be applied by adding to the right-hand sides of equations (3) or (4) group factors of which the coefficients are small compared with those of g or of the specific factors.

Next suppose that a set of tests is concerned with similar qualities. Each one of these qualities (or, if Mr Burt's conditions are not satisfied, its difference from a real multiple of an independent $\overline{n+1}$th quality), if considered in conjunction with dissimilar qualities, could be approximately expressed by means of the right-hand side of equation (3) or (4) into which only g and a specific factor enter; and it could be completely expressed by adding group factors of which the coefficients were small compared with those of g and of the specific factor. When, therefore, a set of similar tests is in question it is still convenient to express the measures (q's) of the qualities tested in terms of independent variables that include g among their number. But now the independent variables

[1] *Loc. cit.* 273. Cf. also Dr Webb, *loc. cit.* 53 (first line).

other than g that enter into the various q's (or q''s of equations (3)) will no longer all be specific factors: some will be group factors with significant coefficients.

Our new factor c is such a group factor. Along with g it enters into a number of intellectual qualities several of which as we have seen are expressible in terms of c and g alone without specific factors by means of equations of the form

$$q_i = g\cos\theta_i + c\sin\theta_i \text{ ....} (13),$$

where q_i is the measure of one of these intellectual qualities and θ_i is the angle which its axis makes with the axis of g measured in the direction from that axis to the axis of c. But, so far as this group of intellectual qualities is alone concerned, neither g nor c is a 'single general factor' within our definition.

6. '*Purpose,*' *a group factor in tests of certain character qualities.*

Another group factor which, along with g, enters into a number of purpose qualities—notably those numbered 34, 33, 18, 20, 21, 32, and 28 in the above Table I—was described by Dr Webb[1]. So far as this group of purpose qualities is alone concerned, neither g nor this new factor is a single general factor within the meaning of our definition; but any large series of sufficiently dissimilar mental tests may, as we have said, be so expressed that g and specific factors enter into all of them while this new factor, like our factor Cleverness, enters as a group factor to an extent which is insignificant in all but a small proportion of them.

Dr Webb demonstrated the existence of the new (group) factor by showing that if, by means of Yule's equation, the partial coefficients of correlation with g constant, between qualities Nos. 34, 33, 18, 20, 21, 4, 11 and 31 in the above Table I, were calculated and entered in a correlation table, the correlation between columns averaged ·94, and therefore approximately satisfied Professor Spearman's condition.

If we apply the test afforded by equation (8) to discover whether these eight qualities depend upon two, and only two, independent factors, of which we may take one to be g, we find that all the combinations of the first six qualities three at a time do satisfy all the series of conditions within the limits of probable error (assumed to be ·10 in

[1] *Loc. cit.* 58–60. But, as we are about to observe, the factor Dr Webb described, and measured by w, is not the same as the new group factor z, independent both of g and c, that is here in question. We shall, however, see that w and z have much in common.

Dr Webb's table), except that two combinations (34, 18, 20; and 34, 18, 4) of three of the six qualities miss satisfying the condition (within this limit of probable error) by 4° and 2° respectively. In view, however, of the fact that the correlations between qualities 34 and 33 are very high, as are also those between qualities 18, 20 and 21, it would follow that, if equation (8) is satisfied by qualities 34, 20 and 4, the same equation will be satisfied approximately by any other combination of three qualities out of the six. Too much importance must not, therefore, be attached to the conclusion that, within the limits of probable error that we have defined, the six qualities 34, 33, 18, 20, 21 and 4 are dependent upon two independent variables and upon two only. But this result is entirely consistent with Dr Webb's conclusion that apart from g, one and only one (group) factor, independent of g, is contained by these six qualities. It may be added that quality No. 11, in combination with any other pair of the six qualities just named, in most cases satisfies equation (8), and in other cases very nearly does so. Quality No. 31, however, does not satisfy our test in combination with pairs of the remaining seven qualities.

Dr Webb discusses the nature of this new group factor, and recognizes that it is in some close relation to 'persistence of motives.' He adds that "this conception may be understood to mean *consistency of action resulting from deliberate volition, or will*[1]." But will, used in this sense, is liable to be confused with the word as used to describe an effort of will. It would perhaps be better to describe the new (group) factor as being intimately related to Purpose[2]. It may, for example, measure the extent to which thought and action are influenced by interest—the individual's whole system of innate and acquired neurograms—and especially interest in the future.

Dr Webb's new factor, as defined in the next paragraph, is independent of g.[3] It is not the same as our new factor Cleverness; for the calculated correlations between the seven qualities in which the new factor has been shown to enter as a general factor, and Cleverness, are respectively:

[1] *Loc. cit.* 60.

[2] Cf. first footnote on p. 355 above.

[3] But, in discussing its nature, Dr Webb was more concerned with the quality which we are going to call Purpose and of which the axis Op in Fig. 2 below is *not* at right angles to Og, than with the quality independent of 'g' of which the axis is Oz in that figure and is at right angles to Og.

Table II.

No. in Dr Webb's schedule	Name of Quality	Correlation with 'Cleverness'
34	Tendency *not* to abandon tasks from mere changeability ...	−·06
33	Tendency *not* to abandon tasks in face of obstacles	·39
18	Kindness, on principle	·40
20	Trustworthiness	·07
21	Conscientiousness	−·05
4	Readiness to become angry	·15
11	Eagerness for admiration...	·18

There is therefore a third factor, independent both of g and c, which enters into the constitution of a number of character qualities. But the character qualities in question may depend on more than these three independent factors. Suppose we take g and c as the first two independent factors. Then we may express the measure q of any of these character qualities by means of equation (11): namely,

$$q = r_{gq} . g + r_{cq} . c + l_3 x_3 + \ldots + l_N x_N \quad \text{..............(11)},$$

where $x_3, \ldots x_N$ measure the remaining independent factors. We may then define the new factor x_3, whose existence was proved by Dr Webb, as that which gives the highest average value of l_3 for all the q's in question.

If, in Fig. 2 below, we represent Dr Webb's new factor, thus defined, by an axis Ox_3 or Oz at right angles to the plane of our former diagram

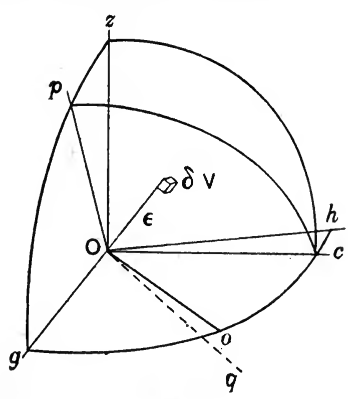

Fig. 2.

(Fig. 1) the axes corresponding to the seven qualities in which the existence of this new factor has been demonstrated, and of some others—notably No. 32 (Degree with which he works with distant objects in view) and No. 28 (Extent of mental work bestowed upon usual studies) into which Dr Webb's new factor also enters in a high degree[1]—lie in or near the plane zOg, since all these qualities have small correlations, with Cleverness, the axis (Oc) of which is perpendicular to that plane.

Of the seven qualities (Nos. 34, 33, 18, 20, 21, 4 and 11) named in Table II, the first five have high positive correlations with each other and with the two qualities (numbered 32 and 28 in Table I) to which reference has just been made; and all these seven qualities (Nos. 34, 33, 18, 20, 21, 32, 28) have negative correlations with the last two qualities (Nos. 4 and 11) in Table II[2]. Let us now for shortness describe the seven qualities (Nos. 34, 33, 18, 20, 21, 32 and 28) having high positive correlations with each other as 'purpose' qualities: a description suggested by the names of these qualities in Table I above and by the further particulars of the qualities in question given by Dr Webb[3]. To these seven purpose qualities we may, if we please, add the inverses of qualities No. 4 and No. 11, defined as being measured by the measures of No. 4 and No. 11 respectively with their signs changed. Suppose that p represents the centroid of the points in which the axes of the various purpose qualities meet any sphere described with O as centre. Then, since the average correlation of all these nine qualities with Cleverness is very small (·04), Op lies very near, if not actually in, the plane zOg; and it lies much nearer to Oz than to Og[4].

If now we repeat for our three-dimensional diagram (Fig. 2) the description that we gave for our two-dimensional diagram (Fig. 1) by imagining a small solid element, δV, between the three perpendicular axes Oz, Og and Oc; and if we consider especially positions of δV near the plane cOp; we shall find that exceptional men whose mental qualities, in so far as they depend upon the three independent variables measured along Og, Oc and Oz, are represented by points lying inside δV belong to one or other of two well-recognized types, according as δV lies near to Op on the one hand or to Oc on the other.

Take, for example, the following comparison quoted by Dr Webb[5]

[1] See Webb, *loc cit.* 59.

[2] Webb, *loc. cit.* Table VI.

[3] *Loc. cit.*, Appendix II, pages 84 *et seq.*

[4] Taking Dr Webb's figures the angle gOp lies between 70° and 80°. Dr Webb found that the correlations of the purpose qualities were very small in the case of schoolboys. This suggests—what is probable on other grounds—that the angle gOp decreases as years go on.

[5] *Loc. cit.* 61.

as a sample of the observations on types of character made by G. Heymans and E. Wiersma:

	Avaricious.	*Spendthrift.*
1.	Man of principle.	Not so.
2.	Not emotional.	Emotional.
3.	Depressed and gloomy.	More cheerful and lively.
4.	Anxious and serious.	Irresponsible.
5.	Hard to appease.	Easily appeased.
6.	Adheres to his once-formed opinion.	
7.	Man of habit.	Change-loving.
8.	Works for a far-off goal.	Works for an immediate result.
9.	Answers questions in a provisional manner.	Answers positively.
10.	Mathematical talent.	No mathematical talent.
11.	No artistic ability.	Talent for music, drawing, authorship, dramatic art.
12.	Not witty.	Witty.
13.	Quiet and reserved.	Not so.
14.	Not given to telling anecdotes.	Given to telling anecdotes.
15.	Does not speak much in public.	Fond of speaking in public.
16.	Never in debt.	Often in debt.
17.	Not easy to manage.	Easy to manage.
18.	Brings up his children strictly.	Allows much freedom.
19.	Conservative.	Radical.
20.	Hermit-like.	Pleasure-loving.
21.	Not a sophist.	A sophist.
22.	Not sport-loving.	Sport-loving.
23.	Interested in finding out the means of others.	Not so interested.
24.	Morose.	Complimentary.
25.	Dignified and precise.	Ironical.
26.	Slow and short in speech.	Loud in speech.
27.	Seldom or never laughs.	Laughs much.

Dr Webb gives this example to illustrate a greater or less degree of his new factor. But the typical spendthrift differs from the typical avaricious character, not only in possessing less purposefulness, but also, according to the above analysis, in being more witty and more cheerful, in having more artistic ability and less mathematical talent. For all these reasons, the typical spendthrift must be represented by a point which, since he is 'typical,' i.e. an extreme representative of a type, is at some distance from the origin, and which not only lies a long way from the 'Purpose' axis, Op, but also lies near the 'Cleverness' axis, Oc.

Take another comparison, also quoted by Dr Webb:

Harold.	*Earl.*
More control of voluntary movement.	In free movement quicker, showing impatience and lack of control.
Industrious—more patient and persistent in performing tasks.	Restless and animated in school, given to idle occupation. More easily distracted.
	More competitive spirit.
More helpful—dependable—regular in his expenditure of energy and in application of effort.	Has periods of feverish activity, alternating with periods of listlessness.
Always willing to try, but never expected to do *very* well.	Great enthusiasm in attempting a new task, but enthusiasm soon exhausted.
More moral, good-natured, polite, keen to do work well, at times sensitive and morose.	More sunny and laughing; was a tease.
Less self-confident.	Bolder in social relations, always acted as spokesman.
More sympathetic and generous.	More often acting apparently to show off.
More conscientious in his work.	Impatient of detail.
More genuinely pleased at praise.	
Neat and careful writing.	Careless, hasty and irregular work in copy-books.
Superior in literal memory, both when tested after a year and after five minutes study.	Superior in rapidity of complex mental processes.
Association reactions show more clear and more stable mental imagery, and perhaps less in quality.	Reactions much less uniform—a more variable mental process; more frequently caused by recent experiences and objects in the immediate environment.

Earl's superiority in rapidity of complex mental processes indicates that he differs from Harold, not only in having less 'Purpose,' as Dr Webb points out, but also in having more 'Cleverness.'

In fact, the type of temperament which Dr Webb and others[1] have distinguished from the purposeful temperament differs from it, not merely in having less than the average degree of Purposefulness, but also in having more than the average degree of Cleverness.

[1] For example, William James's "explosive Italian" (*Principles of Psychology*, II. 538) possessed more than average (i.e. a positive degree of) Cleverness, as well as less than average (a negative degree of) Purposefulness.

7. *Summary of Conclusions.*

1. It has here been shown that, unless we carefully define beforehand what we mean by general factors and group factors, much confusion is likely to arise in discussions of the question whether the correlations obtaining between a set of mental tests are due, on the one hand, to a single general factor—'general ability'—entering without group factors into all of the qualities tested, or, on the other, to an indefinitely large number of independent factors each of which may enter as a group factor into any two or more of the tests.

2. If Professor Spearman's 'correlation between columns' conditions are satisfied by the correlations between the measures q_1, q_2, ... q_n of n tested qualities, the n differences q_1', q_2', ... q_n' between the respective q's and a real multiple of an $\overline{n+1}$th variable y, independent of the q's, will satisfy Mr Burt's conditions for a hierarchy.

3. Mr Burt's conditions for a hierarchy are equivalent to $\frac{1}{2}n(n-3)$ independent relations between the n mental qualities tested.

4. Were Mr Burt's conditions satisfied by the correlations obtained in a set of mental tests, each of the n qualities tested would be expressible in terms of two independent factors of which one was specific, appearing in that quality alone, while the other was a single general factor common to all the qualities. The n qualities would thus be expressed in terms of $n+1$ independent factors, viz. a single general factor and n specific factors.

5. The fact that the correlations between any set of sufficiently dissimilar tests approximately satisfy Professor Spearman's or Mr Burt's conditions is evidence that the measures of the qualities tested (or, if Mr Burt's conditions are not satisfied, the differences between these variables and a real multiple of an $\overline{n+1}$th variable independent of them all) may be expressed as made up for the most part of a single general factor and specific factors together with a comparatively insignificant part due to group factors; or, put mathematically,

$$q_s' = (q_s - ky)(1+k^2)^{-\frac{1}{2}} = l_s \cdot g + m_s \cdot x_s + {}_sn_1 \cdot z_1 + {}_sn_2 \cdot z_2 + \dots,$$

where q_s is the measure of the sth quality tested, k is a real quantity which vanishes (making $q_s' \equiv q_s$) when Mr Burt's conditions are satisfied, g is the general factor (which would be a single general factor were the coefficients of the group factors all zero instead of only being on the average very small), x_s is a specific factor, and z_1, z_2, ... are group factors whose coefficients (n's) tend severally and in the aggregate to be very small compared with the coefficients (l's) of g or (m's) of the specific factors.

6. In addition to g, group factors enter to a significant extent into certain groups of similar qualities.

7. Thus certain intellectual qualities may be regarded as compounded (according to the formula expressed in equation (10)) of g, General Ability, and one other independent factor c, which we described as Cleverness.

8. Into the constitution of a number of purpose qualities having a most intimate connexion with character there enters to a significant extent not only g but also a group factor z that is independent both of g (General Ability) and of c (Cleverness). The new factor described by Dr Webb as 'Will,' but as we have argued better described as 'Purpose,' is not the same as z but is a compound of z with g and possibly, but to an insignificant extent, with other independent factors also.

(*Manuscript received* 9*th March* 1919.)

JOINT NOTE ON "THE HIERARCHY OF ABILITIES."

By J. C. MAXWELL GARNETT and GODFREY H. THOMSON.

SINCE the above article[1] was set up, the correspondence which has passed between us concerning it, and concerning certain papers on related subjects, which we have, separately, published elsewhere, has induced us to make a brief statement of the points on which we find ourselves in agreement; for we fear that our conclusions (independently arrived at) may to a casual reader appear more antagonistic than is, we believe, really the case. We therefore submit the following paragraphs as common ground, though not of course as an exhaustive statement of the conclusions or opinions of either of us on this matter.

The assumption expressed by the 'all-or-none' condition in the conclusion of the above paper may be put mathematically by saying that, if the n correlated variables q, and the N independent factors x of which they are made up, are each distributed according to the normal law, and if the units in which each is measured are so chosen that the frequency distribution of each variable, dependent or independent, has the same standard deviation, the effect of the assumption is that the measure q_s of the sth of the correlated variables is given by

$$q_s = \frac{1}{\sqrt{N_s}} \text{ (sum of } N_s \text{ whole } x\text{'s)} \qquad \text{.................. (1)},$$

instead of by the more general form

$$q_s = {}_sl_1 \cdot x_1 + {}_sl_2 \cdot x_2 + {}_sl_3 \cdot x_3 + \dots \qquad \text{.................. (2)}$$

when the l's have any values whatever, subject only to

$${}_sl_1^2 + {}_sl_2^2 + {}_sl_3^2 + \dots = 1 \qquad \text{.................. (3).}$$

If the q's are formed from the x's by chance sampling, according to equation (1), the correlations of the q's will not in every case approximate to perfect hierarchical order. But nevertheless, even with a finite number of x's, there is a tendency towards such hierarchical order. The cases are distributed so that a wide departure from hierarchical order

[1] This *Journal*, IX. 337–344.

is improbable, while an approximation to hierarchical order is probable. Indeed, if the average of a large number T of chance distributions of N x's among the q's in this way be taken to form a new set of correlated q's, namely $\bar{q}_1, \bar{q}_2, \dots \bar{q}_n$, then the correlations between these new variables will always be in good hierarchical order, which becomes perfect when T and N become infinite.

If the hierarchy is perfect there is one and only one general factor g and no group factors, but there may be any number of specific factors in addition.

Imperfect hierarchies formed by the correlations between n q's may therefore be accounted for *either* (a) by supposing that a single general factor and n specific factors are present, together with comparatively insignificant group factors which vanish in the limit when imperfection becomes perfection; *or* (b) by supposing that the correlations are due to a number of elements that are additive, each acting on the 'all-or-none' principle, which may appear in any number of tests but are not necessarily present in all—that is, group factors. In the limit when imperfection becomes perfection these elements become the infinitesimal elements, infinite in number, of the single general factor, g: in fact

$$g = \frac{1}{\sqrt{N}} (x_1 + x_2 + x_3 + \dots + x_N).$$

For reasons which are not relevant in this connexion, we differ in the order of preference we assign to these alternative hypotheses, each of which, however, we agree to be mathematically possible.

(*Manuscript received* 10*th May* 1919.)

PUBLICATIONS RECENTLY RECEIVED.

Psychological Principles. By Professor JAMES WARD. Cambridge: University Press. 1918. Pp. xvi + 478. 21*s.* net.

There is only one person who could suitably contribute to the Cambridge Psychological Library a work with this title: the eminent teacher who is not only the *doyen* of British psychologists but also the man who first illustrated in the field of mental science the characteristic tradition of the University of Newton and Clerk Maxwell. Professor Ward apologises in his preface for putting out a book which is substantially only an expansion of the famous article in the *Encyclopaedia Britannica.* The apology was unnecessary, and suggests a doubt of welcome which will surely be falsified. An authoritative study of psychological *principles* must clearly be included in a series of which other volumes will presumably deal with special provinces or aspects of the science and reflect the wide divergencies of method among its present investigators; and there could hardly be an exposition more suitable than one based on the treatise which no less a master than William James saluted as masterly.

While the old lines remain for the guidance of younger inquirers, one-third of the book is almost entirely new,—more than enough to tempt maturer students to refresh their acquaintance with one of the classics of the science. The new chapters deal with intellection and belief, the self and self-consciousness, conduct, and the development of the concrete individual. They fit so well into the original structure that no new reader would suspect the joints, though those familiar with the author's intervening works may feel the influence of his recent preoccupation with philosophical, as distinguished from scientific, problems. Moreover they show that the lapse of years has not affected Professor Ward's splendid powers. There is the same virility of thought, the same lucidity of exposition, the same literary artistry; while the two concluding chapters on the concrete individual constitute an admirable and moving piece of work which must rank as one of his finest achievements.

Dr Ward confesses that he is less concerned about the "patches" in his new book than about the "holes." It must be admitted that his index contains, so to speak, some rather surprising omissions. It directs the reader to no discussions of certain important questions round which recent psychological debate has largely centred. A single disparaging sentence in a footnote (p. 356) contains, perhaps, the only mention of Freud, while one nowhere comes across the name of Jung. The practical value of the work of these psychologists is something with which, however great it may be, a writer on principles may rightly hold that he is not concerned. But the controversy between them and their schools has raised in a very definite form an issue of fundamental importance—namely, the scope and validity of mechanistic and teleological interpretations in psychology. It must be regretted, therefore, that Dr Ward has not brought his powerful analysis to bear upon the "facts for psychology" which these writers and their followers have placed on record, and the theoretical notions they have based on them. Again, it is to be regretted that Dr Ward in his references to instinct has not dealt specifically with Mr McDougall's views, which, whether they are sound or not, have at least stirred up a great amount of thought upon the subject, and have widely influenced current expositions of psychology and the sciences that draw from it. For those views present a theory of behaviour and conduct which it is not easy, *primâ facie,* to harmonise with Dr Ward's own. Lastly, one must regret that Dr Ward's impatience with the "exact" inquirers to whom he refers somewhat slightingly on p. 433 did not permit him to deal constructively with the results of the controversy in which Professor Spearman has been the storm-centre—results that can hardly be judged to have no positive bearing upon psychological principles. One can only hope that Professor Ward may be able both to enlarge and to fulfil the half-promise made in his preface, and to define fully elsewhere his position towards these and other matters which are dealt with in his present book only obliquely or not at all.

According to Dr Ward, the fundamental datum of psychology is the duality of subject and object in all experience, and he constantly shows how vital it is always to bear this datum in mind. There can be no question that this principle is exceedingly important, that the main results to which it leads are wholly true, and that any principles which exclude those results are somewhere fatally defective. Yet one is left obstinately questioning whether the subject, "the pure Ego" as Professor Ward conceives it, has any legitimate place in an empirical science. The doubt, which makes one uneasy at many points of the exposition, becomes acute where Dr Ward proceeds to castigate with relentless severity James's well-known view that the *I* is the "judging Thought" of the moment. One cannot think that his strictures are wholly deserved. James was apt in his generous way to express himself with less than Euclidian precision, and was, moreover, hampered by the rather risky metaphor of the "stream of consciousness." What he meant to express in his statement about the "judging Thought" was, that in all experience there are two things, an experiencing and its object, and that successive experiencings have to one another unique relations which, at the level of self-consciousness, are also experienced. Beyond this he thought it impossible for psychological science to go. Professor Ward, as the reviewer understands him, thinks we may and must go farther. To the question, What is it that experiences? James is contented to reply, in effect, that the experiencing experiences. Professor Ward rejects this answer as a "flagrant absurdity" (p. 381), and insists that there must be an experiencer that is more than a grammatical subject of the verb to experience. Thus we reach the pure Ego, a being with one capacity—feeling, and one faculty—attention (p. 371). There is a common objection to this conclusion that Dr Ward easily destroys: the objection, namely, that the experiencing subject cannot be known, that is, cannot be an object. But his explanation, that the subject, though not known as an object, is yet experienced, still lies open to the objection that what is experienced is not a real subject but simply the experiencing. And if this objection is waived or defeated, there is a further difficulty which one cannot feel sure that Dr Ward has overcome. It may be presented in the form of a dilemma. The term "subjective selection" which is applied to the activity of the pure Ego must be understood either as a metaphor modelled on "natural selection," or in the plain sense of the word "selection." In the former case, how can the subject be said to "initiate," except (again) in a metaphorical sense? In other words, does not Professor Ward's psychology reduce, after all, to a quasi-mechanistic system like Herbart's? In the latter case, must the subject not be credited with much more than the capacity for feeling and the faculty of attention? Must it not contain, in fact, a structure of the same general character as the empirical Me? If so, it would seem to reduplicate the latter superfluously; if not, its power of "selection" appears so mysterious that to bring it into a psychological interpretation would again be merely to complicate gratuitously the facts to be explained. And the mystery is greatly thickened by the circumstance that the origin of the subject seems to lie altogether outside the scope of heredity as that term is commonly understood (pp. 424–5).

In face of these difficulties it may, after all, be but prudent to apply the razor of Occam to the subject regarded as an explanatory factor in psychology. The waywardness of the "objective" psychologists consists, in that case, in the fact that they have performed the operation so clumsily as to eliminate with the subject the indubitable features of experience it is meant to explain. Perhaps some sympathetic critic will show how it ought to be done.

Human Nature and its Remaking. By WILLIAM ERNEST HOCKING, Ph.D., Professor of Philosophy in Harvard University. New Haven: Yale University Press. 1918. Pp. xxvi + 334. 12*s*. 6*d*. net.

This is not a treatise on Education, nor even (it may be said) a practical treatise, but a philosophical discussion of human nature and the agencies internal and external that mould and remould it. There are, says the author, three vital questions: "What is original human nature? What do we wish to make of it? How far is it possible to make of it what we wish?" The first of these questions he answers, without any pretence to dogmatism or finality, in the second Part of his work—on *The Natural Man*; and this part contains most of its psychological interest.

Prof. Hocking takes it as a working hypothesis to begin with, that the entire human being is originally a bundle of instincts; and that the self, if at any moment contrasted with a given instinct, may then be regarded as representing all the other instincts. Hence, to describe original human nature, we must try to determine the number and relations of the instincts. Several grades of instinct may be recognised: (1) *Units of behaviour*, such as reaching, biting, walking, etc., which are used in various combinations in the major instincts, and fall most readily under "the formula of sense-stimulus and specific response." Some writers regard these as the only true instincts; but (2) there are more *general* instincts arising from the combination of many units to a single serviceable end, such as flight—excitable by many different stimuli and manifesting various reactions. General instincts fall into pairs: sociability and antisociability, for instance, dominance and submission, and so on. (3) Some instincts, again, such as fear, play and pugnacity, have, besides their primary manifestations, a wider form as "an instinctive control of instinct." They are then instincts of the *second order*. Pugnacity, e.g., controls primary fear; it makes an obstacle a spur rather than a discouragement. (4) Finally, there are *central* instincts, "in which the stimulus as well as the response are (*sic*) primarily central." The best example of these is curiosity, not in its primary form, but "as an independent hunger of the mind." Aesthetic tendencies and delight in rhythm belong to the same group.

One finds at p. 56 an interesting tabulation of these constituents of human nature: they are classed as positive or expansive and negative or contractive; units of behaviour are printed in italics; "indentation" distinguishes primary instincts from their subordinate impulses; and secondary instincts are printed in large type across the columns.

Secondary instincts, however, are not separate and independent activities, but are fused in the will. Beginning with the self "as a permanent principle of selection," the course of experience with good and evil brings about "a stable *policy* towards incoming suggestions and impulses. And to have a stable policy is to have, in the specific sense of the word, a *will*." If men are alike in nature, there must be in all instincts "a nucleus of common meaning which we should be justified in calling the fundamental instinct of man." No one description of this is sufficient, but "the will to power" expresses a large part of the truth.

There is not space for much criticism. The distinction between primitive and secondary instincts seems right and important. But one misses in this account of original human nature any appreciation of the intelligent and imaginative side of man, apart from which that distinction of instincts would not exist, and which is too characteristic and influential to be merely taken for granted. Moreover, no list of qualities of human nature, however exhaustive, though summed up in "the will to power" (how empty are such generalisations!), can give a just idea of the original nature of man, without a picture of the life in which these qualities displayed themselves, and which decided their proportionate values. This raw man, who is to improve in the course of many suns, in what circumstances did he live, and what did he do there?

The remainder of the book treats, in successive parts, of Conscience, Experience, Society, Art and Religion, Christianity. There is a sort of inherent dialectic of the instincts by which they struggle toward social, civilised and ideal values. Much interesting reflection occurs on these matters—sincere, refined and always rightly-felt. Christianity, by the way, is treated without theological implications: it means the ethics of the gospels; and to call it Christianity is a little misleading. Lessing has a short but instructive essay entitled *Die Religio Christi und die christliche Religion.*

Dreams and Primitive Culture. By Dr W. H. R. Rivers, M.A., F.R.S. Manchester: University Press. 1918. Pp. 28. 1*s*. net.

This highly suggestive lecture, delivered in the John Rylands Library, is here reprinted from the Library *Bulletin*. Its purpose is first "to consider the psychological mechanism by means of which the dream is produced, and then to compare this mechanism with the psychological characters of the social behaviour of those rude peoples who are our nearest representatives of the early stages of human progress." Accordingly, after having described the chief transformations—through dramatization, symbolization, condensation, and secondary elaboration—undergone by dreams, Dr Rivers proceeds to demonstrate the existence of these same processes in the imagery, magical and social customs, dramatic and pictorial art and in the general culture of various primitive peoples. The lecture is full of interest in its attempt to establish parallels between the psychology of dreams and that of primitive man.

War Neuroses. By Dr John T. MacCurdy. Cambridge: University Press. 1918. Pp. 132. 7*s*. 6*d*. net.

This is one of the most remarkable books which have appeared during the war. It is written by an American physician who, with intimate previous experience of the psycho-neuroses of civil life, came to England in 1917 and paid brief visits to seven different hospitals, in order to advise the medical profession in America on the special problems which his country was about to face on its entry into the war.

It is a reprint of a paper contributed by him in July 1917 to the *Psychiatric Bulletin* of the New York State Hospitals, now prefaced by a sympathetic introduction from Dr Rivers, whose valuable work in the psycho-neuroses of the war is so well known.

Captain MacCurdy's book is a striking testimony to his preparedness for the task entrusted to him. In a few weeks he was able to indicate some of the main causes and the proper treatment of the functional nervous disorders of warfare, towards which it had taken three years for this country to grope its way, in the face of considerable opposition from neurologists who neglected all but the obvious bodily symptoms, and from those psychiatrists who, tending to regard all delusions, obsessions, and hallucinations as symptoms of insanity, were content to treat them as they treated asylum patients, merely with rest, drugs, or occupation. The book also testifies how far the best opinion in America is in advance of that in any European country as regards the proper attitude towards mental disorders. It is written in non-technical language and is eminently sane and psychological in character.

For those who have had longer experience than Capt. MacCurdy in the psychoneuroses of war, especially for those who have seen cases near the firing line, it is not difficult to criticize some of the conclusions to which he has come. His division of these disorders into Conversion Hysterias and Anxiety States is unsatisfactory; his efforts to trace all gross bodily functional disabilities to a wish to escape from the firing line are mistaken; his perpetual desire to obtain from the patients a confession of previous fear of bombardment is dangerous. But such blemishes are inevitable in the circumstances in which the book was written, and they are negligible compared with the untold benefits which its perusal will confer on those who wish to consult the best available presentation of the most successful lines of treatment of these important disorders of warfare.

The Modern Treatment of Mental and Nervous Disorders. By Dr Bernard Hart. Longmans, Green & Co. 1918. Pp. 28. 1*s*. net.

In the short space of an hour's lecture delivered at the University of Manchester, on March 25th, 1918, Dr Hart has given a good account of the aims of psychotherapy. He shows that in patients presenting nervous or mental symptoms the chief fault may lie in the constitutional make-up, in physical changes in the brain, or in mental causes. He points out that from the therapeutic point of view the most hopeful of these causes is the last, and that therefore steps should be taken to spread the teaching of the methods for investigating and treating mental states. In the short time at his disposal Dr Hart is unable to indicate how this should be accomplished, but he succeeds in giving a succinct and lucid description of the idea of conflict, the explanation of which is often followed with difficulty.

For many years to come it seems likely that there will be a larger number of patients suffering from these disorders than was the case before the war. This little book will stimulate the practitioner to study these serious but curable conditions.

Studies in Word-Association: experiments in the diagnosis of psycho-pathological conditions carried out at the Psychiatric Clinic of the University of Zürich under the direction of Dr C. G. Jung. Authorised translation by Dr M. D. Eder. London: Heinemann. 1918. Pp. vii + 575. 25*s.* net.

This book contains the following series of papers emanating from Jung's school and previously published in the *Journal für Psychologie und Neurologie.* I. Upon the significance of association experiments, by Prof. Bleuler. II. The associations of normal subjects, by Drs Jung and Riklin. III. The associations of imbeciles and idiots, by Dr Wehrlin. IV. Analysis of the associations of an epileptic, by Dr Jung. V. Reaction-time in association experiments, by Dr Jung. VI. Consciousness and association, by Prof. Bleuler. VII. Psycho-analysis and association experiments, by Dr Jung. VIII. Cases illustrating the phenomena of association in hysteria, by Dr Riklin. IX. Association, dream, and hysterical symptoms, by Dr Jung. X. On disturbances in reproduction in association experiments, by Dr Jung. XI. Statistical investigations on word-associations and on familial agreement in reaction-time among uneducated persons, by Dr Fürst. XII. On the psychogalvanic phenomenon in association experiments, by Dr Binswanger. XIII. On the physical accompaniments of association processes, by Dr Nunberg.

The translation, though better than that of many books of this class, leaves much to be desired. The following is an abstract of the contents of the more important investigations:

II. The educated have a shallower and more superficial reaction type than the uneducated, for whom the reaction experiment is something strange, who know a word less as a mere verbal sign than as something with a meaning or a question, and are unused to single words divorced from sentences. The educated person shows more meaningless reactions, more associations by similarity in sound, more 'egocentric' reactions. He controls himself less and adheres less strictly to the stimulus word. Sexual differences are very slight. Lessened attention induces a more superficial reaction.

III. Feeble-minded persons seldom react with one word: they usually answer in several words or in whole sentences, because, like the uneducated (II), they apprehend the stimulus word as a question. When asked to reply by a single word, the experiment is "soon ruined by the limited apprehension, the verbal poverty, and the consequent formal method of expression." They reply, typically, with a definition. Their reaction-times are slower than normal, and the stimulus word is often repeated.

IV. In the epileptic patient examined, as in uneducated persons (II), superficial word-associations are rare; the associations are in part affected by a morbid complex. As in the feeble-minded (III), the associations take the form of a sentence, the reaction-times are unusually long, the stimulus word is often repeated, and the definitions have an awkward circumlocutional character. Perseveration of the emotional tone, carried over from previous stimulus words, produces long reaction-times, the inhibitory effect of the emotional tone setting in with abnormal slowness, and generally only being perceptible in the reaction following the critical reaction.

V. The average reaction-time for educated and uneducated is 1·8 secs., 1·6 for the male, 2·0 (misprinted 2·9 on p. 264) for the female, 1·5 for the educated, 2·0 for the uneducated; the shortest reaction-times, save among the educated, being to concrete stimulus words, the longest to general concepts and verbs. About 83 per cent. of the stimulus words causing prolonged reaction-times have a very strong emotional tone, only about 17 per cent. owe this prolongation to their difficulty or rarity. The emotional tone may disturb the reaction following on the critical one. Sound associations are generally long because their occurrence is due to some disturbance of the subject.

XII. Only affectively and emotionally toned mental processes produce the psychogalvanic phenomenon, wide deviations of the galvanometer corresponding generally to long and complex reactions. If the stimulus be repeated often enough to lose its emotional effect, the psychogalvanic phenomenon disappears. A long reaction-time may occur without an increase (or with a lessening) of the psychogalvanic response, when the emotional tone perseveres from a critical to the next reaction, or when the stimulus, being imperfectly apprehended, arouses an 'intellectual feeling' of uncertainty or hesitation. A normal reaction-time may occur with the psychogalvanic phenomenon, when the subject is able to reply immediately by a verbal reaction—for which he may have been already prepared.

XIII. Movements of the arm or hands are stronger with critical than with indifferent associations. Respiration is inhibited through complexes, especially when they are unconscious. Conscious complex associations usually result in a stronger psychogalvanic phenomenon than occur with unconscious complex associations.

Papers on Psycho-Analysis. By Dr Ernest Jones. Revised and Enlarged Edition. London: Baillière, Tindall & Co. 1918. Pp. x + 718. 21*s.* net.

A detailed review of the second edition of this well-known book is unnecessary. For it is universally recognised as the best exposition of Freud's views in the English language, and its re-appearance, after having been out of print for more than three years, will give wide satisfaction. Twenty-one new chapters have now been added, of which eleven have not been printed previously in English. The forty papers of the present edition are grouped by Dr Jones under the following five heads—general papers, papers on dreams, and on treatment, clinical papers, and papers on education and child study. They are written in the clear style and logical manner which have always characterized the author's publications. Inevitably in such a collection of papers there is an abundance of needless repetition; and by so enthusiastic a devotee of Freud there is an almost complete absence of criticism of his hero, together with a certain intolerance and prejudice towards those who hold more moderate or more complicated views than Freud on the subject.

The Psychology of Conviction: a Study of Beliefs and Attitudes. By Professor Joseph Jastrow. Boston and New York: Houghton Mifflin Company. London: Constable & Co., Ltd. 1918. Pp. xix + 387. 10*s.* 6*d.* net.

"It has been made plain as never before," says Professor Jastrow, "that the strength and direction of men's convictions—authoritatively formulated in loyalties—furnish the decisive motive power of the world's energies. Under this stimulus the need of inquiry into the mental processes that generate and direct convictions becomes increasingly imperative." With these words there can be little disagreement; a book on the psychology of conviction needs no apology at the present moment. The reader's complaint is more likely to be that Professor Jastrow has not given us psychology enough, that the discussion of the mental processes in virtue of which men adopt certain beliefs and attitudes is not sufficiently fundamental and exhaustive. The reader will probably feel that in a treatise by a professor of psychology he has a right to expect a more penetrating and illuminating method of treatment than the somewhat arid and superficial analyses which Professor Jastrow for the most part has to offer, and on concluding the work he may well experience a sense of disappointment that a student of the human mind should have nothing more profound or novel to say as regards the intimate nature and causes of conviction—a subject connected with some of the most important problems of individual, general and applied psychology.

On the purely descriptive side, however, the book will be found of greater value. It consists of eleven separate essays; the first three—on The Psychology of Conviction, Belief and Credulity and The Will to Believe in the Supernatural, respectively—

being of a general nature, while the subsequent ones are each devoted to a special topic illustrative of the nature and working of conviction. The subjects of these are: The Case of Paladino, The Antecedents of the Study of Character and Temperament, Fact and Fable in Animal Psychology, Malicious Animal Magnetism, The Democratic Suspicion of Education, The Psychology of Indulgence, Alcohol and Tobacco, The Feminine Mind, Militarism and Pacifism. In the earlier of these essays will be found a presentation and discussion of a number of interesting facts which undoubtedly provide excellent and varied examples for the study of the conditions and development of belief, though unfortunately the discussion remains for the most part at the relatively superficial level which we have deplored. In the later essays there is a tendency to desert the truly psychological standpoint altogether for an orderly presentation of the arguments advanced for and against militarism, feminism, indulgence in alcohol, etc.—a task which is, however, for the most part performed in an admirably impartial and judicious manner.

Many readers will probably regret that there is not more frequent reference to, or use of, the psycho-analytic and experimental methods. One might suppose that a psycho-analytic study of delusions would form in many respects an admirable starting-point for the psychology of conviction in general. The regret is intensified by the fact that when Prof. Jastrow does make use of psycho-analytic conceptions, he is sometimes able to make interesting and stimulating suggestions; as when he says (p. 27) that "the Teutonic insistence upon the superiority of German 'Kultur' may be interpreted as a Freudian confession of a sense of lack, the inability to achieve that delicate appreciation of the values of life that is characteristic of the French, or the well-poised directive capacity and clean-cut analysis of the English mind. The compensation is the gigantic and immodest delusion of superiority." Equal regret will perhaps be felt that the now fairly numerous experimental investigations of belief as manifested in testimony find no consideration here, though these too, one would have thought, might have contributed much of value to the subject.

Though, consequently, in some respects deficient in scope and depth of treatment, the book is nevertheless encouraging in so far as it indicates that psychology may ultimately be of real use and interest in connection with a number of sociological problems of the utmost importance.

Studies in Psychology. By Colleagues and Former Students of Edward Bradford Titchener. Worcester, Mass.: Louis N. Wilson. 1917. Pp. 337.

This volume is dedicated to Professor Titchener by his colleagues and former students in commemoration of the completion of twenty-five years of distinguished service to psychology at Cornell University. It contains sixteen papers, representing many different aspects of psychological research.

They are prefaced by a letter from E. C. Sanford, briefly describing three preliminary but suggestive inquiries of a statistical character. Three general discussions follow—by Margaret Washburn on the social psychology of man and lower animals, by Pillsbury on principles of explanation in psychology, and by C. G. Shaw on the content of religion. Two excellent and elaborate researches are reported in detail by J. W. Baird and R. M. Ogden on the memory for absolute pitch and on the consciousness of meaning respectively. Another investigation upon meaning, carried out by means of the reaction-method, is described by H. P. Weld. An equally thorough investigation is contributed by J. N. Curtis upon tactual discrimination and susceptibility to the Müller-Lyer illusion, carried out upon normal, supernormal and abnormal individuals. The shorter papers include three upon special problems in the psychology of vision; one upon the distribution of time in learning; one upon temporal judgments after sleep; and a paper—one of the shortest, but one of the most original—upon the psychology of the blindfold chess-player. H. M. Clarke contributes a short note on recognition; H. C. Stevens upon the Rossolimo tests.

The papers are admirably representative of the numerous lines of research which Professor Titchener has inspired at Cornell. A striking testimony to his multifarious and unceasing activity is afforded by the bibliography which concludes the volume, enumerating no less than one hundred and nine published books and papers.

The Law of Struggle. By HYMAN SEGAL. New York: Massada Publishing Company. 1918. Pp. 161. $1.50 c.

Experimental Education. By Dr ROBERT R. RUSK, M.A. London: Longmans, Green & Co. 1919. Pp. viii + 346. 7*s.* 6*d.*

Although in other countries attempts have been made to collect and systematise the many experiments that have been made in education within the last few decades, no attempt was made in England till Dr Rusk published about six years ago his *Introduction to Experimental Education.* Two editions of that little book have been exhausted and the present volume is a re-issue; but with so many alterations and additions that it becomes in substance as well as in title a new book. And a very useful and timely book it is. The author has used discrimination in sifting the significant from the insignificant, he brings out clearly the various questions at issue, and he differs markedly from his foreign rivals in doing fuller justice to the work done in England—work which, though small in amount, is good in quality. At the outset Dr Rusk claims for Experimental Education the position of an independent science; but since the bulk of the work described by him consists in the application of psychological principles and methods to the solution of educational problems, many will be inclined to challenge his claim. His plea is that since Experimental Education deals with data from its own special standpoint and is indeed able to dictate problems to Psychology, it is impossible to regard it as merely a branch of Applied Psychology. The author's terminology, however, whether justifiable or not, in no way detracts from the essential merit of his book. There is one point on which he is wisely emphatic: he insists on the necessity of employing the exact methods of observation and experiment used in Experimental Psychology, and of rejecting the looser and more casual methods which characterise much of the work that goes under the name of Child Study.

Conscience and Fanaticism: an Essay on Moral Values. By GEORGE PITT-RIVERS. London: William Heinemann. 1919. Pp. xvi + 112. 6*s.* net.

L'Expertise mentale militaire. By Drs A. POROT and A. HESNARD. Paris: Masson et Cie. 1918. Pp. 137. 4 fr. 40 c.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

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THE BRITISH JOURNAL OF PSYCHOLOGY

INSTINCT AND THE UNCONSCIOUS[1].

I.

BY W. H. R. RIVERS.

IT is necessary to begin by making as clear as possible the sense in which the two terms of the title of this symposium should be used. In my contribution to the Symposium of 1918 on "Why is the 'Unconscious' unconscious?" I stated the sense in which I used and shall continue to use the term 'unconscious.' I use it for experience which is not accessible to consciousness except under certain special conditions and yet is capable of influencing consciousness and conduct indirectly in various ways. It is now necessary to undertake the far more difficult task of defining 'instinct.'

At one time, not very long ago, it was sufficient to regard instinct as the mode of mental activity proper to animals in distinction from intelligence which was held to be the attribute of the human mind. Increasing knowledge, such as that set forth in the Symposium of 1909 on "Instinct and Intelligence" and that gained through all recent work on animal psychology, has shown the distinction to have little value. It is now generally recognised that the behaviour of animals shows many features, such as adaptability to unusual conditions, which can only be explained by qualities of the same order as those belonging to intelligence. The behaviour of animals does not differ from that of Man in kind, but rather in the relative degree and importance of the different reactions of which the behaviour consists.

Another widely held distinction between instinct and intelligence is that the former is innate and the latter acquired. If an animal behaves

[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

in a certain way which is quite independent of any experience it can have acquired in its individual existence, so that its nature must have been wholly determined by innate disposition, the behaviour is regarded as purely instinctive, while in other modes of behaviour regarded as partly instinctive and partly intelligent, the instinctive component is distinguished by its independence of acquired experience. This distinction, if adhered to strictly, is one of great value, but it is biological rather than psychological in nature. It is one which is very difficult to utilise scientifically, for as soon as an animal has acquired experience of any kind, it becomes a matter of the greatest difficulty to distinguish between the innate and the acquired conditions. In many cases, as that of the insect or other kind of animal acting in a highly complex way of which it cannot possibly have acquired any individual experience, the distinction is valid and useful, but in the case of Man and other higher animals, it is of little value in the attempt to distinguish between the different modes of mental activity.

A third mode of distinguishing between instinct and intelligence is psychologically even less valid than the last. In the higher animals, *i.e.* in those which have developed a cerebral cortex or neo-pallium as part of their central nervous system, instinct is regarded as the product of subcortical activity, while intelligence is held to depend on the activity of the cortex or neo-pallium. It is instructive to observe that in the last resort even so psychological a writer as Lloyd Morgan is continually driven to employ this difference in his work on instinct and intelligence[1], thus virtually giving up the attempt to make any psychological distinction between the two.

To the philosopher and psychologist, such means of distinguishing instinct from intelligence as those which have been mentioned can hardly be satisfactory. The psychologist especially is compelled by the canon of his study to look for some distinction capable of expression in psychological terms, for something by which the two kinds of behaviour can be distinguished quite apart from their exhibition by animal or Man, from their dependence on, or independence of, acquired experience, and from the part of the nervous system with which they are associated. The psychologist requires something in the nature of the behaviour itself by means of which he can distinguish the instinctive from the intelligent.

In seeking such a distinction it will be well to turn away from the behaviour of insects and other invertebrata which are usually taken as the model for the instinctive. These animals differ so enormously from

[1] *Instinct and Experience*, London, 1912, pp. 93, 104, 105, 107, 110, 112. etc.

ourselves that it is too great an adventure into the unknown to base any distinction on the differences between their behaviour and that of Man. Let us rather look to the behaviour of Man as compared with the animals to which he is more closely related, and that of adult Man as compared with the infant, for our clue to the nature of the difference between intelligence and instinct. I have space here only to consider one such difference. An animal or child exposed to danger, which is so recognised as danger that it produces a reaction, gives itself to the reaction fully. If it runs away, it runs with every particle of the energy it is capable of putting forth; if it screams, it does so with all the vigour at its command. Its reaction by flight or scream is the same whether the danger be small or great. There is no discrimination of the degree of danger, and consequently there is a complete absence of the graduation of the reaction to the nature of the stimulus which is characteristic of the animal in its more intelligent behaviour and of adult Man even when danger threatens. If the danger is sufficiently great, or if certain lines of reaction which are of relatively late development are frustrated, even the adult man will fail to graduate his reactions and will devote every atom of his available energy to flight or other form of primitive or instinctive behaviour. Thus, if he becomes angry and assumes an aggressive attitude, his anger and aggression will go far beyond those called for by the needs of the stimulus. It will be noted here that I accept the position, now very generally adopted, that such emotions as fear and anger, with the special forms of behaviour they accompany, belong to the domain of instinct.

Such instinctive reactions as those I have described are characterized by the absence of graduation of the reaction according to the nature of the stimulus. If they take place at all, they take place with their full strength. This form of reaction is one which is known in physiology, where it is characteristic, for example, of the behaviour of the individual nerve fibre, as the 'all-or-none reaction[1]. I propose to adopt this term for the corresponding kind of reaction in response to such conditions as danger. I suggest that this 'all-or-none' reaction is one of the properties of instinct.

It is noteworthy that the work of Head and his colleagues[2] has shown that this 'all-or-none' reaction characterizes the activity of the protopathic constituents of the afferent nervous system and of the thalamic

[1] E. D. Adrian, *Journ. of Physiol.* 1912, XLV. 389; *ibid.* 1914, XLVII. 460; *Brain*, 1918, XLI. 26.

[2] W. H. R. Rivers and Henry Head, *Brain*, 1908, XXXI. 428; Henry Head, *ibid.* 1918, XLI. 67–69, 79.

region of the brain. If, as we suppose, this activity depends on the release of an ancient mode of reaction from an influence by which in later development it has come to be controlled, we have in this work a case in which an experimental procedure in Man has brought to light an instinctive mode of reaction, which has been superseded, at any rate in part, by a later mechanism for discrimination and graduation of response.

According to the view here put forward the protopathic constituent of the nervous system shows characters which only appear in the behaviour of the normal man or animal when exposed to conditions which bring instinctive reactions into activity, in the cases I have taken for my examples, conditions of danger which arouse the fundamental instinct of self-preservation.

The work of Head has also shown that the sense in which it is here proposed to use the term 'instinct' corresponds in essence with that of such a writer as Lloyd Morgan. Head, especially in his work with Gordon Holmes[1], has shown that the earlier and cruder forms of central sensory function are associated with the activity of the optic thalamus, and especially of its essential nucleus. The view here put forward therefore supports the position held by Lloyd Morgan that instinct is a function of the sub-cortical centres.

I have only dealt with one psychological characteristic of instinct, viz., that it is subject to the 'all-or-none' principle. It must suffice here to suggest that the work of Head gives the clue to other psychological properties of instinct, such as the crudeness, the vagueness of spatial reference, and the immediacy and uncontrolled character of the response, which are shown by the protopathic form of cutaneous sensibility.

The way in which it is here proposed to speak of instinct differs so widely from that in current use that the question must be raised whether it would not be wise to give up the use of this term in scientific literature and, in place of 'instinct' and 'intelligence,' to adopt Head's terms 'protopathic' and 'epicritic.' These words, used originally to indicate the primary nature of one kind of sensory process and the later and discriminative character of the other, are equally appropriate to indicate the distinction between the earlier and later forms of the more general reaction.

I can hardly leave this attempt to define the nature of instinct psychologically without a reference to the reactions of insects from which our notions of instinct are so largely drawn. It is quite clear that the

[1] *Brain*, 1911–12, XXXIV, 102.

'all-or-none' principle does not hold good of the activity of the bee when constructing the cells of the honey-comb nor of the cruder art of such an animal as the grub of the Capricorn[1] (*Cerambyx miles*) in its elaborate preparations of conditions which will allow the developed beetle to emerge from its larval home.

The 'all-or-none' principle seems to hold good of many reactions of the insect, but it is evident that some other principle must also be in action, giving to the behaviour of the insect the power of discrimination and graduation of response. The lines on which development has taken place are probably very different from those which have been followed in the case of the vertebrata. The vast difference between the central nervous system of an insect and that of a vertebrate animal would lead us to expect a corresponding difference in the nature of the controlling and graduating mechanisms of the two kinds of animal. If the view here put forward seems worthy of adoption as a working hypothesis, it would become the duty of the student of insect behaviour to discover in what respects the controlling and graduating processes of the insect differ from those of Man or other higher vertebrate.

Having now made clear the sense in which I propose to use the term 'instinct,' I can proceed to state the main thesis of this opening paper of our Symposium. This thesis is that the early forms of 'all-or-none' reaction, together with the experience associated with them, are incompatible with the graduated reactions which develop later, and are dealt with by utilising a process which, as I put forward in the Symposium of last year, has always been closely associated with instinct. I refer to the process of suppression or dissociation by which the disturbing experience is not abolished, but becomes separated from the mass of conscious experience which is readily capable of recall. The view I put forward is that this suppressed or dissociated experience makes up 'the unconscious' in the sense which I have given to the term at the beginning of this paper.

I must be content here with this statement of my position, the full development of which will be undertaken elsewhere. It remains to point out that the view I have put forward implies a radical difference between the two kinds of reaction with which I deal. Instinctive reactions with their associated experience are thrust into the unconscious because they differ so greatly in nature from those developed later that the two are incompatible with one another and can only be dealt with by the drastic method of suppression. If we consider the development of mind in the

[1] J. H. Fabre, *The Wonders of Instinct*, London, 1918, p. 49.

light of the view here put forward, there is suggested a history in which mental development proceeded for a time along the path of the 'all-or-none' reaction. When this path had led the animal kingdom a certain distance in the line of progress, a new development began on different lines. The view I put forward is that Nature did not proceed simply by modifying the earlier process, by graduating its reactions to the needs which the animal had to meet, but started on a new path, developing a new mechanism which utilised such portions of the old as suited its purpose and treated the rest by the process of suppression. In this process those parts of the older form of reaction which were useless or noxious were shut off or dissociated from the newly developed forms of conscious reaction, only to emerge in sleep or in such states as hypnotism in which the later-developed and controlling influences are in abeyance, or have been replaced by influences of another kind. The suppression, however, is rarely complete and is always liable to break down under excessive shocks or strains. The fears or phobias and many other, more or less morbid, features of the mental life of Man are the expression in indirect and veiled form of this incompleteness of suppression, while the functional nervous disorders and insanities which occur under the stress of adverse circumstances are due to the weakening of the controlling forces and consequent emergence into activity of instinctive reactions which in health are suppressed. According to the thesis of this paper, the mental life of man is a life-long struggle between two widely differing and often incompatible modes of mental activity in which, so long as health is maintained, victory lies with one of the two opposing forces, but the vanquished force is ever there ready to reassert itself whenever the control of the victor is weakened or removed.

POSTSCRIPT[1].

In this opening paper of the Symposium, written six months ago, I have regarded the 'all-or-none' principle and the absence of graduation as the distinguishing marks of instinct. This conclusion was derived from the study of the instincts of Man, and especially from those varieties of instinctive behaviour which are associated with such primary emotions as fear and anger. A wider study of the subject since the paper was written has led me to take up a somewhat different position. I am now inclined to continue the use of the word 'instinct' as a term for innate

[1] This postscript had not been added when the preceding paper was submitted to the other contributors to the Symposium.

mental process, and to distinguish different varieties of instinct according as they are or are not subject to the 'all-or-none' principle. If these two main varieties of instinct are denoted by means of the words devised by Head, the instincts connected with the needs of the individual will be in the main of the protopathic kind while those subserving the welfare of the group will be in the main of the epicritic kind. I now believe that the instincts which I had especially in mind when the paper was written are characterized by the 'all-or-none' principle because they came into being to meet circumstances of a primitive and urgent kind which made immediacy and fulness of response more necessary than delicacy of adjustment of means to ends. I suppose that with the gregarious mode of life there came the need for adjustment of the needs of the individual to those of the group, and that this took a large, if not predominant, part in bringing about discrimination of the conditions which call for a response and a corresponding graduation of the response itself.

INSTINCT AND THE UNCONSCIOUS[1].

II.

By CHARLES S. MYERS.

An important initial aim of Dr Rivers's paper is to find a satisfactory psychological definition of instinct, and he bases it on the 'all-or-none' principle. To me this result is of special interest, as it recalls an earlier Symposium, on the nature of sensation intensities, in my contribution to which[2] I endeavoured, mainly on the results of Sherrington's researches, to apply the same principle to a differentiation of various classes of sensation and of reflex action. On that occasion I pointed out that three classes of reflex action are easily recognisable. There is (i) the simple 'all-or-none' reaction, illustrated by the extensor thrust reflex which is obtainable by pressing the skin beneath the pads of a dog's hind foot and is practically unaltered by varying, within certain wide limits, the strength of the ingoing stimulus. The same 'all-or-none' principle has been found in the striated muscle of the frog, and there can be little doubt that it holds for medullated nerve fibres also. "Their increase in *intensity* of function seems to depend on a greater *quantity* of elements (muscle fibres or nerve fibres) taking part in the action....Each element follows the 'all-or-none' principle[3]," although different elements may be differently sensitive, needing a weaker or stronger stimulus to excite them. In this sense only is any grading possible. If, therefore, we can imagine groups of such 'all-or-none'-reacting reflex elements, of varying sensitivity, gathered together, we reach the second type of reflex action, (ii) the 'graded monophasic' reaction, which I illustrated by the scratch reflex, where the strength of the reflex movements can be graded in amplitude, force and frequency, according to the intensity of the stimulus. This I distinguished from (iii) the 'graded diphasic' reaction, where two simple diametrically opposite reflexes (*e.g.* those of flexion and extension of a joint) are so integrated that the activity of the pair is balanced, as it were, on a knife edge, in a state of continuous tone or (else) posture,—"a condition which may be described as a state of active equilibrium of the double reflex." From this state the activity of the reflex "may be made to swing in one or other of two opposite directions as between two poles," and a condition of temporary equilibrium or

[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

[2] This *Journal*, 1913, VI. 137–154.

[3] *Ibid.* 143.

adaptation may be set up within wide limits of such swing. The "afferent impulses which cause (or are set up by) reflex *contraction* of a group of muscles governed by one centre of the double reflex simultaneously cause *inhibition* of contraction in the antagonistic group of muscles governed by the other centre of the same reflex. It is the special rôle of such reciprocal graded inhibition to procure an exact adjustment between the strengths of incoming stimuli and outgoing discharge[1]."

The 'all-or-none' and 'graded monophasic' reflexes are characteristic of the spinal cord; each reflex element is easily fatigable and shows a well-marked 'refractory period,' during which the movements produced by it are inexcitable. The full-blown 'graded diphasic' reflex, on the other hand, occurs in the decerebrate animal, in which the spinal cord retains its connexion with the bulb, pons and mid-brain; it is devoid of refractory period: its activity is continuous and, within certain limits, it is indefatigable. The outgoing impulses, instead of being periodically cut short, outlast the stimulus, so as to produce continuous, instead of intermittent, reflex muscular contraction. Moreover, prolonged excitation of one member of the double reflex, accompanied by prolonged inhibition of the other member, tends to give way to a reversal of these processes: the balance, when disturbed, swings sooner or later in the opposite direction.

It is not difficult to imagine that far more complex groupings of reflexes occur in higher regions of the central nervous system, giving rise, for example, to what may be described as (iv) the 'graded polyphasic' reaction, where one complex reflex action may be followed by one or other of a variety of complex, not necessarily wholly antagonistic, reactions, according to the condition of the centres concerned.

My object in such classification of the reflexes was to point out (*a*) that the 'heat and cold spot' systems of cutaneous sensibility bear a striking analogy to the first class of reflex actions, in respect of the 'all-or-none' principle, the absence of adaptation, the easy fatigability of the spots and the different degrees of sensitivity of different spots; (*b*) that in the 'warmth and cool' system of cutaneous sensibility (and in the paired 'blue and yellow,' 'red and green' systems of retinal sensibility) we can find an equally striking analogy with the third class of reflex actions, in which gradation, adaptation and (in vision) reversal are such characteristic features; (*c*) that auditory sensations—where neither the fatigability and 'all-or-none' principle of (*a*) occur, nor the bi-polarity and adaptability of (*b*),—may be likened to the second class

[1] This *Journal*, 1913, VI. 146–7.

of reflexes. And it seems possible that the peculiar contrast relations and possible fusions between the taste sensations of sweet, sour and salt[1], may prove analogous to the behaviour of the fourth class of reflex actions which I have sketched in this paper.

But Dr Rivers has now enlarged this series of attempted analogies; he has ingeniously extended it to instincts. He considers that in instincts may be recognised the 'all-or-none' principle, "the crudeness, the vagueness of spatial reference, and the immediacy and uncontrolled character of the response, which are shown by the protopathic form of cutaneous sensibility."

I propose to try to cast the net even wider. Can we usefully bring still other modes of mental activity into analogy with the various reflex and sensory systems already classified? Let us turn first to pleasure and displeasure. There can be no doubt that they are finely graded, and are hence not analogous to the first simple 'all-or-none' class. There can equally be no doubt that pleasure and displeasure must be grouped as antagonistic, incompatible, incopresentable members of a pair, that they are set as on a swinging balance, and that they are readily susceptible to equilibrium and to adaptation. Their likeness to the third or 'graded diphasic' class is therefore unmistakeable.

Now we have a considerable body of evidence that pleasure and displeasure are intimately dependent on the activity of the optic thalami. But the striking characteristic of thalamic activity is its 'protopathy.' When through injury or disease cortical control is removed, 'thalamic' affective and sensory experience becomes grossly exaggerated, diffuse, practically gradeless, with little reference to subject or object. We conclude, then, that although in the intact human organism pleasure and displeasure belong to the 'graded diphasic' class, they are nevertheless built up from the more lowly 'all-or-none' class[2].

But the optic thalami are concerned not only with protopathic sensibility and affectivity but also with the emotional experiences which Dr Rivers agrees to include in the term instinct. If, as has been just pointed out, sensory and affective experiences become graded owing to cortical control, there seems *a priori* no reason why (at least some) emotional experiences should not acquire similar gradation, and yet retain their specific character. Moreover if, as has been previously maintained, we can distinguish several classes or levels of reflex actions, there

[1] Cf. my *Text-book of Experimental Psychology*, Cambridge, 1911, I. 103, 105–6.

[2] I suggested a similar complex origin of colour sensations in the Symposium on sensation intensities (*loc. cit.*).

seems *a priori* no reason why different instinctive (including emotional) experiences should not belong to different classes or levels, some obeying the 'all-or-none' principle, others being graded.

Certain emotions, sentiments and attitudes are clearly on the same plane as the graded affections of pleasure and displeasure,—for example the antagonistic pairs of self-assertion and self-abasement, self-confidence and self-distrust, certainty and doubt, familiarity and strangeness. But Dr Rivers would probably exclude these experiences from his definition of instinct. He accepts "the position...that such emotions as fear and anger...belong to the domain of instinct"; these, however, are the only emotions which he expressly cites as characterized by complete absence of gradation.

Now it is quite obvious that fear, anger and, I would add, sexual and maternal love stand on a different plane from pleasure and displeasure and from the other pairs of antagonistic experiences just mentioned. They are far more potent and passionate, they obtain far stronger hold over the individual, they are of rarer occurrence, and they are of greater importance for the preservation of the species. They cannot be grouped in pairs of incompatible, incopresentable members; there is no single emotion exactly antagonistic to fear, anger or love; indeed in their later development, organization and combination, a variety of mental states, not necessarily antagonistic, may ensue from any one of them. They thus come to resemble the fourth or 'graded polyphasic' class of reaction.

I find it difficult to accept Dr Rivers's contention that in children and animals fear and anger obey the 'all-or-none' principle. Surely children and animals may experience mere timidity and annoyance, on the one hand; or they may suffer uncontrollable terror and anger, on the other; or any grade may occur between these extremes. Nevertheless, within certain limits I believe Dr Rivers's view to be practically correct, for it seems to me that *at their first appearance* fear and anger cannot but *tend* to follow the 'all-or-none' principle. This also occurs in the case of adolescent 'falling in love' or 'becoming religious.' At their first entry such experiences must *tend* to react 'for all they are worth.' If one could assume that the experiences were *entirely* new, I would omit the words 'tend to.' But although no experience can ever be entirely new, so close is the approximation in these cases that the two words have hardly more than a theoretical value. Still, with the further growth and repetition of such instincts, with the increasing prominence of intellectual factors, and with the developing powers of comparison and self-control, the 'all-or-none' principle quickly gives way, these instincts become graded, and analogy to the first class of reflex actions is replaced by analogy rather to the fourth.

But, we may ask, does not every experience obey the 'all-or-none' principle on the occasion of its first occurrence? Does not the first pleasure or the first sensation of sound obey the same principle? No doubt the impossibility of determining the moments (if they exist) when such experiences *first* occur, must rob our answer of any practical importance. Yet it can hardly be ruled out of theoretical consideration, if my agreement with Dr Rivers's definition of instincts is limited practically to the first occasions of their appearance. Indeed I am disposed to think that Dr Rivers is fundamentally in agreement with me, for he writes: "as soon as an animal has acquired experience of any kind, it becomes a matter of the greatest difficulty to distinguish between the innate and the acquired conditions."

In a previous Symposium on instinct and intelligence, I gave reasons[1] for regarding their separation as a convenient but purely artificial act of 'abstraction.' I maintained that in the intact organism "there is not one nervous apparatus for instinct and another for intelligence[2]." For me, instinct wholly severed from intelligence is indistinguishable from reflex activity. Let me endeavour to justify this psychological standpoint on the grounds of further physiological parallelism.

The optic thalami, the probable seat of 'abstract' instinctive experience, never function in the intact 'individual' without some measure of cortical control. The cerebral cortex and the thalami have evolved from a common origin,—the prosencephalon, which contains within it the rudiments of later cortical and thalamic structure[3]. Only from injury or disease can the thalami act independently of the cortex. Now just as the prosencephalon has become differentiated into the optic thalami and cerebral cortex, so, I suggest, 'abstract' instinct and intelligence have evolved from a common origin, neither having a separate existence in the intact organism. Herein, I think, lies the *crux* of the difference between Dr Rivers's views and mine. He regards instinct as having "led the animal kingdom a certain distance in the line of progress," whereupon "a new development began on different lines,"—a *new path* being

[1] This *Journal*, 1910, III. 209–218, 267–270. [2] *Ibid.* 267.

[3] Since this has been printed, I have come (mainly from correspondence with Prof. Elliot Smith) to realise that it has embryological rather than functional significance. But the general argument is unaffected: in lower vertebrates some common 'organ' is doubtless mainly responsible both for 'intelligent' and 'instinctive' activity, and these functions in the highest vertebrates have become more fully differentiated and transferred to two different 'organs,' the cerebral cortex and the optic thalami.

started, "which utilised such portions of the old as suited its purpose...." In other words, he regards intelligence as something later added to instinct, while I regard both as differentiated out of a *common origin.* His is broadly a synthetic, mine an analytic concept of mental evolution.

I am prepared to go even further. I doubt whether not only 'abstract' instinct, but also the pure 'all-or-none' principle, by which Dr Rivers seeks to define it, exists in the intact human organism. For it is only in the 'spinal' animal (*i.e.* when the cord has been severed from the brain) that the 'all-or-none' principle of the extensor thrust reaction has been purely studied in the dog. It is only under abnormal 'clinical' conditions that heat and cold spots have been completely studied and the 'all-or-none' principle of their reaction fully and cleanly laid bare; and even in these circumstances the 'all-or-none' principle is only approximately obeyed. In the (intact) lower animals we have no evidence that this principle exists to the complete exclusion of graded forms of sensibility[1].

Dr Rivers is quite ready to admit that the 'all-or-none' principle does not hold for many instances of instinctive behaviour among insects. He attributes it to some unknown controlling process, distinct from intelligence. But despite the profound differences in the structure of the invertebrate and vertebrate nervous systems to which he invites our attention, I prefer to subscribe to an earlier sentence in Dr Rivers's paper,—"The behaviour of animals does not differ from that of Man in kind, but rather in the relative degree and importance of the different kinds of reaction of which the behaviour consists."

I have little space to consider whether it is correct to state that instinctive reactions are thrust into the unconscious because, being 'protopathic' in character, they are incompatible with maturer 'epicritic' experience. I do not think we have adequate evidence that in the intact organism the protopathic (p) and epicritic (e) systems of cutaneous sensibility have undergone 'dissociation,' in the sense in which the term is here used as implying the more or less independent, subconscious survival of a suppressed, non-utilised portion of experience. We find merely a composite unity $\overline{p + e}$, the constituents of which, as I have just pointed out, are only clearly differentiated and separable after neural lesion. So, I maintain, 'abstract' instinct (*ins.*) and intelligence (*int.*), though neurally differentiated, are inseparable in the intact organism; they occur as a composite unity $\overline{ins. + int.}$ It seems to me that the protopathic element is 'fused' rather than 'dissociated.'

[1] I am confident that protopathic sensibility will not be found in a pure state on the normal human nipple and glans penis where typical epicritic sensibility is absent.

Moreover, I cannot agree that what is dissociated in any conflict between instinct and intelligence consists merely in the protopathic characters of the former, and that it is these characters that "emerge in sleep or in such states as hypnotism" or after excessive shocks and strains in which the control of intelligence is supposed to be in abeyance. Loss of control is not, I think, to be confused with protopathic reaction. The dreaming or hypnotised person exhibits graded instincts.

Whilst I agree fully with Dr Rivers that dissociation and repression occur through incompatibility and incopresentability, I would point out that the former are not the only solution for the latter. When *p* and *e*, or *ins.* and *int.* are opposed, fusion and integration, instead of dissociation and repression, may well arise from their compresence. Repression *may*, of course, occur when instinct meets intelligence, but it is not confined to such conflict. It may also occur when a lower form of instinct meets an uncompromising, incompatible, higher form, when one affective experience meets another diametrically opposite to it, or when two uncompromising, incompatible, cognitive or intellectual experiences strive independently to coexist. What is then thrust into the unconscious is not merely or necessarily the protopathic constituents, but the entire experience (cognitive, conative and affective, intellectual, emotional and 'instinctive') which is involved in the incompatible conduct. While I am writing this article, a clock strikes or some one asks me a question. I am 'not at home' to these experiences. They are temporarily inhibited or repressed, but a little later I may become aware of them[1]. Or I may forget an experience because it is unpleasant. Yet what is dissociated, what returns when control is in abeyance, is in neither case an instinct, nor is it characterized by protopathic features.

What returns from the unconscious has often suffered strange metamorphosis. I have already written at too great length to discuss the causes of incomplete repression, or the purposes of the sublimation, condensation, symbolism, etc., which dissociated experiences undergo in the unconscious. But it is clear that out of the unconscious there emerge not merely the more or less imperfectly repressed activities which have been dismissed thither through dissociation from consciousness, but also fresh activities, intellectual as well as instinctive. In the unconscious germinate (perhaps in accord with Mendelian conceptions) new instincts for the species and the creative flights of individual genius, which, when the time is ripe, sprout forth into consciousness for its use.

[1] I am, of course, fully aware of the differences in fate that may attend an inhibited cognitive and a dissociated emotional experience.

INSTINCT AND THE UNCONSCIOUS[1].

III.

By C. G. JUNG[2].

THE subject of this Symposium concerns a problem which is important from the biological as well as from the psychological and philosophical standpoints. If we are to discuss the connexion between instinct and the unconscious, it is indispensable to use clearly defined terms. Unfortunately the terms 'instinct' and 'unconscious' belong to those more or less general conceptions, which play an important rôle in different spheres of scientific thought. While the conception of the unconscious chiefly concerns the widely separated faculties of medicine and philosophy, the conception of instinct is of equal importance in the spheres of biological, medical, psychological, philosophical and sociological thinking. On account of the wide use of the conception of instinct it is easily intelligible that it has undergone many different interpretations.

As regards the definition of instinct, I recognise that it is characterized by the 'all-or-none' reaction, as maintained by Dr Rivers, and it seems to me that this peculiarity of instinctive activity must be of great importance for the psychological side of the problem. Naturally I must confine myself to the psychological aspect of the problem, because I do not feel competent to treat the question of instinct from its biological aspect. But when I attempt to define psychologically the nature of instinctive action, I find it impossible merely to rely on Dr Rivers's criterion of the 'all-or-none' reaction for the following reason: Dr Rivers defines the 'all-or-none' reaction as a reaction that is without any gradation of intensity in respect of the circumstances which call it forth. It is a reaction that takes place with its own intensity under all conditions and without any proportion to the intensity or nature of the stimulus. But when we examine conscious processes to see whether there are any which likewise manifest an intensity which is inappropriate to the intensity of the stimulus, we easily find a great number of them

[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

[2] The second contribution, by Dr C. S. Myers, to this Symposium had not reached me when I wrote mine.

in everybody: as for instance disproportionate emotions and impressions, exaggerated planning and acting, and so on. It seems to be impossible to attribute all these processes to instinct. Therefore it seems necessary to use some other criterion for our psychological definition.

Colloquial language frequently makes use of the word 'instinct.' We speak of 'instinctive' actions whenever we consider an action the motive and aim of which are not fully conscious, and which has been excited by a more or less dim internal necessity. This peculiarity of instinctive activity was pointed out by Reid, who says, "By instinct, I mean a natural impulse to certain actions, without having any end in view, without deliberation and without any conception of what we do[1]." Thus instinctive action is characterized by a certain unconsciousness of the psychological motive behind it, in opposition to the strictly conscious processes which are distinguished by the conscious continuity of their motives. Instinctive activity appears to be a more or less abrupt psychic experience, a sort of intrusion into the continuity of conscious events. On account of this fact we feel the instinct as an internal necessity. In the same way Kant defined instinct as an "internal necessity[2]." On account of these qualities instinctive activity must be attributed to the realm of unconscious processes, which are accessible to conscious apperception through their results only. But were we to be content with such a definition of instinct, we should soon discover its insufficiency. When we came to examine the totality of unconscious processes, we should find it impossible to conceive of them all as instincts, although colloquial language does not differentiate them. If you suddenly meet a poisonous snake and are extremely frightened, then you rightly designate such an impulse instinctive, because there is no difference between your impulse and the instinctive fear of snakes in monkeys. It is just the uniformity and regularity of the phenomenon, which is the most characteristic peculiarity of instinctive action. As Lloyd Morgan justly remarks, it would be as uninteresting to bet on the occurrence of an instinctive reaction as on the rising of the sun to-morrow.

But on the other hand, we must not forget that psychological phenomena are not infrequently met with which are very similar to instinctive activities, although not at all to be understood as such. It may happen that some one is regularly seized with a terrible fright when meeting a harmless hen. Although the mechanism of fright in this case is an unconscious impulse similar to an instinctive impulse, we still have

[1] *On the Active Powers of the Mind*, III. 2.
[2] *Anthropologie*, I. § 78.

to draw a sharp line between the two kinds of impulse for scientific reasons. In the former case the fear of snakes is a teleological instinct of general occurrence. But the latter case is, when habitual, a 'phobia' and not an instinct, on account of its purely individual occurrence. Whereas instinct is an inherited quality, a phobia is an individual acquisition, even if it may also be the outcome of inherited disposition. There are innumerable similar unconscious necessities, for instance, thought obsessions, musical obsessions, sudden moods and phantasies, compulsive emotions and tendencies, depression and feelings of anxiety. These symptoms are met not only in abnormal persons, but also in so-called normal individuals. In so far as these phenomena occur individually only, and in so far as they are neither uniform nor regular, they must be distinguished from instinctive processes, despite the fact that their psychological mechanism is almost identical with that of instinct. They share the character of the 'all-or-none' reaction, a fact easy to observe in pathological cases. There are many such cases, where a stimulus excites a wholly disproportionate reaction, comparable to a true instinct.

But all such reactions have to be clearly distinguished from instinct. Only those processes can be called instinctive which are inherited and unconscious, uniformly and regularly occurring everywhere. At the same time, they must show the mark of compelling necessity, comparable to the character of a reflex. Instinctive activity is fundamentally distinguished from a mere sensori-motor reflex by its complicated nature only. William James calls an instinct "a mere excito-motor impulse, due to the pre-existence of a certain 'reflex-arc' in the nerve-centres[1]." Instincts have their unconscious motives, their uniformity and regularity in common with the reflexes.

The question of the origin or primordial acquisition of instinct is a most intricate one. The statement that instincts are always inherited does not explain their origin. It only puts back the problem to our ancestors. The view is widely held that instincts originated from individual and general volitional actions frequently repeated, and that they thus became automatic. This explanation is plausible only in so far as we are able to recognise how certain activities laboriously acquired have gradually become automatic through frequent practice. But if we consider some of the most marvellous instincts in certain animals, we must confess that we can hardly imagine how such instincts could have been acquired by trying, learning and repeating. There are cases where

[1] *Principles of Psychology*, II. 391.

it is almost inconceivable how learning and practice could ever have taken place. Let us take as an example the instinct of propagation in *Pronuba yuccasella*, the yucca-moth. Each flower of the *Yucca* plant opens for one night only. The moth fetches the pollen from one flower, and kneads it into a little clod. Then the moth carries the pollen to another flower, where it cuts open the pistil, lays its eggs between the ovules of the plant and then climbing to the top of the pistil, stuffs the little clod of pollen into the funnel-shaped opening of the pistil. Only once in its life does the moth carry out this complicated action. If the ovary of the plant were not fertilised at the same time, the young insects could not develop; nor would the plant itself get fertilised without the help of the moth. In all such cases a possible explanation seems to be, that a moth once discovered the way by mere accident, and that its descendants became influenced during their embryonic state by the special conditions of their surroundings, so that some of them, at least, acquired an instinct to repeat their mother's accidental experience.

But such an explanation is far from being satisfactory. Bergson's philosophy suggests another way of explanation, where the factor of 'intuition' comes in. Intuition, as a psychological function, is also an unconscious process. Just as instinct is the intrusion of an unconsciously motivated impulse into conscious action, so intuition is the intrusion of an unconscious content of an 'image' into conscious apperception. Intuition is a process of unconscious perception, either of subjective unconscious contents, or of objective but subliminal facts. Thus colloquial language speaks of intuition as instinctive apprehension (*Erfassung*). The mechanism of intuition is analogous to that of instinct, with this difference that whereas instinct means a teleological impulse towards a highly complicated action, intuition means an unconscious teleological apprehension of a highly complicated situation. In a way intuition is a counterpart of instinct, not more and not less incomprehensible and astounding than instinct itself. But we must never forget that things we call complicated or even miraculous are only so for our human mind, whereas for nature they are just simple and by no means miraculous. We always have a tendency to project into things the difficulties of our understanding and to call them complicated, while they are very simple in reality and do not partake of our intellectual difficulties. Intellect is not always an apt instrument; it is only one of several faculties of the human mind.

A discussion of the instinct problem without considering the conception of the unconscious would be incomplete, because instinctive reaction cannot be conceived unless it is understood as a psychic but unconscious process. Thus the conception of the unconscious becomes an integral part of the instinct problem.

I define the unconscious as the totality of all psychic phenomena that lack the quality of consciousness. Instead of being called unconscious those phenomena may equally well be called 'subliminal.' The term 'subliminal' presupposes the hypothesis that each psychic content must possess a certain energic value in order that it may become conscious. In proportion as the energic value of a conscious content decreases, the more easily the content disappears below the threshold of consciousness. From this it follows that the unconscious is the receptacle of all lost memories and of all contents as yet too feeble to become conscious. New products originate from the association and combination of unconscious contents; dreams are the commonest instances of such products. In addition to the lost memories, and the combinations not yet conscious, intentional repressions of painful and incompatible thoughts and feelings form an important part of the unconscious. I designate the totality of the contents just mentioned as the 'personal unconscious.' It contains the acquisitions of the individual life, in opposition to another stratum or form of the unconscious, containing the 'supra-individual' qualities which were not acquired but inherited, as for instance instincts, *i.e.* impulses to actions without conscious motivation. Moreover in this stratum we discover the pre-existent forms of apprehension, or the congenital conditions of intuition, viz., the 'archetypes' of apperception, which are the *a priori* determining constituents of all experience. Just as instincts compel man to a conduct of life which is specifically human, so the archetypes or categories *a priori* coerce intuition and apprehension to forms specifically human. I propose to designate the sum of such inherited psychic qualities as instincts and archetypes of apprehension by the words the 'collective unconscious.' I call it 'collective' because it does not possess individual contents of sporadic occurrence, but qualities of uniform and general occurrence. Clearly instinctive activity is an essentially collective phenomenon of uniform and regular occurrence and has nothing to do with the individual qualities of man. The archetypes of apprehension have the same uniformity and universality as instincts, and therefore equally deserve denomination as collective phenomena.

I am convinced that from the psychological standpoint the instinct problem cannot be treated without a consideration of the archetype problem, because at bottom it is the same problem. But I find it somewhat difficult to discuss these questions as opinions about the rôle of instinct in human psychology differ widely. Thus, William James held the view that man is filled with instincts, while others have restricted the number of instincts to a very few processes only slightly different from reflexes, namely to certain more or less complicated movements, *e.g.* of suckling, certain particular motor reactions of the arms, legs and larynx, the use of the right hand, and the formation of vocal sounds. In my opinion such restriction goes too far, but it is in itself quite characteristic of human psychology. Above all we should never forget that, when discussing human instincts, we are speaking of ourselves and we therefore are doubtless prejudiced.

We are far more capable of observing and judging of instincts in animals or in primitive men than in ourselves. This is due to the fact that we are accustomed to criticize our own actions, to discriminate their presumable motives and to seek rational explanations for them. But it is not yet sufficiently proven and it is even quite unlikely that our rational explanations can stand the test. A superhuman intellect is not needed in order to see through the shallowness of certain of our arguments and to recognise the true motives, namely natural instincts, behind our rationalistic constructions. It is on account of our rational arguments and our artificial reasoning that it looks to us as if we were not actuated by instinct, but exclusively by conscious and rational motivation. Of course I do not mean to say that by careful training man has not sometimes succeeded in transforming instincts into volitional actions. There is no doubt that the instincts of civilised man have become considerably modified; but underneath, instinct remains as the motive nucleus. We have doubtless succeeded in wrapping up a great number of instincts in rational motivations and volitional purposes to such an extent that we are now unable to recognise instinct behind so many veils. Moreover, with regard to civilisation, it is perhaps not desirable to see too clearly how powerful instinct is, and how thin the stratum of civilisation is. We have reason enough to make us believe that the number and intensity of human instincts have decreased. But if we apply the criterion of the 'all-or-none' reaction to our actions, then we find numerous cases of *excessive and exaggerated* reactions. Exaggeration is a peculiarly common human manifestation, although everybody carefully tries to explain his reactions by rational motives. There is, of course, no difficulty in finding good arguments, but they never alter the fact that our reaction was exaggerated and out of proportion to the exciting cause. And why does man not do or say, give or take, just as much as is needed or reasonable or justifiable in a given situation, but so frequently much more or much less? It is because an unconscious impulse is released and carries the action sometimes far beyond the limits of logical motivation and proportion. This phenomenon is so uniform and so regular, that we can only designate it as instinct, though nobody would immediately recognise the instinctive nature of the reaction.

Thus I feel convinced that human actions are influenced by instinct to a far higher degree than is usually admitted. I also believe that the rational motivation of our actions and reactions is an explanation *a posteriori* rather than a true motivation in the sense of a rational efficient cause. Our judgment in this respect seems to be biassed by an instinctive exaggeration of the rationalistic standpoint. The instinct of rationalism serves the purpose of civilised life. It conceals the impulsive character of our actions by means of logical arguments, and, as pointed out above, it aids in this way the subjection of other instincts to the real or apparent control of consciousness.

Instincts are typical ways of action and reaction, and whenever it is a matter of uniformly and regularly repeated reactions we are witnessing instinct. It is in so far quite indifferent whether there is an association with conscious motivation or not, and it is also indifferent what the momentary individual form of the action is.

Just as it is questionable whether man possesses many instincts or only a few, so it is doubtful—and this is a problem hitherto little discussed—whether he possesses many primordial forms or archetypes of apprehension or not. We meet here with the same difficulty I have already mentioned. We are so accustomed to the use of self-evident concepts that we have become quite unaware of the extent to which these concepts are founded upon the archetypes of human apprehension. The primordial forms are concealed, like the instincts, by the extraordinary development and differentiation of our apprehension. Just as certain biological views attribute only a few instincts to man, so theories of cognition reduce the archetypes of human apprehension to relatively few and logically limited categories. Plato's philosophy, however, shows a very high valuation of the archetypes. They are held to be the metaphysical paradigms or models of the real things. The real things are nothing but imitations of the model ideas. In spite of certain variations, all immediately succeeding philosophy paid a similar regard to the archetypes. Mediaeval philosophy, beginning with St Augustine (from whom I take the term 'archetype') and ending with Malebranche and Bacon, stood on the same ground of conviction. Thus the notion of archetypes, as being natural images engraved on the mind, entered into scholasticism. These images, understood as primordial forms of judgment, were designated 'instincts[1].' But since Descartes and Malebranche this metaphysical concept of the archetype or idea has continually decreased in importance. It gave place to thought—a psychological factor, an internal condition of cognition, as was clearly formulated by Spinoza: "Per ideam intelligo mentis conceptum, quem mens format." Finally Kant reduced the archetypes to the limited number of the categories. Schopenhauer went still further in simplifying Kant's categories, but on the other hand, he returned in certain respects to the Platonic standpoint.

This sketch is unfortunately too summary, but nevertheless it may serve to indicate that same psychological development which, as pointed out above, has concealed the instincts under the cloak of rational motivations. Here it has transformed the archetypes of apprehension, over-valued before as metaphysical ideas, into logical categories. It is no longer easy to recognise the archetypes under this veil. But the way in which man conceives the world, is still, in spite of manifold variations in detail, as uniform and as regular as his instinctive actions. Just as we have been compelled to establish the concept of an instinct determining and regulating our conscious action, we must also have recourse to the correlated concept of a factor determining the uniformity and regularity of our apprehension. It is this factor which I term the archetype, the primordial image. The image might be suitably understood as intuition of the instinct in itself, analogous to the conception of consciousness as an internal image of our objective vital processes. Just as our conscious conception determines the form and purpose of our conscious action, so unconscious apprehension determines through the archetype the form and purpose of instinct. Just as we believe instinct to be thoroughly adapted and sometimes incredibly clever, so we have to assume that intuition to which instinct owes its existence, must be of extraordinary precision.

The criterion of the 'all-or-none' reaction, pointed out by Dr Rivers, has helped us to discover the activity of instinct everywhere in concrete human psychology. I hope that my concept of the primordial image may perform a similar service, when we try to discover the activity of intuition in practical human psychology. It is quite easy to discover intuitional activity in primitive peoples. There we constantly meet with typical images and motives which are the foundations of their mythologies.

[1] Herbert of Cherbury says: "Instinctus naturales sunt actus facultatum illarum a quibus communes illae notitiae circa analogiam rerum internam, cuiusmodi sunt, quae circa causam, medium et finem rerum bonarum, malum, pulchrum, gratum etc. per se etiam sine discursu conformantur."

Those images are autochthonous and of relatively great uniformity, as for instance, the idea of magic power or magic substance, of spirits and their behaviour, of demons and gods and their legends. We see the perfection of those images, and at the same time their envelopment by rational forms, in the great religions of the world. The archetypes appear even in the exact sciences where they are at the root of indispensable auxiliary concepts, as of the ether, energy and the atom. In philosophy Bergson affords an example of the revival of a very old image in his conception of the 'durée créatrice,' already met with in Proclus and, in a still more primitive form, in Heracleitus.

Modern analytical psychology has constantly to deal with disturbances of conscious apprehension due to the admixture of archetypes with rational conscious processes, and this occurs in normal persons as well as in pathological states. Just as the admixture of instinctive impulses with conscious apprehension produces exaggeration and distortion of our actions, so the admixture of primordial images with conscious apprehension produces characteristic misconceptions. Such misconceptions depend upon archaic prototypes simultaneously excited. Their admixture causes too strong or too weak, or in other ways distorted, impressions, and thus leads to the construction of erratic views. Such views again excite typical instincts because of the close association of instincts and archetypes. War psychology for instance has produced innumerable examples of archaic instincts and of archaic misconceptions.

Archetypes are typical forms of apprehension; indeed, wherever we meet with uniformly and regularly recurring ways of apprehension, they are referable to archetypes.

The collective unconscious consists of the sum of the instincts and their correlates the archetypes. Just as everybody possesses instincts, so he also possesses archetypes. The most striking evidence for the existence of archetypes is seen in mental derangements characterized by an intrusion of the 'collective unconscious' into the conscious, as occurs in all paranoias and hallucinatory psychoses. Here we can easily observe the occurrence of instinctive impulses associated with mythological images. It is impossible to say which is prior,—apprehension or impulse to action. It seems to me as if both belong to the same vital activity, which we are incapable of imagining as single and therefore dissect into two essentially distinct processes.

INSTINCT AND THE UNCONSCIOUS[1].

IV.

By GRAHAM WALLAS.

I PROPOSE to discuss Dr Rivers's statement of the "main thesis" of his paper. "This thesis," he says, "is that the early forms of 'all-or-none' reaction, together with the experience associated with them, are incompatible with the graduated reactions which develop later, and are dealt with by...the process of suppression or dissociation by which the disturbing experience is not abolished, but becomes separated from the mass of conscious experience which is readily capable of recall.... Instinctive reactions with their associated experience are thrust into the unconscious."

Dr Rivers's description of "the early forms of 'all-or-none' reaction" seems to me to be admirably clear. I am not so certain what he means by "the graduated reactions which develop later." In an earlier paragraph he calls them "intelligence," and says (in a still earlier paragraph), "Let us ... look to the behaviour ... of adult Man as compared with the infant, for our clue to the nature of the difference between intelligence and instinct."

His thesis, therefore, seems to be that the normal process by which an adult man now controls and graduates his simpler instincts is by thrusting them into the unconscious. With the thesis so stated I do not agree.

I do not, of course, deny that "suppression or dissociation" takes place. But I wish to argue that it is neither the only, nor the most effective way by which civilised man gains control over his instincts. It seems to me that the best process by which the child who "gives himself fully" to fear can make himself into a reliable soldier in modern warfare should be described rather as a "bringing into full consciousness" of the psychological phenomena of fear than as a "thrusting" of them "into the unconscious." Dr Myers, in answering a letter in which I argue this, puts my point well by saying: "There have been instances in this war where psychologically acute officers have trained their men

[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

by telling them exactly what to expect [*i.e.* what feelings to expect] in the trenches, with the result that the emotions have been 'graded' or 'under control.'"

A kindred question arises in pedagogy—Shall we, as the 'sex-educationalists' propose, tell the child before puberty "what to expect" when puberty comes, or shall we, by compulsory football, etc., attempt to thrust the sex instinct into the unconscious?

It must be remembered that, in the case of sex, and, under the conditions of modern warfare, in the case of fear, there is no hope that the instinct will not, sooner or later, enter consciousness. The question is whether it shall enter a consciousness that expects it, and can therefore recognise it and watch it critically, or not? In the case of fear I believe that under modern conditions the method of bringing it into consciousness is better than that of ignoring or suppressing it, even when suppression is accompanied by the creation (through mechanical drill) of a strong habit of obedience to orders. The modern European soldier is much less liable to panic than his ancient or mediaeval predecessors, and that fact is apparently due in large part to his custom (which reminds one of what one is told of the Freudian methods of treating acquired phobias) of discussing freely with his fellows 'cold feet,' the 'wind up' and other physiologico-psychological phenomena of fear.

There is a sense in which this bringing of instinctive phenomena into full consciousness may be said to involve a separation of them from the 'self' or 'will.' When an ancient Greek or Persian was seized with panic, the panic was his 'self.' He, in Dr Rivers's phrase, "gave himself fully" to it. When a British soldier discusses his 'cold feet,' or I myself watch my knees trembling as I stand at the edge of a precipice in the Alps, the cold feet and the trembling are a nuisance, but are not one's 'self.'

Our consciousness of, and separation of our 'self' from our instinctive feelings, and, therefore, our power of controlling our actions in their presence, may be increased in efficiency, not only by our expecting and recognising them, but also by our 'understanding' them—that is to say by our giving them a place in our general conception of cause and effect.

Dr Rivers's argument as to the 'all-or-none' nature of instinct raises the further question whether the bringing into consciousness of an instinct weakens, or intensifies, or, as he would seem to argue, leaves unchanged its actual manifestation. In the case of some of the simpler reactions I suppose that no change takes place, though I have no experimental knowledge on the point. When one's nerves of vision are tired, the resulting changes of colour sensation may be the same to the student who expects them and to the student who does not expect them. My knees may tremble in the Alps no more or less because I 'know all about it' (though in fact my impression is that they tremble less). In the case of fear, I should suppose that the 'feet' of the soldier who expects and recognises 'cold feet' are in fact less 'cold' than the feet of the soldier to whom fear comes as a surprise unrelated to the other contents of his mind.

The general question as to the relative efficiency of thrusting into the unconscious and bringing into consciousness as methods of control can be asked in approaching a large number of the more difficult problems of civilised conduct. In civilised life, for instance, men, whether they are professors, or officials, or artisans, or clergymen, are from time to time called on, owing to some new discovery or invention, to change their methods of work. Men have a natural shrinking (which, as it arises from a typical innate disposition, should, I suppose, be called an instinct[1]) against change of habit. If a man is not looking for that shrinking, and does not 'understand' it, it is, when it comes, part of himself, and therefore he has no fulcrum of resistance to it. He simply hates the faddists, or heretics, or blacklegs who are suggesting change, without understanding or controlling his hatred. In that, as in many other respects, the full use of applied science in civilised life is waiting for a general increase of psychological self-consciousness.

[1] It is a pity that one has to use the word 'instinct' (which means a particular kind of natural disposition) when one simply desires to say that some fact is part of our 'nature' and not of our 'nurture.' It would be very convenient if one could call any fact of our nature a 'nat,' whether it is the shape of our skull or our fear of snakes, and any fact of our nurture a 'nurt,' whether it is the scar of a wound or knowledge of Greek. One could then speak of physiconats and psychonats, and of physiconurts and psychonurts. One could say "shell shock should be treated rather as a psychonurt than as a physiconurt"; or that "an intense fear of snakes may either be a nat or a nurt; an intense fear of cats is almost certainly a nurt." But I suppose that we must wait till somebody invents more euphonious words than 'nat' and 'nurt.'

INSTINCT AND THE UNCONSCIOUS[1].

V.

By JAMES DREVER.

Dr Rivers begins by attempting to make clear the sense in which the two terms 'instinct' and 'unconscious' are understood by him, and the sense therefore in which they ought to be understood in the present discussion. Whether the attempt is wholly successful may be doubted. At all events his definition of instinct involves a theory which does not appear to be accepted by Dr Jung, and is not wholly accepted by Dr Myers, while his view of the unconscious, carried over from the 1918 Symposium, involves a theory which is not adopted by Dr Jung. Hence it might be well to devote part of the present paper to a further survey of the ground and a further attempt to clear up the issues in the present discussion, so far as a psychological universe of discourse is concerned. This is all the more necessary owing to the fact that both terms tend to be used somewhat loosely and indefinitely, sometimes with a psychological connotation, sometimes with a physiological, sometimes with a biological, and not infrequently with a philosophical or metaphysical. Especially perhaps is this the case with the unconscious. The psychology of the unconscious has too often tended towards a somewhat crude mysticism which is to be deprecated in the interests of progressive science psychical or physiological. It is difficult to avoid the conclusion that such a tendency is partly the inevitable result of using a term like 'the unconscious' and resting content with mere exclusion in lieu of definition, and is partly the nemesis of the murderous attack on language perpetrated in our phrase 'unconscious experience.'

In his contribution to the 1918 Symposium, Dr Rivers, in place of clearly defining the unconscious, chose rather to indicate the sense in which he understood the term by an illustration, a case of claustrophobia. It is true that this indication marked off a certain definite group of phenomena as that group with which he was concerned in the Symposium in question, and at the same time suggested that as far as the human being was concerned the phenomena were more or less abnormal. But in a discussion like the present something more than the citing of individual

[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

cases seems necessary if we are to arrive anywhere, and for my part I do not see my way to accept the suggestion that the term 'unconscious' should be in the main restricted to abnormal phenomena of the dissociation order, or rather that such phenomena are necessarily abnormal. I would therefore in the first instance attempt to discover in what sense we can regard the unconscious as psychical yet not conscious in the ordinary sense, and then try to bring my result into relation with the kinds of phenomena we can agree to group under the head of 'the unconscious.'

I take it that 'psychical' includes facts of at least two distinct kinds, those of the order of 'dispositions'[1], and those of the order of 'experiences.' Dispositions as such, though determining experience, are never experienced. They therefore necessarily belong to the sphere of the unconscious in a wide sense. This is not, however, the sense in which Dr Rivers ostensibly uses the term, for he speaks of "experiences of which we are not conscious"[2], and in the opening paragraph of his paper he explicitly states that he uses the term "for experience which is not accessible to consciousness except under certain special conditions," though it is worth noting that both he and Dr Jung actually do include dispositions among the facts of the unconscious which they cite and discuss, and it may be truly said of dispositions that they are "capable of influencing consciousness and conduct indirectly in various ways." Let us however consider experiences. What kind of experiences can we include under the unconscious? Dr Rivers specifically excludes 'marginal' experiences,—the 'fringe of consciousness'[3]. Are there experiences apart from these which the psychologist can admit as in any sense unconscious? This it appears to me is a question of great interest and importance for the pure psychologist. As a pure psychologist I am now speaking. In ordinary speech we say that we are conscious only of that of which we are *personally* conscious. I do not think there can be any doubt that there is 'subpersonal' consciousness, as for example a purely perceptual consciousness.

I have elsewhere[4] described consciousness as 'psychical integration,' and this integrating function I take to be the peculiar and unique characteristic of experience. But in addition to the psychical integration in different degrees, which is consciousness, psychical life presents a character which we may call 'aggregation,' or better 'synthesis,' and again in varying degrees. The highest degree of synthesis is presented

[1] Stout, *Manual*, I. ch. ii.
[2] This *Journal*, IX. 237.
[3] *Loc. cit.*
[4] *Instinct in Man*, Cambridge, 1917.

by the rational, self-conscious psychical life. A lower degree of synthesis is exhibited on the level of ideal representation. A lower degree still is shown on the purely perceptual level. In the first case the synthesis clearly constitutes a 'self' or 'person.' In the second case this is not so clear, but there is nevertheless a continuity, which may without any great strain of language be also called personal. But the synthesis on the purely perceptual level is without doubt 'subpersonal.' It ought to be noted also that the syntheses of the psychical life are at bottom dispositional, and only indirectly, as it were, affect experience[1].

I would suggest then that experiences which are subpersonally conscious and the corresponding dispositions would include some at any rate of the phenomena at present under discussion. Repressed or dissociated experiences or dispositions would be included, as well as experiences of which we were never personally conscious. But this 'unconscious' is not really unconscious. An unconscious which is really unconscious, apart from the disposition, does not appear to be psychical at all. The unconscious I would regard as subpersonal consciousness, including, in so far as it is experience, purely perceptual or even subperceptual experience.

The meaning of instinct for the psychologist I have discussed at length elsewhere[2], and there is no purpose to be served in repeating the discussion. Briefly, the sense in which I understand the term is 'determinate conscious impulse which is not determined by previous individual experience, but which nevertheless enters into, and determines individual experience and attitude.' Structurally such determinate conscious impulse would be represented by an innate psychophysical disposition, functionally by an experience, normally at the subpersonal level when occurring in its purest form. Regarded in this way the psychological relations of instinct and the unconscious become more or less obvious. In its wider sense, as inclusive of both disposition and experience, the unconscious covers a group of psychical facts of which the facts of instinct constitute a part. In its narrower sense as applied to experiences, which, as Dr Rivers says, are not conscious, and which I prefer to call subpersonal, the unconscious again covers a group of facts, of which

[1] I fully recognise that this necessarily brief statement is inadequate, but another opportunity must be taken for its expansion. One of the greatest services rendered to the science of psychology itself by recent work on abnormal manifestations of the unconscious is the way in which it has secured that emphasis will henceforth be laid on the exceeding great complexity of the psychical life of Man. See also below on the 'strata of consciousness.'

[2] *Op. cit.*

the facts of instinct (including appetite) probably constitute the main part. This appears to be as far as our present line of thought will take us.

Hitherto we have avoided dealing with some of the most interesting points raised by Dr Rivers, Dr Myers, and Dr Jung, in order to establish a standpoint from which the various facts may be regarded. Dr Rivers suggests that the 'all-or-none' reaction is characteristic of instinctive behaviour, and that this may be taken as a basis for the psychological definition of instinct. Dr Myers, however, shows that this kind of reaction is characteristic of a rather primitive form of reflex, and hence to define instinct by means of this characteristic is impossible. It may be noted in passing that an explanation of this apparent difficulty and difference is that external behaviour can never furnish us with an ultimate psychological criterion. Reactions that are alike as far as externals go may be psychologically very different. Pure 'behaviourism' of the extreme kind invariably and inevitably breaks down as a psychology. Nevertheless this 'all-or-none' kind of behaviour is worthy of the most careful attention. It is clear that reactions which have the highest possible specificity will inevitably be of the 'all-or-none' description. It is *this particular element* in the situation, and nothing else matters; it is *this specific response* and nothing else. Physiological implications apart, the 'all-or-none' type of reaction is preeminently characteristic of behaviour on the purely perceptual plane, though it is only when we concentrate attention on one particular reaction that this becomes evident. The grading of reaction upon which both Dr Myers and Mr Graham Wallas lay stress, is, as it were, a subsequent characterization from the point of view of a higher level. But in any case this type of reaction is always possible, even for the organism whose psychical development is far above the purely perceptual plane, whenever at the particular moment of reaction the operation of the higher psychical levels is for any reason suspended, a notable instance of which in normal life is the case of violent emotion. As McDougall has pointed out[1], the primary emotions are invariably bound up with instinctive behaviour, and it is precisely those emotions which manifest themselves most frequently in a violent form, and which therefore tend to determine action at a purely perceptual level. Hence instinctive behaviour will always in a certain sense be of the 'all-or-none' type, and this sense seems to have largely influenced Dr Rivers in his emphasizing of this type of reaction in connexion with instinct. In this sense I agree with him as against Dr Myers and Mr Graham Wallas.

[1] *Social Psychology.*

Dr Rivers initiates a still more interesting, if somewhat speculative, line of thought by his attempt to correlate the difference between instinctive and intelligent behaviour with the difference between protopathic and epicritic sensibility, and his suggestion that we should henceforth substitute the two terms 'protopathic' and 'epicritic,' as descriptive of two psychical levels, for the terms 'instinct' and 'intelligence,' in our psychological terminology. Like Dr Myers I wish to cast the net still wider, and wider even than he does, though in a different direction. Dr Jung has distinguished between the 'personal unconscious' and the 'collective unconscious.' In the 1918 Symposium Dr Ernest Jones, following Freud, emphasized the distinction between the 'primary' and 'secondary mental systems,' which are "precursors of the unconscious and conscious mind" of adult life, the 'primary' system being "dominated entirely by the hedonic 'pleasure-pain principle,'" the 'secondary' "by the 'reality principle.'[1]" I have elsewhere[2] drawn attention to the fact that the distinction between appetite and instinct rests on a somewhat similar basis.

All this seems to indicate the desirability of recognising different 'strata' in the psychical life, one as it were overlying another. An evolutionary interpretation is almost inevitable. But due caution must be exercised in such an interpretation. We can hardly suppose that the evolution of an overlying mental system A has been unaccompanied by evolutionary changes in the overlaid system B. That is to say, we cannot assume that the system B as now existing is the primitive or primary mental system. The presumption is all the other way. This is a difficulty which is continually cropping up in the biology, no less than in the psychology of instinct. Thus, in another connexion, Dr Jung finds it impossible to explain on either the Darwinian or the Lamarckian hypothesis the evolution of such an instinctive manifestation as that of the yucca-moth. Recent biological work indeed indicates the need of supplementing both hypotheses to account for these and similar cases[3]. But, if we assume, as we should, that the relation between flower and moth has itself evolved, with evolutionary changes in flower as in moth, and indeed in the whole world to which the relation and situation belong, the matter bears an entirely different complexion. So too with regard to the overlaid system B. I am entirely in agreement with Dr Myers when he contends that the pure original protopathic sensibility is not

[1] This *Journal*, IX. 250. [2] *Op. cit.* 255.

[3] Cf. Tait, "Experiments and Observations on Crustacea." *Proc. Roy. Soc. Edin.* XXXVII.

found in the normal intact organism after the epicritic has been developed. So too the pure original instinct (in the biological sense as well as the psychological) will not be found after 'intelligence' has once functioned, at least if the intelligence be on a level higher than the perceptual.

But psychologically there is a more interesting aspect of the question than that dealt with by Dr Myers. I would preface what follows by postulating that for psychology all explanation of the psychical must be based upon our knowledge of the psychical. If we abandon that principle our psychology will become capable of any extravagance. Now, since our only direct knowledge of the psychical is in our own experience, our explanation and interpretation of even the primordial psychical must be in terms of what we know of the psychical in this way. The simplest experience we know is of the perceptual order, but there is reason to suppose that experience at a lower level than our perceptual experience exists. Such experience, however, must be described and interpreted in terms which hold invariably of the experience we do know, and presumably, *mutatis mutandis*, of all experience. Hence Freud's 'primary mental system,' "dominated entirely by the hedonic 'pleasure-pain principle,'" the protopathic sensibility emphasized by Dr Rivers and Dr Myers, my 'appetite' level, must equally be regarded,—in so far as they represent a primordial, and at one stage in evolutionary history the sole, experience,—as essentially bipolar, and as essentially integrating the life activity of an organism and the nature of an environmental situation, related to the life of the organism, in the unique phenomenon we call consciousness.

If this be allowed, some interesting conclusions appear to follow. In the first place, behaviour which shows a character that may be regarded as analogous to the 'all-or-none' reflex will almost necessarily be the issue of primordial experience which is of the perceptual or subperceptual order; but in proportion as this primordial consciousness is overlaid by consciousness of a higher order than the purely perceptual, the behaviour will tend towards consciously determined coördination and grading. Moreover, with the development of discriminative sensibility, the cognitive function of the primordial experience will tend to be obscured, so that it will seem as if the affective constitutes practically the whole of the experience, and we may be led to consider this primary mental system as "dominated entirely by the hedonic 'pleasure-pain principle,'" while the secondary or higher is "dominated by the 'reality-principle,'" simply because of the obscuring of the cognitive function of the lower. But in any case this lower system I should prefer to call 'appetite' rather than 'instinct.' There is one possibility that should not be lost sight of. That is the possibility of avenues of sensibility characteristic of the lower system being entirely obscured by the development of the higher, but becoming traceable under abnormal conditions.

Finally, as regards the topic at present under discussion, we seem to be driven towards the same conclusion as before with regard to the unconscious and instinct. The unconscious or subpersonal consciousness will be consciousness mainly at or below the perceptual level, and therefore consciousness in which appetite and instinct will have the fullest play, but to identify the unconscious with instinct is impossible. The unconscious is wider than instinct, though undoubtedly instinct plays in the realm of subpersonal consciousness a part that is certainly more prominent, if not more important, than in the realm of personal consciousness.

I might represent the 'strata' of consciousness schematically in some such way as the following:

A. Personal consciousness	1. Conceptual level—*Rational purpose*.
	2. Subconceptual or Ideational level—*Desire*.
B. Subpersonal consciousness	1. Perceptual level—*Instinct*.
	2. Subperceptual level—*Appetite*[1].

How this would be related to the distinction between cortical and subcortical process on a physiological basis is not very clear, but the tentative identification of personal consciousness with cortical process, and subpersonal with subcortical is perhaps worthy of investigation.

My main conclusion as regards the subject of the present Symposium may now be stated very briefly. The unconscious or subpersonal consciousness underlies at all times the conscious or personal consciousness, just as the instinctive propensities underlie the ends and purposes of our rational activities, and it is unconscious because it represents either a stage of psychical evolution beyond which we have passed by normal development, or a mass of experience upon which we have, as it were, tried to turn our backs by some more or less abnormal process of dissociation, repression, or substitution, but instinct has precisely the same psychological position and function in subpersonal as in personal consciousness.

There is one part of Dr Jung's paper to which I should like to make

[1] Continuous lines show normal relationship, broken lines possible distribution.

some reference in conclusion. After defining the unconscious as "the totality of all psychic phenomena that lack the quality of consciousness," and after distinguishing between the personal unconscious and the collective unconscious, he suggests that the 'archetypes' of thinking[1] belong with the instincts to the collective unconscious. The archetypes are the recognised categories or forms of thought, together with unrecognised forms including forms of Bergsonian 'intuition.' While appreciating Dr Jung's general position, I do not think there is any psychological justification for thus placing instinctive processes and archetypes of human thinking side by side. Distinct modes of cognition, determinate directions of conation, specific characters of affection, are all data which the psychologist studies, and of which he attempts to give a rational description and explanation, but the forms of thought are ultimate and fundamental postulates of psychology as of all human science and philosophy. They are as ultimate as experience itself. We could not study thought as such scientifically, unless we could in some way get outside of thought; and to relegate the instincts and the unconscious to the same order of existence as the archetypes or forms of thought seems equivalent to putting both for ever beyond the range of human science.

[1] "Archetypes of human apprehension" is Dr Jung's phrase. I have interpreted this as equivalent to 'categories or forms of human thinking.' I am not sure that this is quite what Dr Jung had in mind. The *instinctus naturales* of Lord Herbert of Cherbury included both *notitiae communes* and instincts, in our sense, the former being apparently 'innate' ideas as understood by Descartes, Locke, etc. Now the "determining constituents of experience," which are *a priori*, are from my point of view either the instinctive tendencies or the categories or forms of thinking—thinking, that is, either perceptual or conceptual.

INSTINCT AND THE UNCONSCIOUS[1].

VI.

By W. McDOUGALL.

The new definition of instinct proposed by Dr Rivers and apparently accepted by Dr Jung seems to me to be ill-founded for several reasons. First, I find it very difficult to believe that the 'all-or-none' principle holds good of the single nerve fibre or neurone. Second, even if this were established beyond doubt, and even if it were shown to hold good of the spinal reflexes generally, we should still have to believe that in the working of the higher levels of the nervous system it is completely overlaid and disguised by some compensating principle; for in almost all mental functions delicate and continuous gradation of intensity seems to me to be the rule; and especially does this seem to me to be true of the impulses to action and the accompanying emotions which in ourselves seem to be the nearest approach to purely instinctive functioning. Dr Rivers admits that his principle does not hold good of the complex instinctive actions of insects; and to me it seems to hold good as little of the instinctive reactions, *e.g.* fear and anger, of the animals that are nearer to our own ancestral line. The dog shows every gradation of anger from a faint growl to the most furious excitement; the rabbit retires to his hole with very different degrees of haste and energy according to the nature of the alarming impression; a horse 'shies' with very various degrees of vigour and range of movement; and I cannot doubt that, if the visceral expression of the emotions of animals were quantitatively observed, they would show the same infinitely subtle gradations of intensity as our own emotions and their bodily expressions. That the instincts belong to the 'protopathic level' seems to be roughly true, but nothing that Dr Rivers has said justifies us in going beyond this general statement.

I adhere then to the conception of instinct that I have set out elsewhere[2] and which Dr Drever in the main accepts[3].

It seems obvious that, in this, as in so many other discussions of 'the unconscious,' under this unfortunate word two quite distinct notions are confused. Dr Jung defines "the Unconscious as the totality of all psychic phenomena that lack the quality of consciousness," and the

[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

[2] *Social Psychology*.

[3] *Instinct in Man*.

course of his discussion shows that for him the term 'psychic phenomena' includes facts of mental structure and facts of mental functioning, or, as Dr Drever says, "facts of at least two distinct kinds, those of the order of 'dispositions,' and those of the order of 'experiences.'" I have elsewhere protested against this confusion and attempted to show how it may be avoided[1]. But it is deeply rooted in the psychological tradition and is to be traced back at least as far as Locke's "new way of ideas," if not to Plato. It is bound up with the notion that the mind consists of 'ideas.' Dr Ward seems to have made the most consistent modern attempt to work out a psychology in terms of this conception. Suppose that with him we regard the mind as containing a vast number of presentations, of which at any time very few are in clear consciousness and all the rest present only subconsciously; even this strange assumption leaves us with the necessity of postulating a vast amount of mental structure over and above the presentations themselves, namely the structure which relates and connects the presentations, makes of them a system, and regulates the order of their rise to clear consciousness. If we crudely liken the presentations to the pieces of scenery on and behind the stage of a theatre; then we may liken this structure of the mind to the scene-shifting machinery; the appearance and grouping of the pieces of scenery on the stage cannot be understood without postulating the presence and operation of this machinery, which remains always hidden from our view. But since the theory of 'ideas' and of subconscious presentations was devised in order to be able to describe the mind as existing only of conscious stuff, it is better, when we find that this is an impossibility, to recognise the distinction between mental structure and mental functioning, to admit that consciousness is not coextensive with the psychical, and to resign ourselves to the acceptance of the fact that only mental functioning is or may be conscious, while mental structure is always outside consciousness, is in principle incapable of being introspectively observed, and can be inferred only from the facts of introspection and of behaviour. 'The unconscious,' then, may be and commonly is used to denote facts of two distinct orders, (1) the facts of mental structure, (2) mental functioning or activity of which the subject is not clearly conscious. Dr Jung's "collective unconscious" is the inherited or innate mental structure. Dr Rivers's "unconscious" seems to be nearly identical with Dr Jung's "personal unconscious"; it seems to denote both such mental activity as is not clearly conscious and those dispositions or parts of the total mental structure which are involved

[1] *Psychology, the Study of Behaviour*, Home University Library. Chap. III.

in such 'unconscious,' subconscious, or co-conscious activity. We are then discussing two distinct problems, (1) the relation of instinct to the innate structure of the mind, (2) the relation of instinct to 'unconscious' mental activity and to the mental structures involved in such activity. These two problems, though not unrelated, are distinct and require to be separately discussed.

Instinct is by general admission innate, or, as I have preferred to put it, instincts are innate dispositions, parts of the innate structure of the mind. Do the instincts of any species constitute the whole of that innate structure? In the case of the human species we may, I think, confidently answer—No, the innate structure of the human mind comprises much more than the instincts alone. I have elsewhere argued that an instinct is not only an affective or emotional-conative disposition, but also always has a cognitive side, and that the cognitive part of the total disposition which is an instinct is in many cases much more complex and highly developed than is commonly recognised; for it renders possible the perception of, and discriminative reaction upon, complex objects independently of previous individual experience[1]. If this be so then this fact in itself indicates a considerable development of the innate structure of the mind on its cognitive side. For, as I have tried to show elsewhere[2], each specialised cognitive disposition is but a differentiation of an older system[3], and its operation implies the activity of all that part of the system to which it stands in the relation of direct differentiation or evolution. But we cannot, I think, stop at this point. There are many facts which compel us to go further in the recognition of innate mental structure, such facts as the special facilities shown by individuals in music, in mathematics, in language, and other aesthetic, moral and intellectual endowments. This question of the extent and nature of the innate endowment or innate mental structure is only beginning to take shape and to be seriously considered; it remains one of the largest fields of work for psychology. I will only venture a few discursive remarks upon it. At present we are in the stage of not having secured general recognition for the instincts as part of this innate structure of the human mind; yet the instincts are the part of the innate

[1] This, I have argued, is true not only of many animals but of man also, as most clearly in the case of the sex instinct, which is normally so far specialised on its cognitive side as to confine the specific affective response of the instinct to members of the opposite sex (cf. "The definition of the Sexual Instinct," *Proc. Roy. Soc. Med.* 1914, VII.).

[2] *Psychology, the Study of Behaviour.* Chap. III.

[3] Just as each conative part of each instinct is a specialisation of an original conative disposition through which the conative energy of the mind is directed to a special end.

structure which is the least difficult to discern and define. The discovery of the nature and extent of the remaining parts must be a matter of great difficulty; and for the same reason that the definition of the human instincts is difficult, namely that innate structure may be given or laid down in the mind with all degrees of definiteness and completeness, varying from the highest degree (as exemplified in some of the animals when they execute complicated and delicately adjusted perceptual reactions to objects upon their first contact with them) to a very low degree, at which the innate structure, as given, is incapable of determining the course of mental process but merely facilitates in some degree that form of mental activity or experience through which alone it becomes further elaborated and differentiated.

If it is recognised that the instincts are but a part of the innate structure of the mind, it would yet seem that they are a very special part, and as a speculative hypothesis I would suggest that they differ from the rest not only in that they are the great channels of conative energy but also in that the nervous and bodily structures through which they operate are also innately laid down, whereas for the rest of the innate mental structure no such bodily organs are given.

In the foregoing remarks I have assumed the truth of the developmental view of mind. But it is possible to hold a 'preformation' view, to hold that what we call mental evolution in the animal world and in man is but the development of bodily and nervous mechanism through which the pre-existing powers of the soul are rendered capable of manifesting themselves progressively more fully in material organisms. Something of this sort seems to be implied in Dr Jung's doctrine of the 'collective unconscious,' and I would press him to tell us more frankly whether this is his view[1]. On the other hand Dr Jung seems to accept a Lamarckian view of mental evolution, and to believe that the innate mental structure or 'collective unconscious' is in large part a deposit from racial experience.

These racial deposits he speaks of as the archetypes or primordial thought-feelings, and I gather that in his view these archetypes are in the main concerned with primitive myths and phantasies, and have little or no part in the plain thinking of the plain man in his sober waking hours. I should like Dr Jung to tell us whether our understanding of space depends upon an 'archetype' or is in any way conditioned by

[1] The reader may be reminded that even so distinguished a biologist as Mr Bateson seems to find himself turning to some such 'preformation' view. (Presidential Address to Brit. Assoc. 1914.)

his 'collective unconscious.' To me it seems that all attempts to show that our understanding of space is built up in the course of individual experience, by association or synthesis of sensations, or in any other way, are definitely bankrupt, and that we are bound to believe that it is provided for in the innate structure of the mind, both in men and the higher animals; further that many instincts are intimately related with this part of the innate structure; for many animals display an intimate practical understanding of spatial relations in what seem to be purely instinctive activities, as *e.g.* the spider and the bee, and the nest-building and migratory birds. This seems to be one of the most promising lines along which to study the relation of Instinct to the 'collective unconscious.'

The other problem is the relation of instinct to the 'unconscious' activities of the mind and the dispositions concerned in them. This is the problem to which Dr Rivers directs our attention. First I would protest against his use of the word 'experience.' My impression is that by modern English writers on philosophy and psychology this word is by common consent used as the most general term to denote conscious mental activity. But Dr Rivers uses it in this and in other recent articles to imply that which is retained by the mind in consequence of experience. He condenses his view of the relation of instinct to the unconscious in the sentences—"suppressed or dissociated experience makes up the unconscious," and "Instinctive reactions with their associated experience are thrust into the unconscious because they differ so greatly in nature from those developed later that the two are incompatible with one another." Here we have illustrated the lack of clearness which inevitably results from neglect to distinguish between activities and dispositions. Dr Rivers describes the unconscious in one sentence as consisting of "dissociated experience," and in the next paragraph as consisting of dissociated "instinctive reactions with their associated experience." Now according to all ordinary usage of language both 'experience' and 'instinctive reactions' are activities; but it is, I think, fairly clear that Dr Rivers means to tell us that the instinctive dispositions become suppressed and dissociated, and in this condition constitute the 'Unconscious,' because when they function or are active in this dissociated condition their activities remain outside personal consciousness or inaccessible to ordinary waking introspection.

This view I take to be seriously in error, and it is, I think, easy to convict Dr Rivers of his error out of his own mouth. He tells us that he accepts "the position, now very generally adopted, that such emotions as fear and anger, with the special form of behaviour they accompany, belong to the domain of instinct." In that one point I am happy to find myself in agreement with him.

Dr Rivers also speaks of "the fundamental instinct of self-preservation"; and in another passage he speaks of running away as an instinctive reaction to danger of animal, child, and adult man. What is it, then, that he conceives to be suppressed or dissociated in the adult in relation to this instinct of self-preservation? It can hardly be the whole instinctive disposition; for he seems to recognise that its activity expresses itself directly in both the consciousness and the behaviour of the adult. Is it then the "instinctive reaction of running away with its associated experience" that becomes dissociated? Dr Rivers will hardly maintain this. For he has told us that "if the danger is sufficiently great...even the adult man...will devote every atom of his available energy to flight"; and there can be few of us who cannot recall vividly to mind the 'associated experience' of such headlong flights undertaken at various ages from early childhood onward.

I have devoted a volume to showing that every human instinct of any importance remains capable of and does constantly succeed in manifesting itself in adult consciousness and behaviour; but beyond fear and anger, Dr Rivers has not told us what human instincts he recognises. We might be led to suspect that Dr Rivers postulates a crowd of human instincts not recognised by most of us and constituting in their dissociated condition 'the unconscious'; but he has elsewhere made clear that the suppressions and dissociations with which the psycho-therapeutist has to deal are in his view mainly connected with the instincts of sex and of self-preservation or fear. Confining our attention then to the instinct of self-preservation, we have to ask Dr Rivers to tell us more explicitly what is it that in his view becomes dissociated in the adult. The instinct continues to determine behaviour of appropriate kinds accompanied by the appropriate emotional experience and therefore is not dissociated. The instinctive reaction of running away is not dissociated. He mentions only one other 'reaction' of this instinct, namely screaming. This, like running away, and like all other bodily reactions that can be and commonly are regarded as the natural expressions of fear, including all the visceral symptoms and signs, continues to show itself in the normal adult and to require his control and, if, as Dr Rivers says, "the danger is sufficiently great" to escape it. It, therefore, is not dissociated. But perhaps Dr Rivers believes that this instinct comprises other 'instinctive reactions' which become dissociated; perhaps he conceives it as Freud seems to conceive the sex instinct, namely as made up of an indefinite number of components which are shed or dissociated, during the course of individual development. If so, he should, I think, give us some indication of these components. The fact is that, before this problem of the relation of instincts to repressions, suppressions, or dissociations can be profitably discussed, it is necessary to set out clearly what we mean by instincts, what instincts we recognise in man, what is the nature, what the structure and mode of operation of each instinct that we conceive to play any such part. Dr Rivers has not attempted this (and the same complaint may be brought against Dr Jung). Presumably he does not accept as approximately correct the account of the human instincts that I have given, or he would have told us so. Implying the rejection of this scheme by his silence, he offers us nothing in its place, except the very disputable propositions that instinct "is subject to the 'all-or-none' principle" and that it has 'protopathic' properties; propositions which, even if they be true and therefore presumably useful, go but a very little way towards furnishing an alternative scheme of the instincts such as is required for any profitable discussion of the present topic.

I would suggest on the basis of my own scheme of the instincts, the following concise statement of their relation to the 'unconscious' in the second of the two senses defined above. I am not sure that we should, with Dr Rivers, identify 'dissociation' and 'suppression' or in the more usual terminology 'repression'; but, for the purpose of the present discussion, I will continue to do so. The synthetic unity of self-consciousness is achieved only on the ideational level of mental life. The purely instinctive activities are on the perceptual plane and therefore, to adopt the excellent term proposed by Dr Drever, they are subpersonal; and they are mutually exclusive in so far as their ends are incompatible. With the development of the ideational life (or, in physiological terms, of the cerebral cortex) the various instincts become organized in systems, and, with the development of self-consciousness, all these become organized and duly subordinated within the one all-comprehensive system which is the 'character' of the individual man. This organization is effected through the interrelation of cognitive dispositions with which the affective or conative dispositions of the instincts have become connected through experience. When any experience issues in a strong affective reaction which is opposed to the general tendency of the total system (the self-conscious character), there is conflict which may result in the dissociation of the cognitive dispositions concerned in that experience. If the cognitive disposition (or system of such dispositions—large or small) becomes dissociated from the principal system, then, since it retains its connexion with the affective disposition of the instinct (this is what is called a complex and might be alternatively called a dissociated or repressed sentiment), it is capable of functioning as an isolated system; it may then lie quiescent, or may be from time to time roused to life as a subpersonal memory manifesting its appropriate affective-conative tendencies. The instinct itself, or its affective disposition, retains its connexions with other cognitive systems, is not dissociated, and therefore continues to play its part in personal consciousness and activity.

The question to what extent such dissociation occurs in the normal person, or whether it is confined to neurotics, is too large to be raised in this discussion. But for those who have approached this problem from the clinical side, I may sum up the position I would maintain by saying that Janet is more nearly right than Freud, though Janet made the great mistake of ignoring wellnigh completely the conative factors in the production of the *amnesias* with which he has dealt in so masterly, but too intellectualistic, a fashion.

I cannot then follow Dr Rivers in the view summarised in his last paragraph. I cannot see any evidence of such discontinuity of mental evolution. The human instincts are not suppressed by new forces and incompatible modes of mental activity beneath which they crouch like a cage full of wild beasts; that is an old and popular conception of the instincts which seems to me to be wholly false. In conclusion I would ask Dr Rivers to tell us what is this new force which appears when mental evolution has gone "a certain distance in the line of progress" and which somehow gets the better of, and normally keeps the upper hand over, those terrible fellows, the instincts. Is it, perhaps, our old friend 'Reason,' pure and undefiled, or even 'Conscience,' or its more fashionable substitute 'the Herd Instinct'? If this force (or forces) is to do the work assigned to it and in "a life-long struggle" keep the instincts in suppression, it must be a very formidable one. Yet Dr Rivers nowhere gives us a hint of its nature or its natural history. I have attempted at length to show how the instinctive forces are modified and controlled, but without ceasing to be the mainspring of all our thought and conduct, through becoming organized in the one system which is the 'character.' Dr Rivers has said nothing that weakens my belief in the essential rightness of that way of conceiving the matter, and my four years of clinical experience among the war-neuroses has only strengthened it.

THE RELATION OF AESTHETICS TO PSYCHOLOGY[1].

By EDWARD BULLOUGH.

1. *Subject-matter and fundamental problems of aesthetics.*
2. *Individual* vs. *social factors.*
3. *Receptive* vs. *creative aspects.*
4. *The origins of art.*
5. *Comparative aesthetics.*
6. *Conclusion.*

1. The subject-matter of Aesthetics is the 'aesthetic experience.' Its subject-matter is not Art or Beauty, because both these conceptions are, especially in their common connotations, too narrow. There is much aesthetic experience outside the experience of Art and Beauty. Let us start with the fairly safe fundamental assumption that there is such a thing as aesthetic experience. Then there are observable, in the first place, certain *objects* which we call beautiful or otherwise aesthetically effective (*e.g.* pretty, comic, sublime, tragic etc.); and secondly, there are observable certain *conditions of mind* in which we call certain things beautiful, which in other conditions of mind may not appear so, but leave us cold and uninterested.

Now the progress of interest from the first to the second of these aspects marks in a general way the development of the study of Aesthetics from antiquity to modern times. But in a general way only, for a consciousness of the conditions of experience was not quite absent from the mind of the ancient aestheticians. Nor must we modern students of the subject lose sight of the claims of the actual things and their constitution as capable of inducing, sometimes against adverse circumstances, those conditions of mind which enable us to appreciate aesthetically what otherwise would have left us unmoved.

In Great Britain the transition from dominant interest in the one class of data to that in the other occurred in the 18th century, since when this country (together with Italy) has maintained and developed a definitely psychological interest in aesthetic speculation. To Germany we

[1] Read before a General Meeting of the British Psychological Society on 31 May, 1919.

owe the name of the study,—that philosophical pigeon-hole in which it lived till the middle of the 19th century.

Among the multitude of things which we call beautiful or aesthetically effective there is a special class of things, distinguished by the feature that they are made by Man, and (as far as one can tell superficially) are intended to produce this impression—'objects of Art.'

The experience induced by Art-objects is in a narrower sense the subject-matter of Aesthetics. It is its subject-matter *par excellence*, while in other cases the experience may have something accidental or incidental about it.

By Art must be understood not merely the Fine Arts but all human products which aim at producing aesthetic experience, whether they are fine or applied, free or industrial, useless or utilitarian.

Despite the intentional appeal of Art-objects, the conditions in which they produce their effects are variable and diverse, not merely in point of time, but also between different persons and in face of different kinds of objects. Again, the object appears to be—though in an intentional manner—almost one of the conditions of the effect.

What then are these conditions? This is the fundamental problem of Aesthetics.

I remember walking home one night with an eminent scientist, discussing Aesthetics, the existence of which he denied. We met an urchin whistling with obvious enjoyment a tag of 'The Girl from—somewhere or other.' "If you can tell me," said the scientist, "why that boy thinks that tune is beautiful, I will believe that there is such a thing as Aesthetics." The answer, of course, was that the difficulty lay not in the absence of a reason, but in the multiplicity of reasons. In any case, I could have mentioned a large number, but the boy alone could have stated the right one.

For this extremely complicated problem is almost wholly a question, not of speculation or theory or doctrine, but of *bare psychological fact*. These psychological facts are ascertainable; they are open to observation and often to experiment. But they are extremely complicated (as psychological facts are wont to be), and are in addition rendered more elusive by the emotional element suffusing them. The greater part of modern aesthetic research has been directed to the analysis of the experient's state of mind: what happens when I am aesthetically impressed? what is this experience made up of? what constitutes the specifically aesthetic character of the experience? These three fundamental questions, with all the problems of emotional reaction, apperception, adaptation, attention, have been attacked introspectively, retrospectively, experimentally. But a vast amount remains to be done. We possess a great deal of detail, much observation and record; but there is little coordination of all this material. There is a lack, in particular, of a *common stock* of knowledge; and little progress can be hoped for in any study without such a stock of common and accepted truths.

2. At this point it is desirable to refer to two sets of double factors that are involved in all these questions of aesthetic experience.

The first is the observation that the aesthetic experience is clearly not a mere matter of personal, individual concern. The experient does not stand alone in the world, facing its beautiful or ugly objects. His reactions to them are only in part his own. In part they are also the reflexions, the repercussions, of those of others around him, the traditions of his forbears, the conventions of his age. They are in particular and to a large extent the conventions ruling the Art-object, the 'art-conventions' of the moment. In France in the 17th century a tragedy had to be in five acts, and without being in five acts a tragedy had no chance of being considered a good one. To us this seems ridiculous, but our own Art is full of similar conventions. The problem is to distinguish between what is essential and what is merely conventional in Art and in the aesthetic experience which it mediates, and not to be led astray by catchwords and momentary battle-cries.

Until about 1890 Aesthetics was almost exclusively of the individualist type. Since then the collective aspect of the aesthetic experience has received a good deal of attention, notably in France, with the support of the French school of Sociology. The result has been the development of 'Sociological Aesthetics.' The aesthetic experience came to be regarded as a social, not as an individual experience; Art became a social expression, the reflexion of an age, the product of a society. This view appeared to derive strong support from anthropological research which seemed to find convincing proof of it in the Art of primitive and prehistoric peoples.

Whatever be the ultimate truth, so exclusive and exaggerated a view of aesthetic experience is clearly as wrong as the exclusively individualistic theory had been. What we have to deal with is the combination and the interplay of social and individual factors in the aesthetic psychology of the person. What are the relations between them? On which side is the predominance? A most difficult problem, complicated as it is by historical considerations: for the relation has, as a matter of historical fact, not been stable but has changed in course of time.

3. In the second place, when we speak of aesthetic experience, we generally think of the impression made by a picture, a piece of music, a poem. What about the experience that produced these things? What about the Artist? He evidently has a claim to be heard in these matters. After all, he created them. Why? How? What happens when he does create? How does he set about it?—or does it set about him? We are told about 'inspiration,' 'genius.' What are they? Imagination is such a ubiquitous thing, so essential to the most ordinary business of life that we ought to know a great deal about it. We know something about images, imagery, visual and auditory and motor types; but all this is reproductive imagination. About creative, constructive imagination we hardly know anything of practical use.

There is reason to think that for the purpose of aesthetic study the Artist as a type—he is a very clearly defined human and psychological type—may be regarded as enjoying almost pure examples of aesthetic experience, certainly purer, more permanent, more thorough than fall to the lot of the average spectator or listener. From him we ought to secure insights into aesthetic experience far more valuable than from other sources. Unfortunately he is an excessively bad subject. He is not really interested in studying his mental processes, and generally fails to see the point of the scientific attitude to things. He is, as a rule, totally lacking in introspection; he is rarely able—unless he be a literary man or an actor—to express himself verbally with precision and without ambiguity.

The Artist is an interesting object of study, not only in relation to his own aesthetic experience, but in comparison with that of the experient. Indulgent theorists have told us in the past that we are all artists. If words are used with any accuracy, this is nonsense. Nobody is an artist unless he creates; the spectator—*ex definitione*—does not create, and no amount of 're-creating' will make him into an artist. At the same time, there must be certain features common to both types of aesthetic experience, corresponding to the specifically *aesthetic* elements. Any formulation of the experience, therefore, based on the analysis of creative and receptive experience, will have to cover both, showing identities as well as compensating differences.

The French were the first to start a systematic study of the Artist and of his processes, beginning with the elaborate monograph by Toulouse on Zola. Unfortunately this series was never continued, though it was followed by several separate studies devoted to particular artists by Binet in the *Année Psychologique*. A very much larger amount of detailed research is needed, and it can only be obtained by the willing and intelligent co-operation of artists themselves.

To sum up there are two cross-cutting factors to be dealt with: (i) the individual *vs.* the social factor in the experience both of the recipient and of the artist, and (ii) the receptive *vs.* the creative aspects in aesthetic experience.

4. Let us assume that after an exhaustive analysis, research into the individual and social determinants, experiment, observation, reminiscences and introspection, we have reached a formula capable of fitting every kind of experience that is mediated by Art and Nature, and capable of meeting the similarities as well as differences of artistic production and aesthetic reception. Where shall we then stand? We shall have formulated what we mean by aesthetic experience. We shall have a formula—if I may use this word to indicate a relatively stable, pure and probably fundamental type of mind and behaviour—of what we mean by 'aesthetic consciousness.'

But how did this type come into existence? Along what lines has it developed in human history?

This is the aesthetic aspect of the problem of the History of Art and of the Origins of Art, and it is essentially a problem for Psychology.

The problem of the origins of Art is fairly ancient. But it has never had a better chance of being attacked and possibly of being correctly answered than at present, thanks to the great advances of anthropological research in recent years.

It must, however, always be borne in mind that the problem of these origins is not a question only or even predominantly of the origins of those objects which we call Art. It is also and especially a question of the origins of the types and attitudes of mind that created those objects.

The amount of confusion of thought on this topic is truly amazing. Compare, for instance, the mostly valueless stuff written on folk-song alone. The vast collections of mere data, uncritically arranged, compiled without a principle relevant to Aesthetics, incapable of answering hardly a single of its really vital and fundamental questions, are largely useless and constantly suggest opportunities of information missed and so often lost for good and all.

Of course, the problem of the origins of the objects themselves is most interesting, and in so far as it might throw light on the psychological issue, most important. But in themselves the objects supply no answer to this question. Anthropological research led very speedily to the discovery that the greater part of what we call Art—primitive Art—

was *not* Art to the men who made it. And once this discovery had been made concerning primitive Art, a good deal of what hitherto had been considered without question as Art, even in highly advanced stages of culture, has become similarly suspect.

The real question is--how and at what point in the history of any particular culture did the men who made the things produce *Art*? Even the Greeks treated their architects as we treat our masons; *we* call Architecture the 'Mistress-Art' and in the Chinese canon of the Fine Arts we find included therein Archery, Mathematics and Manners.

Here again Aesthetics demands an aesthetically directed psychological research into those internal developments of culture, whereby an originally undifferentiated system of traditional sentiments and conceptions became split into those separate strands of feeling, those distinct systems of ideas, which we have come to term Religion, Magic, Justice, Right, Truth, Craftsmanship, Art. These developments must have taken place —within historical times they are known to have taken place—concomitantly with important external changes in the objects connected with these systems, under the stress of divisions of labour, distinctions of craftsman and public, castes, guilds, priest and layman, the development of media of currency, imports and exports, the effects of wars and travels, interferences of cultures and a number of other factors, which may perhaps all be considered as the outward manifestations of those inward changes which are the true objects of aesthetic research.

Such a research, if successful, should furnish us with the answer to our question, how and why has that mental attitude which we call the 'aesthetic consciousness,' through which and within which we realise aesthetic experience, come into existence, and what has been its history?

The attempt has often been made to fathom the mystery of Beauty by the research into prehistoric and primitive Art, but it has resulted in mere imaginings and wholly unsupported constructions of what the men of Altamira or Neolithic man or the bower-bird thought and felt and meant when producing their 'Art.' It has proved to be an entirely unwarranted substitution of our ideas and views of things for the ideas and views of the men of the stone-age and of certain animals. It has been a vicious circle of the worst description and has meant the search after something which the searchers themselves neither knew nor were able to describe.

5. There is another circular argument of more general application and much subtler in its workings.

Let us again assume that our research, supported by anthropological, material and social-psychological analyses of past and primitive cultures, has furnished us with the whole inner history of that branch of human civilisation which we call Art (which has not always been Art in the eyes of its very creators, but frequently has become Art in those of succeeding generations),—with what one might call 'the morphology of the aesthetic consciousness.'

Is there any check that we could apply to our theories?

We are Europeans and our Art, if we cover the ground best-known to ourselves, most readily accessible and least exposed to misinterpretations, is European, *i.e.* Graeco-Roman in substance and origin. We evolve theories on the basis of this our European experience, and then prove these theories by illustrations and references taken from European Art. Our gratification at the satisfactory results requires severe tempering. Aesthetics has never broken this vicious circle. Can it be broken? Is there any other body of art-experience and material Art, any other culture sufficiently coherent, complex, and conscious, to offer a parallel to ours? There is only one, but an excellent specimen: China. Of even greater age, of the same and even greater coherence, of similar complexity and an equal degree of refinement and critical self-consciousness, Chinese Art and culture, in their at least relative freedom from European adulteration, furnish a unique opportunity for checking our theories deduced from European experience against another experience unconnected in substance, origin and development. It has never been done. We have never had the combination of knowledge requisite for the performance. We are only now beginning to attack problems of Chinese archaeology with modern methods and comprehensiveness, as in B. Laufer's admirable study of jade[1]. We have partial applications of the parallelism as in Prof. Sir W. Ridgeway's recent fascinating paper on ancient Chinese Drama[2]. Much work on archaeology has been done in China recently, but it is not available in translation, and of whole centuries of Chinese history we hardly know anything more than the outward happenings. There is an immense field for work which will have to be done, with results that I venture to think will prove illuminating in the highest degree.

6. What position have we now reached? I assume that we have secured a conception of aesthetic experience as we know it to-day, covering experiences mediated by all the arts, by every kind of aesthetic

[1] *Jade: a Study in Chinese Archaeology and Religion.* Chicago: Field Museum Publication 154, 1912.

[2] "Ancestor worship and the Chinese Drama," *Quart. Rev.* April, 1919

object whether man-made or natural, the experience of the artist as well as of the recipient, linked with the social, as well as with the individual, consciousness and tradition.

We have further secured an understanding of its history in human development. We know how that kind of experience arose, first merged in and diluted with other kinds; how it became differentiated at different stages of human history, how at certain moments of particular cultures it appeared in almost pure and unadulterated form—the 'flowering periods' of Art—to be overlaid and obscured at others by other interests, perhaps owing to some slight shifting of the relations of the individual to his community, the intrusion of some other culture, a sudden expansion of intellectual or moral horizons. We have obtained thereby an insight into the inner history of Art and also into the correlation of that type of experience with the other aspects of civilisation, with religion and law, intellectual pursuits, social structure, the conception of right and truth, with the economic and material factors of existence.

There remains the question: what is the relation of this type of experience called 'aesthetic,' to those other types which we designate by the terms 'scientific' and 'ethical'? It is the old problem of the relation of Beauty to Goodness and Truth. This I conceive to be the problem of a *Philosophy* of Art. It is the point at which Aesthetics issues out into the two fields of human interests: human conduct and human ideals. It is the highest point to which psychological inquiry, directed by aesthetic conceptions, into facts of experience can penetrate, where it must join hands at once with practical action and philosophical speculation.

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THE GENERATION AND CONTROL OF EMOTION[1].

By ALFRED CARVER.

(*From the Birmingham Psychoneurosis Clinic.*)

1. *Introduction.*
2. *Emotion aroused in connexion with instinctive processes.*
3. *The function of emotion in relation to interest.*
4. *The rôle of the bodily concomitants of emotion.*
5. *The interaction of the cortex and lower centres in emotion.*
6. *The functional levels at which integration and dissociation may take place.*
7. *Neuroses due to dissociation at various functional levels.*
8. *Anxiety states, conversion hysteria, and Babinski's 'troubles réflexes.'*
9. *Disorders of the ductless glands due to emotional shock.*
10. *Neurotic symptoms resulting from conflict between opposing forces.*
11. *The determination of pathological symptoms by previous trauma.*
12. *The process of sublimation.*
13. *Reasons for here drawing illustrations from military life.*
14. *Conclusions.*

1. THE fact that emotion plays a capital rôle in the genesis of the psychoneuroses has long been recognised; but recently increased attention has been drawn to the subject owing to its importance in connexion with the war neuroses, and the time seems ripe to undertake, in the light of such experience as has recently been gained, a brief survey of the whole subject.

I propose to consider the problem under the following headings:

(*a*) The biological function of emotion.

(*b*) The rôle played by its physical concomitants.

(*c*) The neural site of its production and control.

(*d*) The dissociation of consciousness which may arise in consequence of its excessive generation.

(*e*) The means by which, under conditions of civilisation, emotion may be turned to useful instead of harmful action.

[1] Read before the Medical Section of the British Psychological Society, 11 June, 1919.

2. Psychologists are now generally agreed that emotion arises in conjunction with instinctive processes. But instinct may be defined in either psychological or biological terms, and one of the great difficulties has always been to keep these clear and separate.

No person has ever yet satisfactorily defined emotion, and any attempt to isolate it from its setting, so to speak, is foredoomed to failure; for emotion is only one part or aspect of a more comprehensive internal adjustment which takes place in higher animals in order to enable them to react as a whole and more completely to dominate any sudden change in their environment. The individual can no longer be regarded as made up of body and mind; he is a biological entity whose activities are manifest at various levels representing successive stages of his evolutionary progress.

For the purpose of clarity, I wish to define the 'interest' of an instinct as the affective tone which accompanies the whole instinctive process when it is carried through in a normally satisfying manner; and to define emotion as the subjective experience which develops when gratification of the instinctive impulse is held in check by higher level control. The reasons which have led me to propose this definition will be made clear in the early part of my argument.

James in his classic work[1] treated instinct and emotion apart, but he added "every object that excites an instinct excites an emotion as well." His well-known view may be summed up in the phrase "I am afraid because I run away."

McDougall[2] has ably argued that emotion is merely the central limb of the whole instinctive process, which like a simple reflex has an afferent and an efferent path. He has thus shown a much truer appreciation of the problem as a whole. Yet his hypothesis merely assigns to emotion a place in a chain of events without suggesting its function, except in so far as its position in the chain indicates a reversion to the common-sense view, viz. "I run away because I am afraid." McDougall's definition further assumes that the 'interest' of an instinct is from the outset always and necessarily an emotion.

That instinctive reactions resemble and are probably the direct descendants of reflex actions has long been recognised. Romanes[3] in particular held that the only point wherein instinct can consistently be separated from reflex action is in regard to its mental constituent.

[1] W. James, *Principles of Psychology*, London, 1905.

[2] W. McDougall, *Introduction to Social Psychology*, London, 1908.

[3] G. J. Romanes, *Phil. Trans. Roy. Soc.*, London, 1877, CLVII.

Discussing the emotional manifestations of organisms at different evolutionary levels, he found that the further one descended in the scale—*i.e.* the lower the mental development of the organism—the more fixed and definite became the instinctive reaction and the less became the manifestation of emotion. Conversely it may be stated that variation of reaction marks the dawn of intelligence. In higher animals the reflexes are under control of mechanisms to whose activity consciousness is adjunct, and the crude affective state is refined until it is recognisable as emotion. Finally in man the enormous development of the neopallium (whose function it is to control the lower mechanically-fatal reflexes and more completely to adapt its possessor to changes in his environment) has led to an infinite variety of possible responses, and correlated with this a maximal disposition for the arousing of emotion. The mere disposition to experience emotion, however, still leaves emotion as an epiphenomenon, without suggesting its function. Yet function is the most important aspect of any biological mechanism and demands therefore our most careful attention.

3. Now a consideration, whether by introspection, or by observations on animals or man, of the circumstances under which the coarser emotions may be aroused leads us to the conclusion that when the conative tendency of an instinct is immediately satisfied the intensity of its emotional component is minimal. The reaction in these circumstances closely resembles a more simple reflex. If, on the other hand, satisfaction is checked, tension arises and the affective element manifests itself and is experienced as emotion. According to this view, in contradistinction to McDougall's, it is only under certain conditions that 'interest' develops into emotion. This explanation harmonizes with the findings of Romanes just mentioned, and with the view of Sherrington[1] that "the primitive emotions seem to involve little other than sense perceptions richly suffused with affective tone." In lowly organisms, indeed, response seems to be massive, on the 'all-or-none' principle, and imperative: it is what we might term a 'protopathic' reaction. On the other hand, with the gradual increase in plasticity of response which develops *pari passu* with intelligence, there is delay in reaction and with this is associated an increase of the emotional component. The longer the issue of the impulse in satisfying reaction is checked by higher level control, the greater becomes the generation of emotion, until in man with his practically unlimited choice of response we reach a stage where delay becomes almost inevitable. Hence it is rare, in man, for response to be unaccom-

[1] C. S. Sherrington, *The Integrative Action of the Nervous System*, London, 1906, 262.

panied by some generation of emotion. Seeing, then, that it is in connexion with this delay or check to the impulse that emotion arises, we are surely justified in considering that the function of emotion is to reinforce 'interest.' The aroused emotion may be likened to a repercussing force which strives to keep the object in the focus of attention and to insist upon the output of a satisfying reaction although this is now under the control of higher centres.

When satisfaction is persistently thwarted while strong emotional stimuli are continuously applied, symptoms, which in man are termed 'psychoneurotic,' are liable to make their appearance. Of these I shall have more to say later. Under the conditions of modern warfare the soldier exposed to practically incessant danger must remain inactive in narrow trenches or dug-outs, with only very rare opportunities of obtaining a directly satisfying reaction to the stimulus. This being so, one would in accordance with the view just put forward expect neuroses of emotional origin to be far commoner now than in the old days of open warfare and hand-to-hand fighting. That they have proved to be so is an indisputable fact. In 1917 it was calculated that one-third of the unwounded and one-seventh of the total discharges from the British army were permanently unfit on account of functional nervous or mental disorders.

4. Having thus briefly considered the function of emotion, let us see what light recent observations throw upon the rôle of those visceral and somatic changes by which it is characterized. The following three views exist with regard to their significance; the excitation of the corporeal concomitants of emotion

(*a*) is brought about *viâ* the visceral nerve centres, but it is the sensations thus generated through the viscera which give rise to the emotional state in the mind;

(*b*) is excited concurrently with the psychical excitement by the same stimulus;

(*c*) is secondary to the psychical part of emotion.

The first supposition was advanced by James, Lange and Sergi, and the arguments in its favour are set out in the chapter devoted to emotion in James's *Principles of Psychology*. Sherrington[1], however, has shown experimentally that in the dog appropriate spinal and vagal transection, although completely cutting off sensations from the viscera and from all the skin and muscles behind the shoulder, produced no obvious diminution of emotional character, fear, joy and disgust being as readily pro-

[1] *Op. cit.* 260.

vocable as ever. This being so, we need not dwell at length upon the first supposition.

Discrimination between the second and third hypotheses is difficult, if not impossible. The purposive nature of reflex action is one of its most noteworthy features, and it may be argued that the bodily adjustments characteristic of emotion are provocable even when the psychical state remains absent. In this event the same stimulus which elicits what we regard as the expression of the emotion must also concurrently and *per se* excite the psychical state. As Sherrington[1] pointed out, animals reduced to a 'spinal' condition react to stimuli which in the intact animal would produce pain, by movements appropriate for escape from or removal of, the stimulus applied. Head and Riddoch[2] have further shown that the spinal cord in its mass reaction exhibits a number of manifestations which might well fit in with most definitions of emotional responses. Goltz[3] also observed, for many months, a dog whose cerebrum had been ablated. Though this animal appeared in general quite neutral to its surroundings, it was still capable of evincing signs of displeasure or anger. This led Goltz to the conclusion that anger is a more primitive emotion than fear, joy, etc. For, as we might expect, the less highly evolved reactions are localised in the more caudal parts of the central nervous system. Sherrington argues that the retention of the expression of anger by Goltz's dog indicates that by retrogression the complex movements of expression may, in certain coarser emotions, have passed into a simple reflex act, and that the stimulus originally arousing strongly affective ideas may come through the canalising force of habit to cause a discharge of the act before it can be apprehended as an idea[4]. The pseudo-affective reactions indicative of resentment are, however, short-lived after ablation of the cortex, the simulacre of mere flashes of mimetic passion.

May it not be that after removal of the higher centres the original reflex reaction, upon which a controlling superstructure was subsequently built up, continues unchecked, though with the removal of the controlling influences the emotional component of the reaction ceases? In this case there is no cerebral reverberation and the response is massive, fixed and short-lived.

A somewhat analogous and more easily studied mechanism is met with in the act of micturition. Here we can trace the acquisition, by the child, of voluntary control over a simple reflex action and, in cases

[1] *Op. cit.* 266. [2] H. Head and G. Riddoch, *Brain*, 1917, XL. 188.
[3] F. Goltz, *Pflügers Archiv*, 1892, LI. [4] Sherrington, *Op. cit.* 267.

of subsequent interference by injury with the paths through which control came to be exercised, the dissolution of the more highly organized products of mental and neural activity leaving the more lowly at liberty again to express themselves freely. According to the completeness of the dissolution the interference manifests itself on the effector side, on the afferent side, or on both[1].

In view of these general conclusions we may consider that although visceral and somatic sensations undoubtedly contribute to reinforce emotion—probably by a process of repercussion which will be considered later—they are not its actual excitants, and that as compared with the cerebral reverberation to which the psychical experience is adjunct, they count for very little even in the coarser emotions of the dog.

It may then be asked what is the rôle played by the bodily concomitants of emotion? Numerous suggestions have been offered to account for them, but all the more recent experimental observations demonstrate that these bodily changes are of such a nature as to put the organism physically in the most advantageous condition to respond effectively to affective stimuli.

Cannon[2] and his co-workers in particular have shown that every one of these visceral changes is directly serviceable in making the organism more effective in the violent display of energy which fear, rage, or pain may involve. The changes are mostly the immediate result of sympathetic activity with which the action of the endocrine glands is associated. But Orr and Rows[3] have shown that "the sympathetic system is intimately linked up with the central nervous system and has no true autonomy." Thus the fact that the visceral adjustments are brought about *viâ* the sympathetic system in no way conflicts with the theory of the cerebral origin of emotion. Cannon[2] has shown by experiments on cats that psychic excitement can through sympathetic paths bring about a hypersecretion of adrenin. The action of adrenin seems identical with that of the sympathetic nervous system itself, and causes adjustments in the economy of the organism which favour an increased energy output. Either directly or indirectly adrenin raises the blood pressure, alters the distribution of the blood in the body, stimulates the heart, dilates the bronchioles, restores to fatigued muscle its normal irritability and liberates sugar into the circulation. These are precisely the phenomena

[1] E. G. Fearnsides, *Brain*, 1917, XL. 184.

[2] W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage*, 1915.

[3] "The Interdependence of the Sympathetic and Central Nervous System," *Brain*, 1918, XLI. 15.

which we meet with as manifestations of the primitive emotions, and their utility to the animal in enabling it to respond effectively to the stimulus is self-evident. They are precurrent reactions, but cannot be regarded as originating the psychic component of the emotional complex.

5. In approaching the problem of affective states by experimentation on animals we are, unfortunately, cut off from introspective help, but disease in man sometimes enables us to study the problem without labouring under this impediment. Head and Holmes[1] have taken advantage of this fact by observing the alterations in affective state in patients suffering from lesions of the cerebral cortex and other parts of the brain. The conclusions arrived at by these observers are that the optic thalamus contains the terminal centre for all sensory stimuli, and that from here the latter are regrouped and distributed in such a way as to act both upon the essential thalamic centre and the sensory cortex; the thalamic centre itself being especially concerned with the affective aspect of sensation, while the cortex exercises discriminative and inhibitory functions.

Not only does emotion, once aroused, serve to bring into play associative memories and stored-up stimuli, but at the same time there seems to be a diminution of the higher control normally exercised through the cortico-thalamic tract; the mechanism here being comparable to, if not identical with, that of 'reciprocal inhibition with simultaneous double sign.' A similar phenomenon is also observable in the inhibitory influence which the cranial and sacral divisions of the automatic system exercise over the sympathetic proper. Again it is well recognised that, in the motor system, there is a delicate adjustment between the reactivity of the upper and lower neurones, and that impaired function of the upper neurone allows more final common paths to be forced in the lower. The relatively few observations which have been carried out indicate that a similar relationship pertains in the sensory system.

6. Another very important aspect of the work of Head and his collaborators is their demonstration of the way in which integration can occur at all afferent functional levels of the nervous system. They show that the struggle of the higher centres to dominate instinctive tendencies is begun at the lower afferent levels, but becomes more clearly defined the nearer sensory impulses approach the field of consciousness. This may be looked upon as a corollary to the statement by Romanes referred to previously.

The question as to the level at which the converse process, viz. dis-

[1] "Sensory Disturbances from Cerebral Lesions," *Brain*, 1911–12, XXXIV.

sociation and loss of control may occur in the neuroses, is one of considerable interest, especially in view of its bearing upon the class of phenomena which Babinski[1] has labelled 'reflex.' Bechterew and Pighini[2] hold the cortex cerebri itself to be made up of many sensori-motor levels, each in functional and anatomical connexion with neighbouring centres. And as Sherrington[3] says, "each synapse is an apparatus for coordination, it introduces a common path." There would seem no inherent reason against the hypothesis that functional dissociation may occur at any level, while there is much evidence in favour of such a view.

In this country as I have urged elsewhere[4], the attention of observers has been too exclusively directed to the psychical aspect of the war neuroses, and the underlying physical basis of disturbances has been too generally neglected. Head[5] truly remarks that "the day of the *a priori* psychologist is over....A man can no longer sit in his study and spin out of himself the laws of psychology by a process of self examination. For we have been able to show that, at a level deeper than any he can reach by introspection, are prepared those states which condition the nature and characteristics of ultimate sensation....At each anatomical situation in the central nervous system dissociation assumes forms appropriate to the functional combinations of afferent impulses at that point....Finally a lesion of the cortex causes changes which can be expressed in psychical terms only."

7. There is a general consensus of opinion that the neuroses, both those of peace and war, arise as a result of functional dissociation due to loss of higher level control, and that the most common cause of this loss of control is emotional in origin. But considerable controversy exists as to the manner in which this operates in the production of the symptoms. Let us then consider this point in the light of the foregoing conclusions. Emotion having as its function the reinforcement of the 'interest' associated with an instinctive process, it is, as I have endeavoured to show, only natural that, when the issue of the impulse in satisfying response is continuously withheld while the stimulus remains active either in actuality, in memory, or in imagination, the result can only be injurious to the organism. In practice we find that the injury

[1] Babinski et Froment, *Hystérie, pithiatisme et troubles réflexes*, Paris, 1916.

[2] Quoted by Orr and Rows, *loc. cit.* 19.

[3] *Op. cit.* 351.

[4] *Proc. Roy. Soc. Med.* 1919, XII.

[5] "Sensation and the Cerebral Cortex," *Brain*, 1918, XLI. 177–179.

manifests itself by disturbances of function in one or more of the following directions—psychic, somatic or visceral.

As already mentioned, the unprecedented conditions of modern warfare are just such as are calculated to arouse intense emotion by withholding satisfaction from the instinctive processes so violently stimulated. The emotional tension, however, continues to seek an outlet; and this being denied to it in a normally satisfying direction, the organism endeavours to escape, or to free itself from its unbearable condition by elimination of the disagreeable stimulus. This it attempts to do by withdrawing attention from the stimulus, *i.e.* from the object or idea, a process technically known as 'repression.' Unfortunately, perhaps, this seems seldom to be wholly successful; for when the focus of attention sweeps on to some associated object or idea the energy properly belonging to the original process, instead of disappearing, has a tendency to attach itself to the latter. Myers[1] gives a good example of this in a soldier in whom the original emotion had been aroused in connexion with a rifle, but by displacement of attention to a series of associated objects it spread first to the bayonet and hence to similar sharp weapons such as knives, until finally the patient was afraid even to use his razor.

It often happens that this process—known as displacement of the affect, but really due to displacement of attention—is arrested at some particular stage, thus giving rise to a simple phobia, of which the case just quoted is an excellent example; but it may continue to spread until a state of general anxiety is reached. In such cases it seems irrelevant what comes to be feared so long as something other than the original excitant of the emotion is feared. The less conscious the source of the original emotion, the greater is the tendency to dissociation. In the war neuroses the original source of emotion is rarely completely unconscious, hence the well-recognised ease with which the soldier can be 'cured' as compared with civilian cases.

When, then, through the mechanism just described, the pent-up emotion finds a vent at the psychic level, it produces either a simple phobia (or some analogous symptom) or a general anxiety state. If, however, the outlet is somatic, the symptom manifests itself as a bodily process, termed by Freud[2] a 'conversion hysteria.' MacCurdy[3] defines this as "a neurosis in which there is an alteration or dissociation of consciousness regarding some physical function." The symptomatology of

[1] *Present-Day Applications of Psychology*, London, 1918.
[2] Breuer and Freud, *Studien über Hysterie*, Wien, 1895.
[3] *War Neuroses*, Cambridge, 1918, 87.

this group is infinitely varied, but practically all the symptoms are such as will secure the patient relief from the conditions which brought about the emotional stress on which they indirectly depend.

In the group of cases now under consideration we are confronted with dissolution, the reverse process to evolution. The most recently acquired and most complex integrations are the first to be abolished. Loss of control beginning at the highest levels spreads down by gradations, each of which represents a successive step in evolution. The variety of conversion symptoms seems unlimited, but the essential mechanism of them all, whether they take the form of aphonia, stammering, heart-neurosis, or the more gross contractures and paralyses of the limbs, is explicable upon the same fundamental basis. There has been an overflow of energy which has abolished higher control, has disturbed coordination and the balance of reciprocal innervation, and has forced open unusual and undesirable final common paths.

8. Myers[1] drew early attention to the fact that, while conversion hysteria is most common in private soldiers and N.C.O.'s, the anxiety states are met with chiefly in officers.

MacCurdy[2] and Rivers[3] have laid later emphasis upon this and have attributed it partly to the general difference in early education, partly to the difference in the character and effects of military training and duties. Anxiety states may be regarded as arising at a higher mental level than does conversion hysteria. The higher ideals and greater responsibility of an officer cause him to repress all manifestations of fear and to endeavour so to act before his men as to set them an example. The fear which originally arose in connexion with the instinct of self-preservation thus becomes displaced and attached to the idea of his responsibility towards others; its original cause becoming *bewusstseinsunfähig*. Finally it may come to manifest itself as a general anxiety state.

In the private soldier the conflict between self-preservation and those forces which have been summed up under the term 'herd instinct[4]' is, as a rule, fought out at a lower mental level and the tendency is for somatic rather than psychic symptoms to develop.

The functional level at which dissociation may take place extends as we have seen even to that at which visceral and sympathetic symptoms make their appearance, though Babinski[5] has pleaded that in such cases—a full consideration of which would lead us too far afield—the

[1] *Lancet*, 9 Sept. 1916. [2] *Op. cit.* Chapter x.

[3] "War Neurosis and Military Training," *Ment. Hyg.* 1918. ii.

[4] W. Trotter, *Instinct of the Herd in Peace and War*, London, 1915. [5] *Op. cit.*

trouble is of peripheral reflex origin. There is little to support Babinski's contention, while apart from its intrinsic improbability Roussy and others have adduced evidence which strongly opposes it. I prefer then to regard the visceral and sympathetic symptoms met with in certain of the psychoneuroses as indicating that dissociation has, in these instances, taken place at a lower functional level.

9. We are not unfamiliar in civil practice with the onset of certain morbid conditions as a result of severe emotional shock, and Crile[1] has drawn attention to them under the head of 'kinetic diseases.' Mott[2] notes that about 10 % of the cases of severe war neuroses under his care showed symptoms of Graves's Disease. It would seem that when excessive disturbance has carried dissociation to this level the resultant disorder is usually much more difficult to bring under control again. I have seen, in the Special Military Neurological Hospitals, cases apparently identical with milder forms of Graves's Disease persisting for months. Yet psycho-therapeutic treatment, though slow, seemed beneficial. We must, however, reckon with the possibility that a severe nervous or emotional shock resulting in dissociation which manifests itself at this level may be capable of causing a permanent disorder in tissues normally under sympathetic influence.

That a delicate balance exists between the functions of the various endocrine glands is well known though imperfectly understood[3]. Crile asserts that there is a chemical interaction between the adrenals, the thyroid, and the brain. In this way he brings together the theories of nervous and humoral repercussion.

When suitable response to the stimulus can be made and vent thus afforded to the emotional and physical tension aroused, this two-fold repercussion would, on the lines previously discussed, be of great advantage to the organism. But the result of excessive outpouring of chemical excitants where no opportunity for their use is given might be expected to damage not only the glands themselves, but other tissues also. It is along these lines that we may profitably look for an explanation of some of the more obscure disorders to which violent and prolonged emotion gives rise.

10. With shock of commotional origin I am not here concerned[4],

[1] *The Kinetic Drive*, 1916.

[2] "Discussion on War Neuroses," *Brit. Med. Journ.* 2 April, 1919, 441.

[3] W. Falta, *The Ductless Glandular Diseases*, Trans. by Meyers, 2nd edit., Philadelphia, 1916.

[4] I have discussed this question in an article entitled "Some Biological Effects due to High Explosives," *Proc. Roy. Soc. Med.* 1919, XII.

neither do I propose to deal with predisposing causes such as 'mental make-up' or fatigue, important though the latter be.

Turning now to the last aspect of our subject, and assuming with Lloyd Morgan[1] that "the primary object and purpose of consciousness is control," and with Head and Holmes[2] that "the aim of human evolution is the domination of feeling and instinct by discriminative mental activities," it remains to find a means by which this control may be achieved. Fortunately we find the means of control to be implicit in the very mechanisms which, if the foregoing conclusions be substantially correct, are operative in the production of emotional disorders.

Brief though the survey of so wide a subject has necessarily been, we have seen that the dynamic trend of the individual is evolved through and continues to manifest itself at successive functional levels: first, the level to which belong, for example, the diffuse chemically-conditioned reflexes of the endocrine glands; secondly, the sensori-motor or spinal (including lower brain) level, through which we can trace the integration of simple reflex actions gradually culminating in complicated instinctive processes; thirdly, the psychic or cortical level, which is built up on the lower and exercises a controlling and inhibiting influence upon them. At the last level consciousness is present which, in the words of Bergson[3], "signifies hesitation or choice." And it is in connexion with the check upon primitive responses, imposed by consciously directed action, that emotion properly so-called arises. We have also seen that, when the conative tendency is thwarted or repressed, the emotional component of the instinctive process tends to find expression by attaching itself to associated ideas in order that a common outlet may be found. That is to say, the symptoms are the result of a compromise between two conflicting forces,—the instinctive impulse which is striving for gratification along primitive but now unacceptable paths, and the higher level control which blocks these paths.

11. Now if the selection of the associated idea be left to chance it is more than likely that the compromise outlet will be an undesirable one. Often an apparently new association will be found to be dependent upon some old disability. Thus in a soldier who in earlier life suffered from wrist-drop, any wound of the arm associated with conditions capable of producing emotional shock will be liable to re-establish the old disability as a functional wrist-drop. When careful histories are taken innumerable instances of this sort are discovered. Again the mere fact

[1] *Introduction to Comparative Psychology*, 1894. [2] *Op. cit.*

[3] H. Bergson, *Creative Evolution* (Eng. trans.), 152.

of having seen some comrade suffering from a particular morbid process may induce the patient to imagine that his condition is of the same order. I remember an officer who had seen a friend dying from cerebro-spinal meningitis develop later, when worn out by physical and mental fatigue, the signs of that disease including marked retraction of the head, which however all disappeared when he was reassured that he was only run down. It is common to speak of symptoms arising in this way as due to suggestion, but I cannot see what use is served by the introduction of this already vague term. In any case we are only dealing here with an exaggeration of a normal mechanism[1].

12. The apparently chance selection of the associated idea, which serves to determine the final common path in cases of emotional overflow, is, however, amenable to control in the following way. If a suitable and generally acceptable psychical equivalent is found, and by education is deliberately substituted for the primitive instinctive tendency, the energy of the instinct and its reinforcing emotion can be diverted into this channel along which its discharge is then beneficial. Such a process is termed 'sublimation.'

Freud has shown[2] that most of our social activities are sublimated forms of instinctive tendencies the direct expression of which would be unacceptable to our present standard of civilisation. In the past this has come about unconsciously or unwittingly, but now that we are in a position to analyse, and in part to understand, the process involved, there is no reason why education, in the broadest sense of the word, should not consciously be directed to the more complete attainment of this goal. The desired result is, as has been shown, not to be achieved by efforts to suppress the energy attached to instinctive processes, but by a utilisation of this very energy in directions which, under present social conditions, are either harmless or of positive value. Unfortunately it does not seem possible to divert this energy into any given channel at will. Experience shows that the new channel must be psychically equivalent to the old; that is to say the original and the new outlets must bear some relation, either conscious or unconscious, to one another in order that the discharge along the new path may afford, to the individual, a satisfying reaction.

The value of sport has long been recognised in the curriculum of boys' schools and in the training of adults. Looked at from the point of view just advanced, its mode of operation is obvious. In this instance

[1] A. Binet, *Les altérations de la personnalité*, Paris, 89.
[2] *Drei Abhandlungen zur Sexualtheorie*, Wien, 1905.

the energy of the primitive combative or aggressive instincts is afforded a similar outlet. A further step is taken when the combative instinct is given a purely mental outlet, as in debate. One may say, as a general rule, that the more the new path resembles the old the more easily is sublimation effected[1] and the less liable is it to break down. Thus an important football match is less likely to be 'foul' than an important debate to be acrimonious and vituperative.

The process of sublimation only occurs at a great expense of energy, and if for any reason the resistance of the new path becomes too high the energy reverts to a more primitive channel, a regression which is a constant feature in the neuroses. In our daily lives we derive the energy for our individual struggle for success from instinctive sources, and the degree to which each of us observes fair play and succeeds in maintaining the struggle depends partly upon his upbringing or training and partly upon whether the more refined means now at his disposal are really satisfactory psychical equivalents for the cruder and more primitive methods of expression.

An amusing outcome of sublimation is afforded by the pacifist movement, for the pacifist in waging war against war is drawing the energy for his campaign from the self-same source—the combative instinct—which originally led to an exactly opposite result. As the conditions of modern warfare fail to afford a satisfactory outlet for the primitive instinct, its sublimation is facilitated, and signs are not wanting that the pacifist eventually will emerge victorious. If his victory is to result in a permanent peace, some means other than war must be found along which international rivalry can discharge itself. Many such suggest themselves[2], but it remains to be seen whether they will prove satisfactory psychical equivalents for the original reaction.

13. Throughout this paper the illustrations and deductions have been drawn from military life because here we are confronted with the basal facts in a much cruder form than they are usually to be met with in civil life, and their analysis becomes so much the easier.

Unless the principles which arise out of a consideration of the problem of the war neuroses hold good over wider fields of knowledge they can be but partial expressions of the truth. But in my experience the principles deduced above do hold good over the whole field of so-called functional or psychogenic disorders. In spite of many complications we

[1] At the same time the two paths must not be practically identical, for in this event no true sublimation is possible.

[2] Cf. W. James's *Moral Equivalents of War*.

can disclose the conflict which has resulted in the repression and dissociation of one set of antagonists—that set which appears to the ego as unbearable—and has displaced the energy properly belonging to the latter along channels of less resistance onto some associated idea or action.

The further step of tracing back, by psychological analysis, these associations to their roots is too big a task to be attempted here, but from the therapeutic point of view its importance must not be overlooked.

14. The following conclusions have been reached:

(1) Emotion is only one aspect of the internal adjustment which an organism makes in order more completely to adapt itself to sudden changes in its environment. The function of emotion is to reinforce the 'interest' of an instinct as higher control over mechanically-fatal massive responses develops and checks the latter.

(2) The visceral and somatic concomitants of emotion are not responsible for originating the affective state, but are anticipatory physical adjustments which enable the organism to put forth all its energy effectively to satisfy the instinctive process stimulated.

(3) The optic thalamus is the centre of consciousness of the emotional state. Its activity is normally held in control by discriminative activities arising in the cerebral cortex.

(4) Dissociation is merely the obverse side of integration, and may take place at any level at which, phylogenetically or ontogenetically, integration has been brought about. The latest acquired and most complicated integrations are the most easily dissociated.

(5) The energy of an instinctive process can find outlet along psychically equivalent paths, but attempts entirely to thwart satisfaction lead to apparently fortuitous 'displacement of the affect' and its attachment to associated ideas, and they are only too likely to result in manifestations comprehensively termed psychoneurotic. Ultimately then our problem resolves itself into the finding of useful psychical equivalents and the inculcation of these as desiderata. The earlier in the life of the individual education along such lines is begun, the easier will be the process and the stronger and the more permanent the result.

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THE RELATION BETWEEN THE WORD AND THE UNCONSCIOUS.

By JOSHUA C. GREGORY.

1. *Intuition and perception.*
2. *Intuition and the unconscious.*
3. *The unconscious not a duplicate of the conscious.*
4. *The inadequacy of the 'stage and actors' metaphor.*
5. *The relation of the conscious and unconscious in image and meaning.*
6. *The relation in memory.*
7. *Words in relation to meaning: meaning as unconscious reaction.*
8. *Fallacy in the notion of 'signs' and 'marks.'*
9. *The word as a directive stimulus to the unconscious.*
10. *Suggestive signs not meaningless.*
11. *The distribution of attention to words and meanings: the relation of signs to the unconscious.*

1. "There is...a sagacity which...does not wait for the slow process of deduction, but goes at once, by what appears a kind of intuition, to the conclusion." Hazlitt adds this sentence from Sir Joshua Reynolds to his own statement that "In art, in taste, in life, in speech you decide from feeling, and not from reason; that is, from the impression of a number of things on the mind, which impression is true and well-founded, though you may not be able to analyse or account for it in the several particulars." "Common Sense," he adds, "is the just result of the sum-total of such unconscious impressions in the ordinary occurrences of life, as they are treasured up in the memory, and called out by the occasion[1]." All mental process tends to assume the form of intuition by condensing into one moment of apparently unsupported insight the effects of experiences dispersed through the mind's past life or of deductions once deliberately and consciously drawn from such experiences. External perception shows this very clearly. The mind *sees* that a stone is hard, though hardness, as such, can affect only the sense of touch. This visual intuition or insight is really supported by memories of contacts with

[1] *Table-Talk*, Essay 4: "On Genius and Common Sense."

objects that looked like the stone and by memories of sensations received from these objects when they were touched or handled. Visual perception becomes intuitional by dropping all conscious reference to experiences of touching hard objects, and instantaneously inferring their hardness from their appearance to the eye.

This acquired intuitional insight "does not wait for the slow process of deduction," with its labour of recalling past occasions when objects that looked like the stone were discovered to be hard by touching them, and with its effort of inference from these memories of the past to the present situation. It accepts the risk of illusion at the price of increased rapidity. Cardboard, shaped and painted like rock, can deceive the eye. This danger of error pervades all varieties of intuition since it necessarily arises whenever an index, like the index of hardness provided by the look of the stone, is assumed to be infallibly connected with the qualities it is employed to indicate. This assumption of invariable connexion becomes particularly precarious when the mind steps outside the ordinary run of its experience: a Highlander, accustomed only to the rocks of his native mountains, might be misled if suddenly placed among the mimic rocks of a city theatre. Protective mimicry in the animal world probably exploits the possibility of error accepted by perception in following its intuitional habit. Birds avoid many insects that sting or are unpalatable, by accepting their gaudy garb as an index of disagreeableness; insects that are really toothsome morsels are left alone if they adopt the same gaudy garb. The intuitional method is the very soul of mental perception, which employs impressions from the external world as hints, transforming them so rapidly and spontaneously into deductions that the mind has to reflect carefully to become aware that they are deductions. Hardness is inferred, not seen; the whistle is heard but the train is deduced; "I came home to my fortification," Crusoe wrote of himself after he had seen the footprint in the sand, "not feeling, as we say, the ground I went on, but terrified to the last degree, looking behind me at every two or three steps, mistaking every bush and tree, and fancying every stump at a distance to be a man." Crusoe had perceived a footprint and *seen* that savages had landed on his island.

2. It seems quite evident that a conscious perception or a conscious idea of any kind is often, if not always, the conclusion of an unconscious mental process. It is a terminus, the momentary ending of a process sustained by the accumulated and organized impress of past experiences on the mind. The mind's tendency to work under the form of intuition depends on its innate habit of using past experiences in an unconscious

way to produce a conscious result, it is, in fact, the natural result of this process. This unconscious working is observed to pervade all mental process the more widely and deeply, the more attentively that mental process is studied. *Seeing* the hardness of stone can be regarded as the conscious result of a rapid, unconscious summary of past occasions when sensations of hardness were consciously associated with certain visual appearances. To suppose that this *conscious* association, equivalent to a deliberate inference that a stony look means a hard object, ever existed is probably to attribute to the conscious what belonged to the unconscious: consciousness surely accepted the stony appearance as an index of hardness in an automatic or unconscious way. Intuitions or ideas consciously *received* into the mind seem often to be "the just result of the sum-total of such unconscious impressions in the ordinary occurrences of life." Hazlitt, in the same essay from which this quotation is taken, describes how a man who had been arraigned in 1794 on a charge of high treason, retired to Wales to write an epic poem. One morning, as he sat at breakfast, a man passed his window. He was instantly haunted by visions of prison, trial and the gallows. He did not recognise the man, but he had actually seen the face of Taylor the Spy. Unconsciously received impressions thrust into his mind the fear that he had been marked down.

3. Because the unconscious workings that terminate in conscious processes cannot be seen or touched or known by direct apprehension, they can be, and often have been, denied. They are deduced because they provide a meeting-place for the convergence of lines of inference that would otherwise hang out, so to speak, in empty space. Bleuler[1], summarising some of these lines of inference, remarks that walking is directed by sensations, by sensations from contacts between feet and ground, by sensations constantly proceeding from moving muscles, tendons, joints etc., that do not press into consciousness, though a trip or a stumble may suddenly remind the walker that he is depending on them. If the walker be immersed in a problem he may thread his way, even change his route, without being consciously aware of the impressions of sight, touch or sound that are guiding him, which might have been consciously received and reacted to if his attention had been less distracted. Impressions that would have been conscious normally act in an unconsciously mental way. He may be heedless, adds Bleuler, during his walk, of some incident that afterwards appears suddenly in his conscious memory. A girl under hypnosis repeats an Arabic dedication which

[1] *Studies in Word-Association*, VI.: "Consciousness and Association."

she neither understood nor remembered on waking. Past scenes, past thoughts, past dreams can remain outside consciousness and, after many years, re-enter it: Merz[1] remarks that memory *compels* an account with the unconscious. Recollection and reminiscence are constant reminders that past experience is constantly in unconscious, or subconscious, operation; and (as will shortly appear) memory both points a steady finger in the direction of the unconscious and suggests false descriptions of its nature.

This combination of correct deduction with false description may have prompted those flat denials that have been, and still are, given to the suggestion that mental process may be unconscious. Bleuler, referring to those who consider it nonsense to speak of *unconscious* mental process, compares the psychologist who confines the mental to the conscious to the biologist who would only study the terrestrial life of amphibia. A belief, of course, is not proved absurd by transforming it into a metaphor. He draws a reasonable inference from the apparent convergence of lines of evidence on unconscious mental process when he affirms that there are some functional formations and mechanisms outside as inside consciousness and as capable of determining the psyche. In his *Psychological Principles*, Dr James Ward implicates the conceptions of the unconscious and the subconscious in the concept of the 'field of consciousness.' At the focus of the field, to which the eye of attention is directed, consciousness is at a maximum: Archimedes is absorbed in his figures on the sand. The 'din of the siege' falls vaguely in the outskirts of consciousness, consciousness falls steadily towards a minimum from the centre of the field to the periphery. The law of continuity naturally suggests that beyond this periphery lies a "volume of subconscious experience supporting the field of consciousness and vitally continuous" with it. Dr Ward distinguishes between the unconscious and the subconscious: "We cannot fix the limit at which the subconscious becomes the unconscious." Bleuler, conveniently and reasonably, even if Dr Ward's triple distinction into conscious, subconscious and unconscious be finally allowed, directly opposes the conscious and the unconscious; but he proceeds too hastily when he adds that there are in this sense (referring to unconscious functional formations and mechanisms) sensations *only* distinguishable from similarly named conscious phenomena by the absence of conscious quality. In insinuating this error, memory is like a pretended deserter who truthfully tells his captors that their enemy has lines in front of them and falsely describes their dispositions.

[1] *A History of European Thought in the Nineteenth Century*, 1912, III. 289.

Memory says truthfully that there is an unconscious mind; it insinuates a very false description of it by suggesting that there is an unconscious duplicate of conscious life.

A chemist recognises the presence of sulphuric acid by obtaining a fine, white precipitate when he adds to the suspected liquid, acidified with hydrochloric acid, a solution of barium chloride. He learns to draw this inference by observing that he obtains his precipitate when sulphuric acid is known to be present and does not obtain it when sulphuric acid is known to be absent. His process of inference rapidly becomes an intuitive connexion between the appearance of the precipitate and the presence of sulphuric acid: he ceases to think consciously of former occasions when he applied the test. His establishment of the precipitate as an index of sulphuric acid contains, like all intuitive processes, a risk of error: sulphuric acid may not be, and is not, the only substance that responds to his test. Memory, recollection, enables him to retain a critical control over his intuitive process. His past experience, acting unconsciously, determines his conscious inference that sulphuric acid is present when he obtains his precipitate; but he can, if he so desire, recall past experiments—recollecting the circumstances under which they were successful or that certain substances, which might now be interfering, were absent in them. Since he can recall past experiences into consciousness it is quite natural to assume that when they unconsciously determine his conscious procedure they exist just as they were first experienced, or just as they were remembered, only unrecalled. All recollection tends to deceive in this way. A man is powerfully influenced by certain thoughts that come into his mind. For weeks, for months, for years these thoughts control his actions or direct all his thinking without coming into consciousness as actual memories. Then he suddenly recollects these thoughts and realises how they have determined his life. He naturally supposes that they have, *as such*, lain in his unconscious mind and *as such* unconsciously influenced him. This illusion is probably assisted by an analogy unconsciously adopted from perception of the external world. We think of a mountain that we have seen and left as existing just as it was and is till we happen to see it again. In the same way we think of the memory of the mountain as permanently lodged in our mind, always the same and merely coming under attention when we think of it at some odd moment. The external world thus becomes the pattern of the world of ideas. Just as material objects remain after we have perceived them for us to perceive again, so perceptions that we have once experienced or ideas that have once entered our consciousness remain, or may remain, in our minds till we again notice them or until they force themselves again upon our conscious attention. This analogy more explicitly applied determines such statements as Bleuler's that there are sensations *only* differing from their conscious equivalents by being unconscious.

4. Consciousness is determined by the unconscious when a thought, after being consciously received, continues to affect the processes of the mind without being explicitly thought or remembered. Its unconsciously exercised influence results from its first conscious reception, so that the unconscious, in its turn, is determined by the conscious. This mutual relation of determination between conscious and unconscious must be clearly recognised in all attempts to conceive the connexion between them: it serves also as a first effort to conceive the character of the unconscious. When an incident is seen and subsequently remembered, nothing more is implied than that conscious perception so affects unconscious process that it may result, at some future date, in a conscious reference to the original perception. Recollections of past incidents usually do only appeal to us as references to the original perceptions; when they succeed in posing as the perceptions themselves they are described as illusions or hallucinations. Thus even recollection, which does insinuate that ideas pass to and fro between unconscious and conscious, as external objects pass into and out of attention, is unable entirely to conceal the difference in nature between conscious and unconscious. Language, constantly wedded to meanings it has acquired through its application to external objects, tends constantly to back the insinuations of recollection against its failure to justify them. It is very natural to speak, with Hazlitt, of impressions being "treasured up in the memory" and of their being "called out by the occasion." Such phrases are inevitable, they may even be said to give a welcome concreteness to expression, but they do, through their habitual associations, convey the suggestion that consciousness is like a stage and ideas like actors that enter and leave it: actors in the play resembling ideas in consciousness and the same actors out of the play resembling the same ideas in the unconscious.

This conception, prompted by recollection and suggested by language, of the unconscious mind as a store, is a convenient working concept; but is a misleading notion if persisted in as a true or final account. It will obviously be difficult to frame an adequate concept for the unconscious. It is difficult to secure an adequate conception of the externa- world that can be apprehended by many minds in common. It is more difficult still to conceive adequately of the individual mind whose conscious ideas can only be directly apprehended by itself. The conscious thoughts of an individual can, however, be laid directly open to inspection by expression in speech or writing or in significant movement. Conscious thought, indirectly, partially and fitfully becoming part of a world common to many minds, appears to be intermediate between the material world that lies freely open to all and the unconscious minds that lie outside even their own consciousnesses. The use of the 'stage and actors' metaphor to represent the relation between conscious and unconscious gives an initial mental grip upon the latter. Preliminary working concepts fulfil one function by giving this initial mental grip; they have a further function in rousing a sense of their own insufficiency. Simple persistence in regarding conscious ideas as constant permanent entities that pass in and out of consciousness, as birds fly in and out of a nest, soon directs thought to think of the unconscious, more vaguely perhaps, certainly less picturesquely, though with less error, as mental process determining the conscious, and, in turn, determined by it.

5. Since the conscious and the unconscious are everywhere in mutual determination, their relation can be studied at any psychical point. It can be studied in the structure and movement of the concept or idea—more particularly in the relation between image and meaning. This relation between image and meaning reiterates the intimation given by memory that unconsciousness underlies consciousness and repeats the error of recollection in insinuating that unconscious ideas are simply conscious ideas unattended to or deprived of conscious quality. More correctly, one aspect of the intimations of memory is presented in the meaning-image relation. When the idea of a 'horse' is aroused in the mind, usually as part of a wider context such as 'the horse will gradually become extinct as motor travel and traction develop,' it has a very small proportionate representation in consciousness. The word 'horse,' either as a visual image corresponding to the printed word or as an auditory image corresponding to the sound of the word spoken, may be the sole conscious aspect of the idea that is certainly wider than the mere word visualised or heard. Faint mental pictures of a horse and various 'kinaesthetic' images can be disregarded since, if they should be present, they only make the conscious imagery a little more expanded and complex: the visual image of the word h-o-r-s-e can be used to denote the whole imagery, actual or possible, of the idea. The printed word h-o-r-s-e has no significance for one ignorant of its meaning, neither has the mental image, as such, any significance either. The whole idea con-

sists of the visual image of the word, presented in consciousness, and the meaning it subserves. This meaning, stating an antithesis that is not too sharp for adequate analysis though it may not be quite so sharp in reality, falls completely within the unconscious. An attempt to explain the word 'horse' or to discover to ourselves its complete meaning shows this clearly. When we read or hear 'the horse etc....' we do not consciously think of a certain shape or size, or of eating corn or of drawing guns or of prehistoric ancestry or of all the many items that rise into consciousness when we explain or analyse out what 'horse' means. All this meaning, we thus realise, has operated unconsciously on the process of our thought through the visual image 'horse.' This analytic process, during its revelation of the unconscious character of 'meaning,' induces us to believe that our conscious recollections are lifted as such out of the unconscious region of meaning into the region of consciousness. It appears to us as though the visual image 'horse' called upon the recollection of 'drawing guns' and upon all other recollections associated with it in the meaning of the image to stand to attention. If the sentence run 'the horse is used for drawing guns,' one of these recollections, roused by the word horse to readiness to enter consciousness, does so enter; if the sentence run 'the horse eats oats,' another recollection, also roused to readiness, takes its cue and takes its turn in consciousness, as an actor treads the stage when his call comes.

6. Memory, in the widest sense of the term, is the organized disposition, or dispositions, to react, impressed upon the organism by its experience. This organization of dispositions depends upon the original qualifications, or capacities, of the organism and upon the stimuli it receives from its environment. Experience, in the wide sense of interaction between stimuli and organism and the results of this interaction in impressed dispositions, continuously enlarges or modifies capacity. The domain of stimuli, or of possible stimuli, increases with this alteration of capacity, for stimuli only become stimuli when they meet with an ability to receive them. An eyeless animal, for instance, or an infant before its experience entitles it to *see*, cannot perceive form or colour, though the same light can stimulate a trained human eye to perceive them. The four closely related terms 'memory,' 'disposition,' 'experience' and 'reaction,' vary together in meaning as the meaning of one of them is narrowed or widened; they also impose, as they expand or contract in meaning, a corresponding variation in the phrase 'qualifications or capacities of the organism.' 'Organic memory' implies (or may imply) organized dispositions to reactions impressed by interactions between

the organism and stimuli to which it is competent to respond in a total process of wholly unconscious experience.

'Memory' is usually restricted to a process formally similar to the above but including consciousness. The sight of a lyre recalls the person who last played upon it and the tune he played: the mind reacts consciously to a stimulus consciously received because conscious experience has impressed a disposition upon it to react in this conscious way. The essence of memory is disposition to react, organized through experience, whether that reaction be expressed in unconscious movement or in conscious reference to the past. Memory tends to be identified with conscious reference to past experience because, just because it is conscious, this conscious reference is striking and arrestive. Memories, conscious mental processes, are naturally accorded the rôle of constituent elements, regarded as composing memory as a whole and conceived away from their real nature of conscious reaction to appropriate stimulation of the organized disposition to react which constitutes memory as a whole. In its most explicit form, a memory recalls a past event with its date attached: a cyclist remembers, for instance, that he was caught in a storm last Tuesday. The dating is often less explicit or even absent, as in remembering that one had been ill after eating strawberries without specifically recollecting the time and place. These conscious references to the past are mental reactions depending on an organized disposition impressed by experience: the cyclist's wetting impresses on his mind a tendency to the reaction of consciously referring to his experience of getting wet. The reaction induced by memory usually involves no conscious reference to the past—we catch trains, put on our clothes, deduce causes from effects or effects from causes without, in any definitely conscious way, *remembering* the past experiences that determine our actions or thoughts. Underneath all conscious mental process and vitally continuous with it, lies a volume of unconscious process that takes its course and determines conscious reaction through its organization into dispositions during the individual's experience.

7. This unconscious process is as integral a part of any thought, idea or perception as the part that we realise consciously. The relation of the word to its meaning, with which this article is most particularly concerned, is, it may be suggested, appropriately represented by the relation between stimulus and stimulated process. Words have meanings because they set the mind going in a determinate way or because they strike into mental process already in being to direct its course. The concrete reality of mental life constantly escapes out of the sharply de-

fined concepts or logical distinctions employed to interpret it. The word may appear in the conscious region as a part of the total mental process or reaction: the sight of a horse, for example, may flash upon consciousness the word used to designate the animal. The extension of speech and writing among civilised peoples continuously extends and expands the functions of words in all mental process and justifies the concept of the word as a stimulus to total mental reaction. This conception of the word as a consciously applied stimulus and of its meaning as an unconscious reaction determined by an organized disposition impressed by experience seems to give a more adequate mental grip on the nature of ideas and their passage through the mind than the notion involved in the terms 'sign,' 'symbol,' or 'mark' that have so often been applied to words or names. According to this conception, an idea, of a horse, for example, is a sectional part of a mental process that occurs, to draw a distinction quite sharply for analytical purposes, quite unconsciously, according to a disposition organized by experience and directed by a word as a conscious stimulus. The word is not the only image that can provide the stimulus for the process of the idea: a visual image of a horse can replace the word 'horse'; but it is with words that this article is concerned.

8. Professor Bosanquet virtually equated 'word' with 'stimulus' when he described (in his *Logic*[1]) a name as a sign that rouses the mind to a set of activities with an identical element. Hobbes realised that the name, which he defined as "a word taken at pleasure to serve for a mark, which may raise in our mind a thought like to some thought we had before, and which being pronounced to others, may be to them a sign of what thought the speaker had or had not before his mind," operated as a stimulus, for he observes that 'marks,' which include words, *recall* thoughts "by the sense of them[2]." The terminology of 'signs' and 'marks,' however, tends to imply the erroneous notion that ideas exist, as such, in the mind in unconscious sojourn and are simply 'recalled' into consciousness by the words that remind the experient of them. Hobbes compares 'arbitrary' signs to the bush formerly hung up before a wine-shop to intimate that wine could be bought there. The bush and the wine-shop are perpetually joined because they are always there together. This naturally suggests that the idea, as also perhaps the word representing it, continuously exists as such—its existence nowadays being specifically referred to the unconscious. The same tendency to error is manifest in Locke's remark that "the use of words" is "to stand

[1] *Logic*, 1888, I. 13.
[2] *Elements of Philosophy*, pt. 1, ch. ii.

as outward marks of our internal ideas[1]." Something permanent is implied in the permanence of meaning, but the permanent element is a disposition organized by experience for the mind to react in the form known as an idea to the stimulus applied by the word; it is not, such at all events is the view here maintained, a permanent existence of ideas that sojourn in the unconscious from whence they rise from time to time into the focus of consciousness or into its outskirts.

If the essence of mental life be process, it is difficult to conceive how a present idea, which is a particular movement in the mind, could be lodged continuously in the unconscious while other ideas, mental processes in their turn, are in progress. Psychical dispositions no doubt imply psychical structure, for function and process involve organs and arrangement of parts, but the structure developed in the mind growing under experience is not the simple absorption of sensations, perceptions or ideas, as they consciously occurred, and their introduction into it as bricks are built into a house. "Nevertheless," writes Professor Ward in his *Psychological Principles*, "it may be urged, it is surely incredible that all the incidents of a long life-time and all the items of knowledge of a well-stored mind, that may possibly recur—'the infinitely greater part of our spiritual treasures,' as Hamilton said—are severally retained and continuously presented in the form and order in which they were originally experienced or acquired." Professor Ward adds that "This, however, is not implied." It is doubtful, however, how far his "psychological principles" desert the belief that conscious images, perceptions and the like persist as such to leave and enter consciousness. He observes, for example, that "the same image may figure in very various connections," implying that "incidents of a long life-time and all the items of a well-stored mind" may be simply re-ordered or rearranged without losing their permanent, substantial individuality. "Because of the manifold forms into which they may *evolve*," he continues later on, "subconscious images, while still *involved*, are sometimes called 'psychical' or more definitely 'presentational dispositions.' The word *disposition* means primarily an arrangement...." He continues: "These dispositions are processes or functions more or less inhibited, and the inhibition is determined by their relation to other psychical processes or functions," a sentence which suggests that the organized dispositions to react, impressed by the process of experience upon the mind, do not imply that percepts and ideas are individually retained in the unconscious to rise from time to time into consciousness. When disposition is referred

[1] Essay 2, II. 9.

primarily to arrangement this is precisely what is suggested. Professor Ward's preference for the term 'subconscious,' which he seems to regard as equivalent to greatly reduced attention, together with his division of the mind into presentations and the subject to which they are presented, seems to incline towards the conception of Bleuler that a sensation may exist unconsciously exactly as it occurred consciously, except for its deprivation of conscious quality.

9. Professor Ward's way of thinking seems on the whole to favour the belief that when the visual image of a horse appears in a mind that has perceived the written word h-o-r-s-e, it simply moves from under a subconsciously attenuated attention to a position where it receives focal or concentrated attention. Whether the movement pertains to the image or to attention itself, or whether it be a joint relation that may conveniently be spoken of either as a movement of attention to image or as a movement of image under attention, does not affect the point here emphasized, the point, namely, that the image as it appears and appeared in consciousness is a permanent psychical lodger. There is a difference between this conception and supposing that the visual experiences of horses and the ideas consciously entertained regarding them impress a disposition upon the mind, organized in the region of unconscious memory, to react to the stimulus of the word by flashing the visual image upon consciousness. The hypothesis presented here is that the word is essentially a directive stimulus to which the organized dispositions within the unconscious respond by a definite process or reaction. This reaction may result in conscious process; it may be virtually completely unconscious, and it is always unconscious in part. When conscious processes do result, they are temporary presentations proceeding from a total process originating in organized dispositions: they are not passages, as when the visual image of a horse is received consciously, of permanent elements from the unconscious to the conscious. The recurrence in memory of an incident formerly perceived is a psychical reaction similar to, but by no means identical with, the psychical reaction in the original perception. The percept does not sojourn in the unconscious or remain under subconsciously attenuated attention; it impresses the mind and memory is organized into a disposition in the unconscious region to react under suitable stimulation by producing a conscious process that contains a reference to the original impression.

10. Words are frequently described as 'expressive signs' and contrasted with 'suggestive' and 'substitute' signs. "A word," writes

Professor Stout[1], "is an instrument for thinking about the meaning which it expresses, a substitute sign is a means of not thinking about the meaning which it symbolises." The symbols of algebra, arithmetic and formal logic are largely mere substitute signs that are substituted for their meanings, as counters may be substituted for coins during a game of cards, and operated upon according to fixed and definite rules derived from the nature of the things symbolised. Expressive signs, such as words, are used for the sake of their meaning, substitute signs are used instead of their meaning. There are also signs that are merely suggestive because they have no meaning. Professor Stout calls the shape of the knight in chess, or the chalk-mark on Ali Baba's door, "a mnemonic help," a device for calling up a certain idea which is then exclusively attended to, the suggestive sign being dropped when it has done its mnemonic duty. The extraordinary conclusion that a chalk-mark *made for a specific purpose* has no *meaning* indicates pretty plainly that the analysis is defective. John Stuart Mill[2] denied meaning to the proper name because it did no more than the chalk-mark, and it is obvious that the doctrine of 'suggestive signs,' as expounded by Stout, will naturally lead to this denial.

These distinctions between expressive, suggestive and substitute signs would not have been so sharply drawn if it had been realised that the 'sign' functions essentially as a directive stimulus. All signs direct mental process in a *definite* way—if they did not do this they would not be signs. Any object may suggest one thing to *A* at one moment, another thing at another moment and every individual person may receive from this same object a different suggestion. A cow may suggest either milk or a dairymaid to one person, an item of religion to an Egyptologist, the Indian Mutiny to an historian and a dozen other things besides. If the model of a cow were hung up over every dairy, it would be *tied* to the associated idea 'this is a dairy' and become a sign. It would then *mean* that milk was sold within, just as the bush, formerly hung over wine-shops, which might suggest any idea while on its native heath, acquired a definite meaning as a sign by becoming tied to the intimation that wine could be bought there. Where there is tied association there is meaning, and where there is determinate direction of mental process, which is the same thing under a different aspect, there is meaning also. A word has meaning because it stimulates organized psychical dispositions to determinate reaction; the shape of chessmen and the robber chief's chalk-mark have a precisely similar function. The substitute sign

[1] *Analytic Psychology*, 1896, II. 194. [2] *System of Logic*, 10th ed. 1879, I. 37.

also determines mental process in a determinate way, for it would be useless if it left the mind free to think exactly as it chose.

11. Many psychologists think like Professor Stout that the word "is an instrument for thinking about the meaning which it expresses." Professor Hoernle, in an article on Image, Idea and Meaning[1], says that the meaning normally occupies the focus of attention: the word is, of course, one species of image. But, if these two statements can be regarded as equivalents, do they not apply, if they apply at all, to the signs regarded as merely suggestive? 'The horse will gradually become extinct as motor travel and traction develop': does the string of words not run through the focus of attention, definitely stimulating and controlling a mental process that virtually constitutes a wholly unconscious process? If a definition of any word be demanded, attention may be said, in a sense, to turn towards its meaning. In defining the word 'horse,' for example, the dispositions unconsciously organized in memory respond to stimulation by processes that result in conscious realisations that a horse is an animal, that it eats oats, that it has four legs etc. Insistence on conscious realisation of the total unconscious psychical dispositions involved in the comprehensive idea of a horse would detain the mind and prevent the continuous mental flow that responds to the whole sentence. It seems to be essentially true of the expressive word that it avoids the intrusion of meaning into consciousness by appropriating the rôle of a definitely directing stimulus on the whole mental process comprising unconscious working and consciously experienced image. Translated into terms of attention this means that it is the word, or image, that receives attention and not the meaning. We think with a word, when we have mastered its definition, without the constant obtrusion of that definition. The value of the word for thinking, privately indulged or publicly expressed, lies in the ease and effectiveness with which it guides the whole mental process without mental reactions of a conscious character. The 'suggestive sign' operates in a way very similar to the demand for a definition of the expressive sign or word. *A* writes a letter at night that he must remember to post in the morning. His desire produces a disposition to think of posting the letter and this disposition remains during the night in his unconscious mind. To stimulate this disposition into so reacting that he will consciously think of the act of posting, he ties a knot on his handkerchief. When he wakes in the morning his mental attitude to the knot may be said, without serious error, to be an attempt to define its meaning. The meaning of

[1] *Mind*, N.S. XVI. 82.

the knot is temporary, it may mean on the succeeding night that *A* must pay an account for his wife: this is no reason for denying meaning to the suggestive sign, though this temporariness of meaning probably induced Professor Whitehead[1] to oppose the knotted handkerchief, as a merely suggestive sign without meaning, to the word with a permanent meaning, as an expressive sign. A sign is a directive stimulus to mental process: it may, as an expressive word, stimulate a process that reacts unconsciously; it may, as a suggestive sign, stimulate organized psychical dispositions into producing a conscious reaction. A word, taken out of verbal context, may function similarly to a suggestive sign by stimulating to the conscious reactions involved in definition or actually become one by consciously referring the mind to the experiences implied in the organized mass of unconscious disposition that constitutes its meaning.

The 'substitute sign' stimulates now the mental process represented by the meaning it is to express when the final calculation is made and now the mental process whose meaning is connected with the mathematical operations to which it is submitted. But all signs, like all words, are stimuli directive of mental processes that proceed largely unconsciously, like the organized dispositions responsible for them, and these mental processes, or reactions, may, more or less, according to circumstances, be conscious operations.

[1] *A Treatise on Universal Algebra*, 1898, I. 3.

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THE RÔLE OF INTERFERENCE FACTORS IN PRODUCING CORRELATION.

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1. *Introduction.*

THE object of this paper is to make some advance in our knowledge of the significance of the correlation coefficient. In setting out the numerical results of psychological and pedagogical experiments this coefficient is so largely used, and by its aid conclusions are so freely suggested, that it becomes of increasing importance to examine with great caution the nature of the deductions which can be made from the magnitudes of the coefficient. While a study of correlation is essentially general to all scientific measurements, the particular problem before us here is the use of the coefficient in attempting some analysis of ability in mental processes. In one sense the significance of the correlation coefficient is very well understood, viz. in the sense of the definition of Galton. If an array of individuals is selected from the population, all having a certain value for one of two correlated variates, then the average value which these individuals show for the other variate will be 'r' times the former value, provided that all measurements are made from their respective means and in the appropriate units, which are their respective standard deviations. This Galtonian point of view therefore supplies us with a very definite and illuminating meaning for the correlation

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coefficient: if a large number of pairs of measurements are made for each of two variates (if, *e.g.*, the behaviour of a large number of individuals is recorded in response to two distinct mental tests), each individual thus giving a pair of measurements, we then have two arrays of values showing a more or less intimate relation, and the correlation coefficient is an exact, though not the only, measure of the intimacy of this relation. While the correlation may thus be clearly measured and expressed as a fraction, and while no doubt remains as to the meaning of the fraction as seen from the Galtonian point of view, yet the cause of that relation between the two arrays—which are the factors that decide, and in what way they inter-act to bring about that relation—is left at any rate without a direct answer. This is the ground which needs exploration, to assist the interpretation of psychological and other data. †

The two measured quantities are in general each of unknown complexity and we may speak as though some mechanism were at work producing the correlation. Consider, for example, two mental tests used by Burt[1]. (A) Tapping, with a blunt needle, and (B) dotting, using McDougall's dotting apparatus. The coefficient of correlation between A and B was ·48 with one group of children. According to a well-known theory, this result can be accounted for by assuming the presence of specific elements peculiar to tests A and B respectively, and, in addition, common elements operating similarly in each. There are many other possible explanations, however; one other in particular, which this paper investigates, is that a number of elements is conceivable which operate in favour of one test and against the other. A group of such elements is described in this paper as an 'interference factor'; it has been already foreshadowed by previous writers[2]. The difficulty of the first-mentioned theory is that we are compelled to assume a large number of specific elements to counteract the high correlating influence of the common elements, whereas, as will be shown, the assumption of an interference factor, helping one test while hindering the other, readily accounts for lowness of correlation between two very similar tests, without the assumption of numerous specific elements. It will also be shown that the assumption of interference may throw light on other psychological difficulties.

[1] This *Journal*, 1909, III. 132, 153.

[2] See page 91.

2. *Possible mechanisms producing correlation.*

What are the conceivable causes of correlation between two measurable phenomena? The most easily conceivable is that there are some actual causes which are the part causes of each of the phenomena. The simplest illustration of this is afforded by a generalised form of Weldon's experiment[1].

This is an easy dice experiment designed to give two arrays of numerical values, such as those referred to in the foregoing section, except that the mechanism of correlation is decided upon, and that the variations are known to be solely dependent upon the laws of chance as they apply to dice throws. For the sake of distinction let some dice be coloured red, the others being white. One pair of values, being respectively the measures of two correlated phenomena, is found as follows. Throw a number of dice, say l red dice and m white dice, and note the total score, which is one of the required values. Now let the l red dice remain untouched, remove the m white dice and throw in their place n white dice. Again take the total score of red and white dice, which is the other required value. Any number of pairs of values can be found in the same way, thus making two arrays of correlated numbers. Obviously the source of correlation lies in the fact that a certain number (l) of red dice is always a common factor in a pair of throws. The correlation coefficient r may be calculated from the 'product moment' formula, but it is possible to prove without performing the experiment at all that r may be calculated from the equation

$$r = \frac{l}{\sqrt{(m+l)(l+n)}} \quad \text{.......................(1).}$$

If the two groups of dice are numerically equal, *i.e.*, if $m = n$, then r is simply the proportion of red dice among the total number used. Or if we let the red dice alone form the second throw every time, then $n = 0$, and

$$r^2 = \frac{l}{m+l} \quad \text{...............................(2).}$$

The illustration treats the elemental causes, the single dice, as additive. It may be asked what would happen were they to combine in some other way, such as by multiplying. It can easily be deduced from Bravais that the results are the same, provided that the form of the function according to which the elements combine is the same in each variate, and the standard deviation of each element is the same.

[1] Thomson, This *Journal*, 1916, VIII. 274.

In the above instances the correlation has been due to actual overlapping, and many real cases of correlation may be due to such. But there are other forms of correlation conceivable in which the connexion is much less tangible. The best example on the same lines as that of overlapping dice throws is perhaps correlation in whist hands. At whist, the number of trumps in my hand is negatively correlated with those in my partner's; if I have many trumps, he has probably few, and *vice versa*. There is similarly a positive correlation, where the connexion is still less direct, between the number of cards of one suit in my hand and the number of cards of any other suit in my partner's. In these cases there is no clear physical overlap, nothing which can be visualised (like the red dice) as being the cause of the correlation. Instead of overlapping, we have here exclusion due to drawing from a limited pool. If I have many red cards dealt to me, there remain proportionally more black ones which may be dealt to my partner. There is a natural tendency to think of correlation in terms of overlapping factors instead of in terms of exclusion as in whist: but it must always be remembered that from the mere presence of correlation we cannot deduce that any such actual overlapping of the phenomena occurs.

In the present paper the simplest case only is to be considered, viz. that in which the correlations can all be considered to be formed in a way generalised from Weldon's experiment; that is to say, the variates are to be formed by the simple addition or subtraction of factors, and all the correlations are to be considered as due to the fact that some of the factors are added to, or subtracted from, more than one variate. Throughout it is necessary to keep in view this limitation, and to avoid any temptation to draw conclusions of too wide a nature. Other forms of correlation-producing mechanisms will be considered on some future occasion.

3. *Terminology.*

It appears necessary, in order to avoid misunderstanding, for us to state clearly the meaning of the terms 'specific,' 'group,' and 'general' as used in this paper. By general factor is meant a factor which is common to all the variates under consideration. A group factor is a factor common to some but not to all variates under consideration. The word 'specific' is used for a factor which occurs in only one variate among the variates considered. The question whether a factor is to be called specific, group or general depends upon the selection of variates for consideration. A particular factor may be specific in one selection of variates, *i.e.* operating in only one of them, but it can be a group factor if the selection of variates contains two or more variates to which this particular factor is common; and if such a selection of variates be made in which this particular factor is common to all, it can become a general factor.

The expression 'general factor' is sometimes used in a different sense[1] from the above, *i.e.* to signify a factor common to all possible mental operations, a meaning which is not intended in this paper. The word 'specific' is also used by some writers[2] to indicate a factor which is common to a limited number of variates. Such a factor would obviously be called a group factor in this paper. A further distinction in the use of the word general is given later (footnote, p. 93).

4. *Condition for the existence of general and specific factors in the case of three variates with positive correlation.*

The extension of Weldon's experiment to three variates[3] x, y, and z involves the use of seven groups of dice, viz. white dice which are common to all three variates; orange, green, and purple dice, which are common to x and y, y and z, z and x variates respectively; red, yellow, and blue dice which are peculiar to the x, y, and z variates respectively. Let the seven groups of dice be visualised as forming a pattern of seven cells, each cell containing all the dice of its proper colour, for instance W cell contains white dice, and so on. The cells are arranged for convenience as in Fig. 1. Let R, O, Y, G, etc. be the actual numbers of red, orange, yellow, green, etc. dice respectively.

$$\begin{array}{ccc} & R & \\ P & & O \\ & W & \\ B & & Y \\ & G & \end{array}$$

Fig. 1.

Instead of two arrays of numbers as in section 2, three arrays can now be made with overlapping correlations between any pair of the three. A trio of values for the three variates will be found as follows. Throw all the dice and note the total score of each group. The sum of the scores of the P, R, O, and W groups will give a value for the x variate; the sum

[1] Cf. Hart and Spearman, This *Journal*, 1912, v. 58.
[2] Cf. N. Carey, This *Journal*, 1915, VIII. 78.
[3] G. H. Thomson, This *Journal*, 1919, IX. 325.

of the scores of the O, Y, G, and W groups will give a simultaneous value for the y variate; and a sum of the scores of the G, B, P, and W groups will give a simultaneous value for the z variate. A number of repetitions of such throws will give three arrays of values for the three variates respectively.

The correlation between the variates x and y will be

$$r_{xy} = \frac{W+O}{\sqrt{(R+O+W+P)(Y+G+W+O)}} \quad \text{.........(3).}$$

r_{yz} and r_{zx} can be found from similar equations. W is here called a general factor; P, O, and G, group factors; R, Y, and B, specific factors. Since R, O, and Y, etc. represent actual numbers of dice, these numbers are naturally treated as positive, and so far all that has been said applies to positive cells. It should be noted that only group and general factors appear in the numerators of the three coefficients, while all factors appear somewhere in the denominators. If, for instance, the value of R is increased, both r_{xy} and r_{zx} will be diminished, and r_{yz} remains unchanged. If, however, W is increased, all three coefficients will be increased, and similarly a fall in W will be reflected by a diminution of all three coefficients[1].

It appears that, when measurement reveals a set of high correlations among three variates, there is a favourable chance that a general factor W underlies these results; and that, when correlations are low, the chance of a general factor becomes more unfavourable. To make the problem more definite we can inquire whether or not there is a magnitude to which the r's can rise, above which we can say with certainty that a general factor exists, or, if it is known not to exist, that the correlations must have been produced in some way different from that here contemplated.

It is obvious, on the other hand, when a set of correlations are very low, that we cannot say with certainty that a general factor is *not* present, for such correlations, however feeble, *may* have been produced by such a general factor. If we strive to create patterns of ever-increasing correlations without the aid of a general factor, our only course is to increase the group factors in comparison with the specific factors. This process cannot be continued indefinitely, as even infinitely large group

[1] In practice, in psychological or other problems, the facts concerning the separate elements P, R, W, etc. are not known. It is necessary, therefore, to observe the greatest caution in making any such inferences as to the nature of the underlying mechanism producing the correlations. Indeed, the incentive to the prosecution of such studies as the present is the belief that a great many such inferences have, in the past, been made without that caution, and without stating what assumptions underlie them.

factors will not produce complete correlation. The same limit is reached when the specific factors are reduced to zero and the group factors remain. In such a case the group factors hold the field, and we have reached the highest set of correlations possible without calling in the aid of a general factor.

It can be shown[1] that in this case

$$r^2_{xy} + r^2_{yz} + r^2_{zx} + 2r_{xy}\, r_{yz}\, r_{zx} = 1 \quad \ldots\ldots\ldots\ldots\ldots(4).$$

This expression marks the boundary between values of r_{xy}, r_{yz}, and r_{zx} for which a general factor is necessary, and those for which it is unnecessary although possibly present. The proof already published shows how this expression, which we shall call D, was arrived at. Its properties may however be seen more clearly from the following treatment, which also gives an independent proof.

We have

$$\begin{aligned} D &= r^2_{xy} + r^2_{yz} + r^2_{zx} + 2r_{xy}r_{yz}r_{zx} \\ &= \frac{(O+W)}{(P+O+W+R)\,(O+G+W+Y)} \\ &\quad + \frac{(G+W)}{(O+G+W+Y)\,(G+P+W+B)} \\ &\quad + \frac{(P+W)}{(G+P+W+B)\,(P+O+W+R)} \\ &\quad + \frac{2\,(O+W)\,(G+W)\,(P+W)}{(P+O+W+R)\,(O+G+W+Y)\,(G+P+W+B)}. \end{aligned}$$

After algebraic simplification this expression reduces to the form

$$\frac{A + W\,(K)}{A + B\,(K') + R\,(K'') + Y\,(K''')} \quad \ldots\ldots\ldots\ldots\ldots\ldots(5),$$

where K involves only general and group factors, and all quantities have positive values.

From the expression in this form the following conclusions can be drawn:

(1) If $D > 1$, the general factor W must have a real positive value.

(2) If $D < 1$, the specific factors R, Y and B cannot all be zero. (If $R = Y = B =$ zero, then the denominator of expression (5) reduces to A only.)

(3) The effect of increasing any specific factor is to reduce D.

[1] See the joint appendix to an article by G. H. Thomson in This *Journal*, 1919, IX. 335.

(4) If the general factor $W =$ zero, D cannot be greater than unity.

(5) If the general factor W and the specific factors R, Y, B are all zero, D must be unity.

These conclusions have already been deduced from general considerations[1], and their application may be shown by reference to the following purely illustrative examples.

(1) Experiments in Auditory Memory[2].

x variate = Pitch of musical notes, y variate = Rates of ticking, z variate = Musical phrases.

$$r_{xy} = \cdot 36, \quad r_{yz} = \cdot 36, \quad r_{zx} = \cdot 55, \quad D = \cdot 704.$$

(2) Experiments in Verbal Memory[3].

x variate = Auditory Motor, y variate = Visual, z variate = Auditory.

$$r_{xy} = \cdot 52, \quad r_{yz} = \cdot 82, \quad r_{zx} = \cdot 76, \quad D = 2 \cdot 168.$$

The contrast between these two sets of memory experiments is significant, D being less than unity in one case and greater than unity in the other. Dice patterns on the basis of Fig. 1 may be made with or without a general factor in the first case, but must have a general factor in the second; whereas specific factors can be dispensed with in the second case but not in the first. In order to find what can be regarded as a high value for D, consider the results which would be obtained were we to apply the *same* mental test three times running (as we do in measuring reliability coefficients), instead of three *different* tests. Taking $\cdot 7$ as a fairly common reliability coefficient, this would give $D = 2 \cdot 156$. Beyond this value D rapidly increases towards 5, which is its maximum value.

It is necessary to emphasize that these inferences from expression (5) only apply to a mechanism with the conditions imposed. The inferences respecting specific factors will be considerably modified when a source of negative correlation is introduced. It would be still further unsafe to claim validity for these inferences in mental measurements when both the elements and the mechanism of correlation are unknown, but it may be possible in such cases to *disprove* general statements, that is disprove their generality, with the aid of the above inferences.

At this stage it is necessary to indicate a method of substituting numerical values for the symbols in Fig. 1. The method is described

[1] This *Journal*, 1919, IX. 336.

[2] N. Carey, *ibid.* 1915, VIII. 79.

[3] *Ibid.* 1915, VIII. 83.

in Appendix I, but we must note here that only positive numerical values can be allowed for the cells. Why is this limitation of positive cells imposed on Fig. 1? Suppose for example the value of R is negative, understanding this to mean that the scores of the red dice are to be subtracted instead of added in computing the variates. This would reduce the variate x *but the values of* r_{xy} *and* r_{zx} *would be just the same* as if R were positive. It can also be seen in the case of group factors that a negative P for instance while tending to reduce equally both variates x and z gives the same value for the r's as when P has a positive value. A similar statement is also true for W, the general factor[1].

Thus, when the only data we possess are the correlation coefficients themselves, it is impossible to say whether positive or negative terms occur in the pattern. No further purpose is therefore served in this paper by introducing negative cells. In applying equation (1) to negative values, a mere algebraic substitution of these negative values is not permissible. Equation (1) is only concerned with the *numerical* values of l, m, and n and is independent of their algebraic sign.

The problem of negative correlation, however, is quite another matter and is now to be dealt with.

5. *Weldon's experiment and negative correlation.*

The simple form of Weldon's experiment has already been described. The question arises whether negative correlation can be visualised in some simple way by a modification of Weldon's experiment. This can readily be accomplished by allowing the score of the common dice to be considered positive in the first variate and negative in the second or *vice versa*. In both cases

$$r_{xy} = \frac{-l}{\sqrt{(m+l)(n+l)}} \qquad \text{........................(6).}$$

Such a set of common dice can now be regarded as a source of negative correlation—not the only possible source by any means, but one that can readily be visualised, and that lends itself to mathematical analysis. It may be of assistance in interpreting correlation data.

Having secured a method of producing both positive and negative correlations by dice throws, it is possible to imagine two sources of correlation operating together, the one tending to produce positive and the other negative correlation. Such would be the case if there were two common sets of dice, l_1 and l_2, where the l_1 dice are regarded as positive

[1] For proof of these statements see Appendix II.

(or negative) in both variates, and the l_2 dice as positive in one variate and negative in the other. We then have the modified formula

$$r_{xy} = \frac{l_1 - l_2}{\sqrt{(m + l_1 + l_2)(n + l_1 + l_2)}} \quad \text{.................(7)},$$

involving four quantities m, n, l_1 and l_2. r will therefore be positive or negative according as l_1 or l_2 is the greater, and will be zero when $l_1 = l_2$. It is even possible to produce all correlations between $+1$ and -1, by these dice experiments, *without the use of specific factors at all*, in which case we have

$$r_{xy} = \frac{l_1 - l_2}{l_1 + l_2} \quad \text{..........................(8)}.$$

A proof of equations (6), (7) and (8) will be found in Appendix II.

6. *The effect of 'interference' in the case of two variates.*

An examination of equation (7) reveals the fact that the magnitude of the correlation between two variates gives no criterion as to the magnitude (not even the comparative magnitude) of the common factors involved. Correlation coefficients are sometimes held to give information in this respect. Take, for example, the following statement[1]: "Zero correlation always gives information concerning both (*i.e.* general or group) factors, that is, that they are both non-existent." We see by equation (7) that two variates may have zero correlation and yet the common factors l_1 and l_2 may be very large, so long as they are equal. Very low correlations are not inconsistent with very high common factors. To make statements involving the values of the unknown factors on the sole evidence of the correlation between the two variates is to take for granted the solution of a single equation involving more than one unknown quantity. The correlation coefficient of itself gives no certain evidence concerning the size of common factors.

That difficulty has been felt in this direction is evident from the writings of W. H. Winch and W. G. Sleight on the question of transfer. Winch says "It appears...that very low and indeed doubtful positive correlations between two mental functions, ... , may nevertheless be consistent with some real connection between those functions within the same mind[2]." Sleight says "It is evident that the old *a priori* methods are useless in the treatment of such problems; and further, that it is equally impossible to frame any such general law as that of the 'common

[1] N. Carey, This *Journal*, 1915, VIII. 73.

[2] This *Journal*, 1910, III. 405.

element'...The results before us prove so complicated that each case needs special investigation[1]."

As regards the question of transfer of improvement the interest in the present paper turns on the common factor which reduces positive correlation by tending always to produce negative correlation, viz. the factor represented by l_2 in equation (7). In dice experiments this factor is regarded as adding its score to one (say the x) variate, and subtracting the same score from the other (the y) variate. Such a factor we have conveniently called an 'interference factor' as it increases the scores of one variate and decreases the scores of the other. As a very simple device illustrating a source of negative correlation, a rôle which it performs to perfection in dice experiments, the interference factor deserves notice and serves as a valuable method of testing conclusions which are obtained from correlation data. The idea of interference in mental tests is not new. Sleight[2] says "Differences in mental processes may lead to loss of transfer or even reciprocal interference." In another connexion we find the following: "The process of learning continues after it ceases to be conscious and this unconscious continuance is greatly interfered with by any further mental work[3]."

The recognition of the interference factor throws some light on the complicated and diverse evidence obtained from mental tests, which are used for determining both improvements and correlations. For the sake of clearness and brevity let us take a case from dice experiments using equation (7).

Let $m = 5$, $n = 5$, $l_1 = 10$, $l_2 = 5$, where l_2 is positive for x.

Then by simple arithmetical calculation:

$$r_{xy} = \cdot 25.$$

Average score of $x = 70$ with a possible range of 100.

,, ,, $y = 35$,, ,, 100.

Now let improvement take place, and let improvement be simply represented by increasing the number of dice. The following are three cases of improvement and their consequent effects:

Case I. Let improvement in x be represented by the addition of one die to m, l_1, and l_2.

We then have $r = \cdot 22$.

Average score of $x = 80\cdot 5$ with a possible range of 115.

,, ,, $y = 35$,, ,, ,, 110.

[1] This *Journal*, 1911, IV. 423. [2] *Op. cit.* 386.

[3] Hart and Spearman, This *Journal*, 1912, v. 70.

Here correlation is decreased; average value for y is unaltered but its possible range is increased.

Case II. Let improvement in x have the large and proportional effect of doubling m, l_1 and l_2.

We then have $r = \cdot 267$.

Average score of $x = 140$ with a possible range of 200.
　　"　　"　　$y = 52\cdot 5$　　"　　"　　175.

Here correlation is but slightly increased and there is only half the ratio of increase in y compared with x, but three-quarters of ratio of increase of possible range.

Case III. Let both x and y be separately improved, adding one die to each factor, thus causing excess improvement in l_1. Let increased values be $m = 6$, $n = 6$, $l_1 = 12$, $l_2 = 6$.

We then have $r = \cdot 25$.

Average score of $x = 84$ with a possible range of 120.
　　"　　"　　$y = 42$　　"　　"　　" 120.

Here correlation is unchanged by such improvement.

In the three cases of improvement given above, the ordinary assumption of transfer through common elements is used. An exhaustive study is not here intended, the three cases being selected as suggestive illustrations. Something of this nature should be borne in mind when endeavouring to interpret the results of experiments in mental measurement. Case II contains increases of the original values in proportion to their original magnitudes, while Cases I and III do not follow such proportion. It is impossible to lay down a law of improvements in mental operations themselves, and hence these dice representations of improvement do not contradict any known principles.

In Case I, although transfer is assumed, there is no increase in the value of y, but there is an increase of scatter, and further in spite of an improvement in common factors the correlation is decreased. The effect of improvement in the y variate is used up in balancing the interference effect and no improvement is noticeable as a result. Does the absence of improvement in the second variate then refute the doctrine of transfer through common elements? Not necessarily, for Case I assumes transfer and yet no improvement takes place in the y variate, and this again is accompanied by a reduction in correlation. Case II might occasion no surprise so far as the improvements are concerned; and yet in spite of the large improvement in the common factor, the correlation coefficient is but slightly increased compared with the usual errors of experimental measurement. In Case III although both variates are separately improved and the common factor l receives, therefore, a double impetus, yet the correlation coefficient remains unaltered. The contemporary training of two abilities with common factors does not necessarily alter their correlation.

It is not surprising therefore that the results of experiment in transfer show great diversity, and we do not yet know sufficient about the factors involved, to say whether the results support or refute the doctrine of transfer. Correlation data do not yet give us the desired knowledge about the factors involved in mental processes. A combined transfer and correlation research would be valuable, if properly planned, and the writer hopes to carry out such an experimental inquiry in the near future.

7. *The effect of 'interference' in the case of three variates.*

We now turn our attention to the effect of interference in the intercorrelations between three variates. It has been found necessary to distinguish between two classes of correlation-producing elements, according as they tend to produce positive or negative correlations. This distinction may be used in the seven-fold pattern (see Fig. 1) and will have the effect of dividing the group factors each into two parts, the first indicating the number of dice whose score is added to (or subtracted from) both its variates, and the second indicating the dice whose score is added to one variate and subtracted from the other. Let these two parts of the group factor be denoted by the suffixes 1 and 2 respectively, see Fig. 2. The modification of the general factor for three variates is complicated by the fact that a factor may add its score to one variate and subtract it from the other two, thus becoming a source of positive correlation between one pair of variates, and of negative correlation between the other two pairs. This may be met for instance by using a factor W_x, where the suffix x signifies that the score is added to the x variate and subtracted from the y and z variates. Let W_y and W_z have similar meanings with respect to the variates y and z. For uniformity let W_1 indicate a general factor which produces positive correlations only[1].

The whole may be visualised in Fig. 2 which is a modified form of Fig. 1. It represents a dice pattern for three variates showing sources

[1] Clearly, in ordinary parlance only W_1 would be called the general factor. The others, W_x, W_y and W_z are general in the sense that they have an influence in each variate: but that influence is not always in the same direction, in some it is beneficial and in others it is harmful. Care must be taken to preserve a careful distinction between these two kinds of generality.

of correlations on the principles described, by imposing interference factors of a group and a general nature.

In constructing numerical examples of Fig. 2 the method adopted has been to begin with a numerical pattern with no interference. Then arbitrary interference factors are introduced, care being taken to preserve the original correlations by leaving numerators and denominators of the correlation coefficients unaltered. This has in the main the effect of reducing the specific factors, in some cases even to zero.

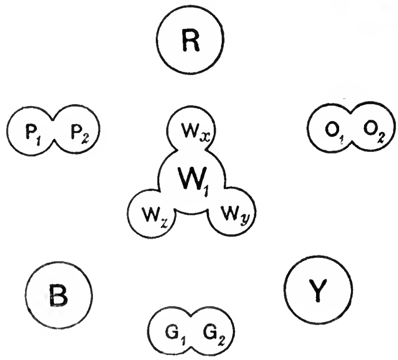

Fig. 2.

Although by the use of interference factors Fig. 2 still further complicates the mechanism of correlations between three variates, it must be remembered that correlation data may give rise to many interpretations, and that the aim is here to open up the way to different interpretations and to guard against the acceptance of any one interpretation by mere neglect of others.

8. *Applications to experimental results.*

The following is an example of experimental results and will be used to illustrate the great variety of possible interpretations. The results are taken from those of N. Carey[1]. Let three tests for visual, auditory, and verbal memory be tests for the three variates x, y and z respectively. We have $r_{xy} = \cdot 44$, $r_{yz} = \cdot 33$, $r_{zx} = \cdot 28$. We desire to use these coefficients in such a way as to make some conclusions about the factors involved in the three mental abilities. What information can these numbers give us?

We may proceed by asking what possible patterns of the dice experiments described will give these coefficients. A little trial soon shows that

[1] This *Journal*, 1915, VIII. 88.

very many such patterns are possible, and to show their variety a few special cases are given in Fig. 3.

Figs. 3 (*a*) and 3 (*b*) contain no interference factors. In (*a*) group factors are missing and in (*b*) the general factor is missing (this case is possible because $D < 1$, see page 87). So far then, neither general nor group factors are certain. Are specific factors certain? When interference is present, specific factors are not necessarily present, in spite of the fact that D is less than unity. Fig. 3 (*c*) is given to show hat group

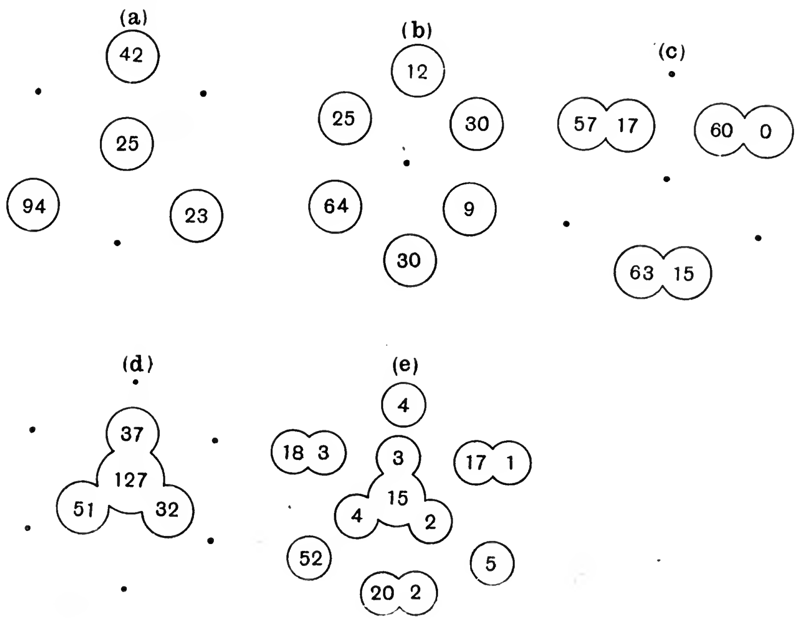

Fig. 3.

factors alone without specific factors and without a general factor will give the required correlations. Fig. 3 (*d*) serves to show that these same coefficients may be produced by general factors only. Indeed general factors, including interference, can be constructed to give any set of three coefficients.

The correlation coefficients of themselves, therefore, do not constitute any certain evidence as to the factors involved. As we have seen, any factor may be dispensed with when D is less than unity; and when D is known to be greater than unity, all that can be said is that a general factor of unknown magnitude is present. There is no case, which cannot be realised without the use of specific factors, for these are found to be unessential among three variates, as also they were found to be unessential in the case of two variates. Since mental processes, however, are thought to be very complex, we may say that perhaps all kinds of factors are present in a greater or less degree. Fig. 3 (*e*) is given as an example of a pattern where all thirteen factors, that have been employed in dice patterns, are present. These factors constitute too many unknowns to allow of their determination from three correlation coefficients.

The failure to appreciate the large variety of ways in which correlations can be produced has led to a number of conclusions in psychological and pedagogical experiments that are not necessarily true; in fact, in some cases only one among the many possible interpretations is taken into consideration. Another set of results by N. Carey[1] will serve as an illustration.

Six sensory discrimination tests are used, three of which are auditory tests, and three visual. The auditory tests are named Pitch, Rate, and Music, which we will call the x, y, and z variates respectively.

We have $$r_{xy} = \cdot 71, \quad r_{yz} = \cdot 60, \quad r_{zx} = \cdot 64.$$

The visual tests are named Yellow, Angles, and Patterns, and if these are taken as a second set of x, y, and z variates, we have

$$r_{xy} = \cdot 13, \quad r_{yz} = \cdot 00, \quad r_{zx} = \cdot 05.$$

From these two sets of correlations the conclusion is drawn that "An auditory factor evidently has a wider range than a visual[2]." The expression 'wider range' is vague. It cannot mean that the auditory factor is common to a greater number of abilities for the experiments are not designed to test this. It may mean that the auditory factor may reach values greater by comparison than the visual factor does. This is certainly a possible interpretation of the results but it would be rash to rely upon it as the correct interpretation. If the test of the seven-fold pattern is applied, other interpretations are found to be possible. Patterns which would support the author's conclusion may readily be constructed as in Figs. 4 (*a*) and 4 (*b*).

The general auditory factor is here 60 while the general visual factor is zero. It cannot be argued, however, that the auditory factor is neces-

[1] This *Journal*, 1915, VIII. 78.

[2] The writer quoted uses the term 'specific auditory factor' to mean a factor common to the three auditory tests. In the terminology of this paper such a factor would often be called a group factor, but in the present instance of three auditory tests it is called the general factor.

sarily larger than the visual, nor is it of much use to say that it is probably larger so long as the probabilities are unknown. Fig. 4 (*c*) gives the same correlations as Fig. 4 (*b*) with a general factor of 80. It is quite possible to say, with the data in hand, that the general visual factor is greater than the general auditory factor[1]. All three patterns have intentionally been made *without specific factors*, so as to emphasize their irrelevancy in the present argument, and to remind the reader that they can be dispensed with, so far as interpreting correlation data is concerned. No special importance is to be attached to the actual numbers in the patterns. Countless patterns could be made, but these three are chosen as illustrations.

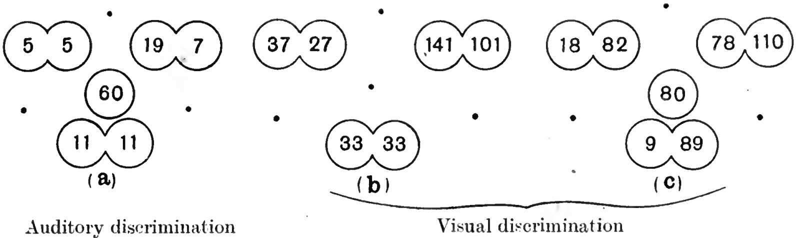

Fig. 4.

9. *Summary.*

Among the many possible mechanisms producing correlations that by overlap of common elements presents a ready means of testing conclusions arrived at from correlation data. Even in the case of dice experiments very uncertain evidence as to the underlying elements is given by correlation coefficients, in view of the great variety of possible conclusions that may be drawn, and of the impossibility of saying which is the correct conclusion. Still more difficult is it to draw conclusions concerning mental abilities in psychological and pedagogical experiments when the method by which mental abilities produce correlation is unknown.

When the mechanism of correlation is one of overlap, as in dice patterns, and three variates are considered, a condition is, however, known which, when fulfilled, gives certain evidence of the presence of a general factor.

A simple modification of Weldon's experiment introduces a source of negative correlation, which either reduces the positive correlation

[1] It is even possible to produce these three correlations (visual discrimination) by using general factors alone, as is done for another case in Fig. 3 (*d*).

present, or produces actual negative correlation. This phenomenon, here described as 'interference,' throws some light on the problem of transfer and suggests a possible line of explanation of the complicated and apparently inconsistent results obtained in actual experiment.

Specific factors are never necessary in accounting for correlation results, while factors of a general nature are sufficient to account for any set of correlation coefficients, provided that these general factors include interference elements[1].

In conclusion, I wish to express my deepest thanks to Dr Godfrey H. Thomson for suggesting this investigation and for his frequent advice during its progress.

Appendix I.

The General Pattern for Three Variates correlated by Positive Overlap.

It is required to insert positive values in the cells of Fig. 1 so as to give three required values for r_{xy}, r_{yz}, and r_{zx} respectively. The method can readily be seen in the general pattern of Fig. 5, where the coefficients C_1, C_2, C_3 are equal to

$$\frac{r_{yz}}{r_{xy}r_{zx}}, \quad \frac{r_{zx}}{r_{xy}r_{yz}}, \quad \frac{r_{xy}}{r_{yz}r_{zx}},$$

and m, n, p, q are selected numbers.

$$C_1 m^2 - m(n+p) + q$$

$$pm - q \qquad\qquad\qquad mn - q$$

$$q$$

$$C_3 p^2 - p(m+n) + q \qquad\qquad\qquad C_2 n^2 - n(p+m) + q$$

$$np - q$$

Fig. 5.

Suppose for instance it is required to make numerical patterns so that $r_{xy} = \cdot 4$, $r_{yz} = \cdot 5$, and $r_{zx} = \cdot 6$. First find C_1, C_2, and C_3 and then substitute *any* values for m, n, p, and q taking care however to avoid negative values for the cells. A large number of patterns may thus be made. For example take $m = 6$, $n = 5$, $p = 9$, and $q = 12$, which will give the

[1] See the distinction drawn in the footnote, page 93.

following pattern (see Fig. 1) $W = 12$, $P = 42$, $O = 18$, $G = 33$, $R = 3$, $Y = 12$, $B = 21$. Now calculate the correlations on the plan of equation (3), and they will be found to give the required values.

Further it can readily be shown that Fig. 5 is perfectly general, *i.e.* that every possible pattern can be made by its aid. In practice the patterns in the foregoing paper have been made so as to give accuracy to two places of decimals in the correlation coefficients. It must always be remembered that if the seven cells are multiplied by the same number, the resultant pattern will give the same correlations.

APPENDIX II.

Equations (6), (7) and (8) in the above text may be derived from the Bravais formula for the correlation coefficient.

Let m =	a small error	committed in a	measurement		a,
,, n =	,,	,,	,,	,,	b,
,, etc.	,,	,,	,,	,,	etc.

We have
$$x = Am + Bn + Cp + \text{etc.},$$
$$y = A'm + B'n + C'p + \text{etc.},$$

where x is the resultant error in the first variate, and y is the resultant error in the second variate.

The Bravais coefficient[1] can be written

$$r_{xy} = \frac{AA'\sigma_m^2 + BB'\sigma_n^2 + CC'\sigma_p^2 + \text{etc.}}{\sigma_x \sigma_y} \qquad \text{...........(9)}.$$

If the measured elements are dice scores, then

$$\sigma_m = \sigma_n = \sigma_p = \text{etc.}$$

We then have

$$r_{xy} = \frac{AA' + BB' + CC' + \text{etc.}}{\sqrt{(A^2 + B^2 + \dots)(A'^2 + B'^2 + \dots)}} \qquad \text{...........(10)}.$$

Further, in the case of dice scores which are added together,

$$A = A' = B = B' = \text{etc., except that some are zero.}$$

The measured units a, b, c, etc. are now represented by the individual dice. The question now arises concerning dice whose scores are considered negative. For instance, let the die b be common and its score negative for both x and y. B and B' in (10) are then each negative, but BB' remains positive and r_{xy} is the same as before. Let a be a specific die in x

[1] I am indebted to Dr G. H. Thomson's manuscripts for equations (9) and (10).

and its score negative; then A is negative, A' is zero, AA' is zero, and A'^2 is zero, but A^2 is positive. Again r_{xy} is the same as before. The statements on pages 87, 88, are thus accounted for. It remains to ask how negative correlation can be produced. The denominator in (10) is always positive. A term in the numerator such as BB' can only be negative when B and B' are of opposite sign, that is, the score of this particular die, in order to produce negative correlation, must add its score to one variate and subtract it from the other. The die b and all other such dice then give negative products in the numerator of (10) and become a source of negative correlation. Thus equations (6), (7) and (8) arise, when dice producing negative correlation are present.

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ON LISTENING TO SOUNDS OF WEAK INTENSITY.

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(*From the Cambridge Psychological Laboratory.*)

Part I.

A. Introduction.

1. *The origin of the present research.*

During the early months of 1918 it was realised that many of the difficulties presented by the various acoustic devices which were being employed for the detection of enemy submarines involved for their solution the careful selection of operators. Thereupon the Lancashire Anti-submarine Committee, mainly owing to the influence of Mr. A. P. Fleming, O.B.E., secured the establishment of an officially recognised psychological selection base where candidates for the anti-submarine service were examined. Tests were devised with a view to the detection of special abilities in listening. This work raised a number of problems largely of theoretical interest which could not be properly dealt with at the selection base itself. The Staff of the Cambridge Psychological Laboratory, which had been intimately concerned with the work almost from its beginning, therefore arranged to carry out research into certain of these points at Cambridge. One of the tests concerned auditory acuity, and the present paper has grown out of the more detailed investigation of the conditions under which sounds of relatively weak intensity may best be heard.

The work reported in this paper has been carried out under the general direction of Dr C. S. Myers, F.R.S., to whom we owe much for his advice.

2. *The aim of the present research.*

The two problems which were definitely attacked in this connexion were: (i) to devise apparatus and methods by which a satisfactory auditory acuity test may be secured, replacing the commonly used single intermittent stimulus by a continuous sound, and (ii) to observe in detail the influence of various objective and subjective factors upon successful listening to sounds of weak intensity. It was considered necessary to use a continuous source of sound as a stimulus because of the desirability of approximating to the conditions of actual listening for submarines. And it was initially evident that listening, even to very simple sounds, may depend on a variety of complex conditions. We have tried to get further and more accurate knowledge of the nature of these conditions, and of their effects.

B. Apparatus and Methods.

Apparatus.

All our early tests and experiments had been carried out with an ordinary Politzer acoumeter[1]. This instrument proved extremely unsatisfactory owing to the many uncontrolled variable factors involved. Slight changes in the pressure exerted by the operator on the lever of the instrument, or small deviations of the instrument from the perpendicular, are sufficient to cause considerable variability in the intensity of the sound. Any form of apparatus, also, which involves movement, either of the listener in relation to the source of sound, or of the source of sound in relation to the listener, must be unsatisfactory, owing to its dependence upon varying acoustic properties of the rooms in which the test is applied. In its simple form, therefore, the Politzer acoumeter was very speedily discarded, and, as a first modification, what may be called the 'boxed acoumeter' was used.

[1] This consists essentially in a small metal lever the weighted end of which, falling through a constant distance on to a metallic bar, produces a sound.

A Politzer acoumeter was carefully mounted inside a weighted box, so that it was held in a fixed horizontal position. An opening at the back of the box admitted the experimenter's hand for the purpose of manipulating the lever of the acoumeter. In the front of the box was a second opening over which cardboard shutters, with apertures of varying size, could be placed. This simple device secured the required variation in the intensity of the sound, while it avoided the necessity of any movement, either of the sound, or of the listener; at the same time the disturbing effects due to the acoustic properties of the room in which the test was carried out were considerably reduced. The results were more definite with this form of the instrument, but were still disturbed by uncontrollable variations due to freedom of play of the fall-hammer of the acoumeter. Further, the single intermittent sound was unsatisfactory for our purpose; and we had more than a suspicion that listening in free-air does not yield results directly comparable with those that may be obtained by listening over a telephone circuit, after the manner which was at that time required for the detection of submarines. It is obvious that when a subject is listening to a sound coming to him through free air it is far more difficult to eliminate distracting noises. For this very reason the boxed acoumeter proved of special service in obtaining definite results in some investigations concerning the effect of distracting sounds, which will be described in the second part of this paper.

Two other devices were used before we finally settled down to the investigations with a continuous sound. First an electrically controlled instrument was tried, the sound stimulus being the click produced by the fall of a small steel rod on to a metal base. The source of sound was mounted upon a scale and could be moved to different distances from the observer: in this way the necessary variations in the intensity of the sound were obtained. The sound was conducted to the subject's ear through a tube about three feet long, which was provided with an earpiece so as to cut out external disturbing noise. The necessary movement of the source of sound was reduced by the introduction of an iris diaphragm into the tube at the end farther from the listener, between whom and the source of sound a screen was interposed. The instrument, which may be called the 'tube and screen' audiometer, proved unreliable and, in particular, gave rise to much difficulty owing to the interference of sound waves reflected from the end wall of the testing room[1]. Conse-

[1] In fact all our observations tended to confirm the conclusions reported by Pillsbury as to the unreliability of the drop methods of producing sound; *vide* "Methods for the Determination of the Intensity of Sound," *Psychol. Monogr.*, 1910, XIII. 5–20.

quently, listening to sound coming through the free air was entirely discarded, and telephone listening was adopted in its stead.

The first kind of sound we employed was a click of varying intensity produced in the telephone receivers, the intensity being controlled by a series of graded resistances. The test devised for use with this instrument gave considerable satisfaction in practice, but was still open to the objection that an isolated click in no way represents the continuous sound of a submarine's engines. Hitherto we had been almost entirely preoccupied with the practical tests and had kept to the use of the click because little was known concerning the reliability of methods which employed a continuous sound. Now, however, we decided to take up definite research on the apparatus and methods needed for an acuity test with a continuous sound stimulus.

Apart from the practical value of the use of a continuous sound stimulus for the present purpose, the latter possesses certain obvious general advantages. Most sounds of daily experience are continuous in nature. The test itself is more interesting and tends to call forth more of the 'psychological' qualities of the listener. With a continuous sound it is easier to keep a definite check upon the observer's readiness to respond. He may be kept listening throughout a certain period during a portion only of which the sound is present, and note may be taken of the exact time elapsing between the actual onset of the sound and the response which marks the subject's reaction to it.

The source of continuous sound which we used in the earlier experiments was provided by an ordinary buzzer, such as may be purchased at any electrical stores. Owing to war conditions, apparatus by which we had hoped to produce a more constant sound was delayed for a long time. The buzzer which we were forced to use varied considerably on different occasions, and the variations were too rapid to admit of control. This renders many of our results difficult to interpret with any strict degree of accuracy. We have now experimented with several different types of buzzer, but have found nothing to give such reliable and steady results as the electrically maintained tuning fork with mercury cup contact, bridged by a condenser, which we employed in the final series of tests. As regards pitch, the original buzzer gave a dominant tone of about 900 vibrations per sec.; the pitch of the tuning fork of which we made most use was of 400 vibrations. Clearly, so large a difference of pitch might be expected to produce significant differences in the results. As a matter of fact, very little direct comparison of the results which we obtained with different settings of the apparatus is possible, but significant differences do not appear to have occurred. The precise effect of variation in pitch upon experiences at the threshold must be left over for more careful investigation.

In the first setting of the apparatus the buzzer and the main resistance were placed directly across the battery, and a galvanometer was introduced into the telephone circuit to give a measure of relative strengths of current used. (See Fig. 1.) With this setting a well-marked click in the telephone receivers was obtained every time the buzzer was switched on or off. In order to allow of 'catch' experiments, a second circuit was arranged giving a click, but no continuous sound.

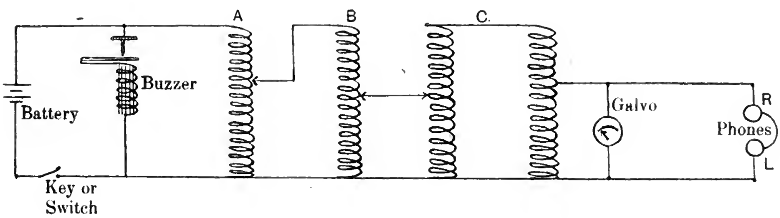

Fig. 1.

It was desired, however, to eliminate the click altogether, since it acted as a warning and proved to be audible when the continuous sound could not properly be heard. With our first setting it was obvious that the

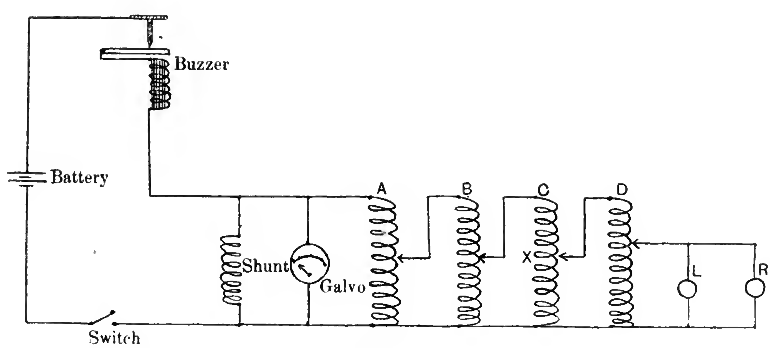

Fig. 2.

telephone receivers were being actuated by direct current from the battery, before the buzzer itself actually started up. This effect was removed when the buzzer was placed in series with the main resistance, and the click now disappeared. It is not clear why the click should have been completely eliminated by this arrangement, but possibly the diaphragms were always slightly in vibration owing to induced current: this, reducing the amount of inertia to be overcome when the buzzer was switched in, may have accounted for the disappearance of the initial and final clicks. We now placed the galvanometer across the first resistance, and used it merely as a check upon the constancy of the total current employed[1]. As a matter of fact the buzzer contacts proved increasingly faulty and variable, and gave a great deal of trouble. With this setting we obtained our measure of the threshold by taking a reading from the scale on one of the resistances, a finely graded potentiometer (see Fig. 2).

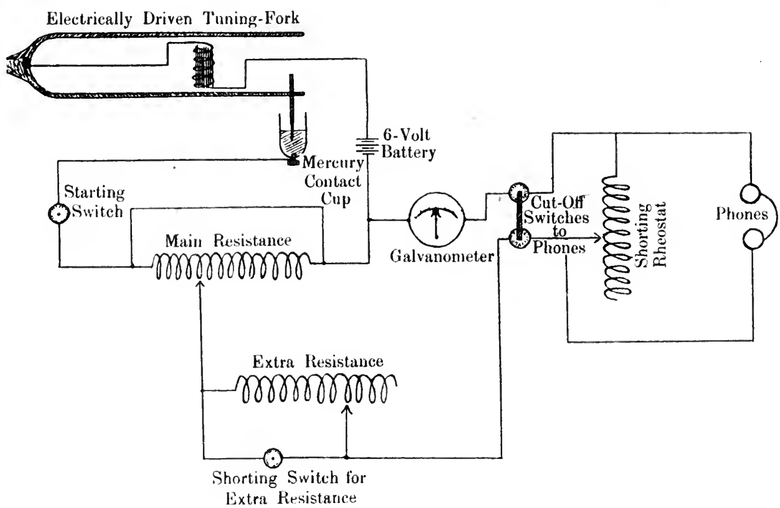

Fig. 3.

In the final setting, a tuning fork was placed in series with our main resistances, the galvanometer was replaced in the telephone circuit, and a shorting rheostat used to increase the possible range of intensities. A defect of the earlier setting had been the necessity for starting and stopping the source of sound at every trial. This was obviated by the use of a double cut-off switch to the telephones. Thus the tuning fork was in vibration during the whole period of testing, the sound being switched into, or out of, the telephones by the use of the cut-off switch. The

[1] This alteration was made on the suggestion of Mr H. Pealing, who was sent by the Lancashire Anti-Submarine Committee to give advice on the matter of apparatus.

threshold was obtained in terms of the strength of current employed, as indicated by the galvanometer (see Fig. 3). This setting of apparatus is perfectly simple to apply, and appears to yield very reliable results in practice. There was throughout not the slightest trace of a click in the telephone receivers.

Hereafter the results obtained from the first setting will be referred to as Series I, those obtained from the second setting as Series II and Series III[1], and those from the final setting as Series IV. In Series I, II and III, each ear, and both ears, were separately tested[2]. For the purpose of testing one ear by itself one of the telephone receivers was cut out, by an alteration in the attachment of the leads to their terminals in the observer's room. That is to say the same total current was flowing through the circuit, but was being received at one ear only. The diminished resistance consequent upon the cutting out of one receiver of course meant that the same scale reading on the apparatus represented a somewhat greater intensity of sound. Probably a better arrangement would be to use a dummy telephone, so that when a single ear is being tested that ear may receive approximately the same amount of current, for the same setting of the apparatus, as in binaural testing.

In most of our experiments, the subject's response was effected by the manipulation of a two-way switch in circuit with two differently coloured flags mounted on ordinary time-markers. Movement of the switch in one direction depressed a red flag and indicated that the observer had begun to hear the sound; movement in the other direction similarly actuated a green flag and was a sign that the sound was considered to have stopped. The necessity of noting all this, as well as of making the required adjustments, and of reading the galvanometer, placed a considerable strain on the experimenter. We are, therefore, in course of testing devices designed to record automatically the onset and duration of the stimulus, and the relation thereto of the subject's response.

Obviously, in order that the results obtained by any of the methods just described should be assessed at their proper value, accurate knowledge is required concerning the relation between the amplitude of vibration of the telephone diaphragms which are being used, and the strength of the current employed. We have made several attempts to determine this, and have secured some interesting results which will be described later[3].

[1] See p. 109 for description of Series III.

[2] The left ear of subject B. was decidedly sub-acute and for this reason it was not tested alone after Series I.

[3] See Appendix to second part of the paper.

Throughout the whole series of our investigations, the listener sat in the sound-proof room of the laboratory The sound was conducted from an adjoining room, in which the experimenter sat, and consequently the observer was entirely undisturbed by noises coming to him through the free air, other than those occasioned by his own movements. We were alternatively subject and experimenter in most of our observations, but as often as possible other subjects were used as well.

Methods.

The first method employed was to present sounds which were gradually decreased in intensity while the subject listened. Ten trials were made, the listener responding by the use of one switch when the sound was heard, and by another when it ceased to be heard. The threshold was taken as the mean of the ten disappearance values. Then a corresponding ascending series was taken. Later a method of mixed intensities was employed, and this yielded far more definite results. A continuous sound was kept going for five seconds at a given intensity, the subject responding in the usual manner if he perceived it. Then a different intensity value was substituted, until the whole range of intensities required had been covered. In order to obtain a threshold, each intensity within the range was presented ten times; each ear separately, and both ears together being examined. In all these cases a signal preceded each test, the warning being given by the flashing of a small electric light. The intensities used were mixed irregularly over such a range as to include values very readily appreciated, some that were appreciated only with difficulty, and others below the threshold: 'catch' experiments, in which no sound followed the signal, were interspersed. This method, which will readily be recognised as an application of the method of 'right and wrong cases,' was found to yield the best results. Clearly it is equally suited either to an intermittent or to a continuous source of sound. It is to be preferred to the method of gradual ascent and descent.

The whole of the experiments so far carried out may be grouped in four series. The first series comprises all the cases in which the continuous sounds were preceded by clicks. It is noteworthy that under these conditions even when the continuous sound had passed below the threshold, the click remained readily perceptible.

In the second series we varied the external conditions of listening. This series comprises tests in which no click was present, in which listening in bright illumination was compared with listening in the dark, and in which the warning light signal was sometimes present and sometimes absent.

Comparison of the results obtained in light and darkness suggested that interesting effects might possibly be produced by the use of coloured lights. As, in our observations under these conditions, we made a slight readjustment of the various resistances, the results obtained will be grouped together to make up the third series.

For the fourth series the final setting of the apparatus was used. We were now particularly concerned in the attempt to find evidence for or against auditory fatigue. The series therefore consisted of sittings extending over two hours' continuous listening. For the first group sounds of varying intensity were introduced at intervals, the subject responding each time he thought he could hear a sound; and for the second group the sound was kept on most of the time, and was interrupted at intervals, the subject responding as soon as he failed to hear. No detailed account of the results obtained from this series will be attempted in the present paper. The material secured raised a number of problems which call for more detailed investigation, and it will be more convenient to deal with the whole of these questions together in a later communication.

C. RESULTS.

A study of the results obtained from the whole series of experiments falls naturally into two main divisions. First we shall consider the conclusions that may be drawn from classifications and analyses of the actual responses secured, at the same time making use of explanatory statements of the observers where these seem to be required. Afterwards we shall consider the significance of such of the introspective remarks as have not been adequately reviewed in connexion with the objective study of results.

1. *Constancy of ranking from day to day.*

By ranking is meant the arranging in their relative order of efficiency of right, left, and both ears for a given setting. Thus, although the actual thresholds obtained might vary greatly from day to day[1], the ears might

[1] Cf. C. S. Myers, *Reports of the Cambridge Anthropological Expedition to the Torres Straits*, II. Pt. II. p. 145, 1903: "The acuity of hearing, probably more than that of most other senses, varies considerably from hour to hour and from day to day, according to the physiological condition of the observer."

yet present a fairly constant order of ranking over a long period. And from a study of the ranking in Series I (see Table I) this appeared to be actually the case.

Table I.

		Both Ears			R. Ear			L. Ear		
Subject	Series	Rank 1	Rank 2	Rank 3	Rank 1	Rank 2	Rank 3	Rank 1	Rank 2	Rank 3
B.	I	10	—	—	—	10	—	—	—	10
	II	13	8	—	10	11	—	—	—	—
	III	4	11	—	13	2	—	—	—	—
	Total	27	19	—	23	23	—	—	—	—
S.	I	7	3	—	5	4	1	1	2	7
	II	15	3	2	5	10	5	5	11	4
	III	5	5	5	6	5	4	8	4	3
	Total	27	11	7	16	19	10	14	17	14

The figures in the vertical columns under 'Rank 1,' 'Rank 2,' 'Rank 3' respectively, indicate the number of times in a series the given ear is so ranked.

Extended observations, however, carried out in Series II and III, made it clear that the order of ranking may undergo considerable change from time to time. For the purpose of comparison of right- and left-ear listening, subject B. may be ruled out, as hearing in his left ear is so subacute that by itself it is useless for the present purpose. But S. shows an interesting change, from a period at the outset, in which the right ear tended dominantly and consistently to rank above the left, through a period in which right and left ears appeared practically equal in ranking (in which that is to say their positions in first, second, or third ranks were almost interchangeable) to a period in which the left ear appeared slightly better than the right. As the separate testing of each ear was effected simply by the reversing of the telephone receivers on the head, no question of difference in relative sensitivity of the telephones can arise. It appears quite possible that, except in the case in which large differences exist, relative acuity in uniaural listening may undergo periodic change, and this conclusion tends to be confirmed by a prolonged series of earlier investigations with S. for subject, the details of which are not here reported.

Far more interesting however, is the study of binaural and uniaural differences considered from the point of view of order of ranking. As already mentioned (p. 107) our uniaural testing was effected by cutting out one of the telephone receivers. With this arrangement the right or left ear in the uniaural test must have been getting about double the strength of current which it received in the binaural tests.

On the basis of lengthy and repeated tests carried out on women students in the summer of 1918, when the boxed acoumeter was being used, it became perfectly clear that, where differences existed in the acuity of the two ears, uniaural hearing was on the whole superior to binaural.

This was undoubtedly due in the main to the fact that the attitude adopted for binaural listening in the Politzer acoumeter test, that of facing the sound, is unfavourable, as compared with the case in which one ear is directed definitely towards the source of sound.

With our present method, however, we found binaural listening to be clearly superior. This was particularly noticeable in the case of B. His left ear was so much below normal that with this ear alone he was quite unable to hear sounds at the binaural threshold. Nevertheless the binaural ranking was at first distinctly higher than that for the right ear. These results are entirely in agreement with those of earlier observers, but the superiority of binaural listening demands special and more careful investigation, and will be made the subject of a later paper.

2. *The effect of the daily order of testing on ranking.*

We varied the order in which the different thresholds were taken from day to day. Thus, for B. the uniaural threshold might be taken before the binaural, or the binaural before the uniaural; while, for S., either ear, or both ears, might occupy any one of three positions, and be tested first, second or third. Had it not been for this precaution a comparison of the uniaural and binaural thresholds would clearly have been rendered difficult owing to the effects of fatigue, adaptation and attunement. In computing the results it was necessary to consider each of the three series by itself. The plan adopted was to find the average values of each uniaural, and of the binaural, thresholds for a given series, taking into account position in the order of testing. This gives three average values each for right, left and both ears according as they were tested first, second or third respectively. The sets of uniaural thresholds, and the binaural thresholds, were then each compared among themselves, and all the instances in which the first position in the order of testing coincided with the first rank were grouped together, the second and third positions in regard to the order of trials being similarly treated. As Table II indicates, for B. position 1 in the order of testing coincided with rank 1 in the order of efficiency four times, while position 2 was only twice in the first rank, three times in the second rank, and once in the third rank. A similar relation is even more striking in the case of S., position 1 coinciding with rank 1 six times, while positions 2 and 3 were of rank 1 only twice and once respectively.

Table II.

Subject	Position	Rank 1	Rank 2	Rank 3
B.	1	4	2	—
	2	2	3	1
S.	1	**6**	2	1
	2	2	**6**	1
	3	1	1	**7**

It therefore appears that the order of testing is by no means without influence, the first threshold to be tested on a given day being to some extent more favourably situated than those which are tested later.

A more detailed summary of the results may be expressed differently. The *actual* thresholds obtained on a given day, instead of the average of a group of thresholds for a given ear, are now ranked in order of delicacy. The number of times in which rank and order coincide is then determined by inspection, and the exact differences occurring in the remaining cases are also ascertained. Thus + 1 and + 2 in the table below indicate that a given threshold ranks respectively one or two places higher than the order in which it was determined on the day in question, while − 1 and − 2 indicate a ranking lower than the order. The results of B., in Series I, are ignored for the purposes of this table, since in this series, on every occasion, the binaural threshold was the lowest, that of the left ear the highest, and that of the right ear intermediate. Consequently the order of testing produced no significant effect; or if any effect was present it was masked by other factors.

Table III.

	Subject B.	Subject S.
No difference	38	45
+1	19	36
−1	15	26
+2	—*	18
−2	—	10

* As only two thresholds (binaural and r. ear) were determined for subject B. each day in Series II and III, the largest difference that can obtain between rank and order of testing in his case is ± 1.

This table serves to bring out another fact, that on the whole there is a tendency for a particular threshold to rank higher than the mere order of its testing would warrant. The order of testing, that is, appears to be more influential in improving the rank of a given threshold than in bringing about the opposite result. It may be that listening is characterized by an 'initial spurt,' followed by a long period of 'plateau,' and this conclusion appears on the whole to be borne out by our later results obtained from continuous testing over long periods.

3. *The effect of the range on the number of errors and false responses.*

Errors are defined as failures to respond to the stimulus; false responses as cases in which a response was given when the stimulus was not presented. 'Catches' are here included under 'false responses[1].'

The limitation of the stimuli presented in a given series to those perceptible only with difficulty was not nearly so unfavourable as might have been expected from the observers' remarks concerning their attitude under such conditions. One of us several times complained that when there were very few clearly audible sounds, she felt thwarted and judged her attitude to be highly unfavourable to success in listening. The actual results, however, suggest that on the whole a small range of sounds of weak intensity is favourable to a low threshold. This is well shown by the following examples.

Table IV.

Subject	Range of intensities*	Threshold	Subject	Range of intensities	Threshold
B.	13·5–1·9	4·9	S.	6·5 –?	4·0
Left ear	12·0–2·2	6·0	Right ear	5·0 –1·8	3·8
	12·0–1·9	4·9		3·75–?	2·4
	12·0–1·5	4·2		3·5 –?	2·6
	11·0–2·0	4·5		3·0 –?	2·0
	6·5–1·5	3·2		3·0 –0·5	2·0
	4·2–0·5	3·0		3·0 –0·1	1·0
	4·2–1·0	2·8		2·5 –?	1·7
	4·1–1·2	2·2		2·5 –0·2	1·5
				2·3 –0·4	0·9

* The figures here given represent readings of the galvanometer scale, the approach to zero indicating the diminution of intensity.

It may be argued that the experimenter probably notices when the listener seems to be responding particularly well, and then tends to reduce the range of stimuli by cutting out the louder sounds. Were this the case, the low thresholds obtained with weak intensities could be set down solely to some condition favouring the listening attitude of the subject on a particular day. But the relation of low threshold to small range appeared to be far too constant for this to constitute the sole explanation.

[1] See footnote to p. 119.

When easily perceptible sounds are included in a series the subject probably tends to be rendered less sensitive. Not only does his attitude become more confident, so that he begins to listen less keenly, but he is prone to set up a kind of standard intensity, and to restrict his responses solely to sounds of which he is sure. Beyond this, it appears highly probable that in some way the inclusion of loud sounds in a series actually rendered the ear less sensitive to the weaker intensities[1]. We may therefore conclude that a limited range of weak stimuli tends to lessen the number of errors.

Turning now to the question of false responses, we find that the conditions of the present trials themselves predispose a subject to make responses at fairly short intervals. Such a tendency might be expected to react upon the threshold, so as to make the latter appear unusually low when a large number of subliminal sounds are introduced. But this result was not observed, and the reason appeared to be that long periods of silence commonly have the effect of rendering a listener relatively inattentive. Not only does he lose interest in his task, but he also comes to distrust himself, and to regard doubtful sounds as subjective in origin.

While therefore a range which contains a number of sounds of weak, but audible, intensity is favourable to few errors and a low threshold, if a large number of inaudible sounds are used responses to 'catches' may become less common, and the threshold may be considerably raised.

The number of stimuli used in a given series may be restricted in one of two ways: (*a*) by using a few liminal sounds differing but slightly in intensity; (*b*) by employing a few sounds varying by large amounts. The former plan is the more favourable to the listener. And this is so not only for the reasons just given, but because identification which, as we shall show[1], is an extremely important factor in the listening to sounds, is brought far more into play when finely graded differences are employed.

4. *Comparison of methods.*

Each intensity included in the series presented on a given day was repeated ten times. As a rule the different intensities were given in irregular order, but on most days an order of regular ascent, or descent, or of both, was followed at least once.

It might be expected that the method of mixed intensities would be less favourable for the discrimination of faint sounds than a method of regular descent, and that the latter would be preferable also to the method

[1] This will be considered in the second part of this paper.

of regular ascent. In the accompanying table (Table V) are set forth the number of times that each method was employed, together with the percentage number of errors for each series.

Table V.

		Regular descent method		Regular ascent method		Method of mixed intensities	
Subject	Ear tested	No. of times series given	% errors	No. of times series given	% errors	No. of times series given	% errors
B.	Both	38	2·3	33	2·5	460	2·4
	R.	34	1·7	32	2·1	460	2·3
S.	Both	40	1·4	35	2·4	450	2·1
	R.	47	1·4	45	2·4	450	2·2
	L.	39	1·3	32	2·3	450	2·2

Comparing the errors incident to the mixed method with those incident to the other two methods we find that while the difference is insignificant for B., a far more definite relation appears to hold for S. For S. the mixed method produces a result intermediate between those obtained by the two regular methods. At the same time the superiority of the method of regular descent over that of mixed intensities is more pronounced than is the superiority of the latter over the method of regular ascent.

Both subjects respond to fewer sounds with the method of regular ascent than with that of regular descent, the tendency being unmistakeable in the case of S., but only very slight in that of B.

Thus, whereas B. may be adversely influenced in a slight degree by the presentation of auditory intensities in an order of regular ascent, he is almost wholly indifferent whether a mixed, or a regularly descending, order is used. But this relation is definitely reversed in the case of S., the method of regular descent being for her easily the most favourable order of presentation. S.'s own reports show that she was conscious of this fact. She said that with regularly descending intensities the whole attitude was uncritical and there was some relaxation of attention. Constant strain and definite decision were absent. Identification was rarely resorted to, and the subject was usually well adapted and ready to receive the stimulus. In short, the reactions became much more mechanical. B. reported many times that the order of presentation of the stimuli was indifferent to him, so that in his case also there is correspondence between the reports and the results.

We therefore seemed justified in concluding

(*a*) that no method of presentation can be regarded as absolutely the best, but that in determining the method to be used individual differences need to be taken into account;

(*b*) that at the same time a method of regular descent as compared with a method of regular ascent probably tends to yield a slightly lower threshold;

(*c*) that the reason for this appears to be the relatively uncritical attitude evoked by the method of regular descent.

5. *The duration of the stimulus as a factor determining the response.*

Our observations have repeatedly shown that up to a time not exceeding five seconds, a distinct correlation holds between the intensity of the stimulus and the reaction-time. Thus while the reaction-time for clearly supraliminal sounds was practically always immediate, that for sounds at the threshold was usually much delayed, seldom being less than three seconds, and often considerably over four seconds. The reaction-time for sounds only just above the threshold was appreciably delayed, but to a lesser degree. It is open to objection that since the duration of the stimulus was the same throughout the greater part of the experiments, and an accompanying warning signal was frequently employed, the subject would fall naturally into the attitude of responding at regular intervals. Consequently, whenever an unusually long interval occurred (as when two or three subliminal sounds were successively presented) the automatic tendency would itself be sufficiently strong to account for the reaction. But the relatively small numbers of false responses, and the almost complete absence of reactions to sounds definitely below the threshold, rob this objection of any weight.

The question then arises how far the prolonged delay of response to weak sounds depended on sensory factors, and how far it was due to difficulties of identification and decision. Certainly the two last-mentioned causes were operative. Whether sensory changes also contributed to the effect is more disputable. But we believe that they did[1].

It is well known that every stimulus requires time to produce its full effect, and within limits it is true to say that the weaker the stimulus the longer is the time required. Kafka, working with auditory stimuli of weak intensity, fixed the time necessary for development at 1·5 seconds[2]. Wells in his study of *Reactions to Auditory Stimuli of Varying Duration*[3], affirms that, with a telephone buzzer as the source of sound, no difference

[1] Cf. C. S. Myers, *Text-Book of Experimental Psychology*, I. 30–31.

[2] "Über das Ansteigen der Tonerregung," *Psychol. Stud.* II. 288.

[3] G. R. Wells, *Psychol. Monogr.* 1913, XV. 68 *seq.*

of intensity was perceived when the period of stimulation was increased from 30σ to 106σ and though there was evidence that a difference in intensity was noticeable when the period of stimulation was increased from 7σ to 30σ. Here, then, are two estimates of the period required for the maximal development of sound, each of which falls very far short of four seconds.

The weaker auditory stimuli which we employed were certainly less intense than that used by Wells, and the same may be true in relation to those of Kafka. These findings, therefore, do not exclude the possibility that the longer delayed responses in our experiments were due to lag in the sensory development of the sound. Urbantschitsch, moreover, as quoted by Luciani[1], allows a longer latent period and states that a "weak tone is only fully appreciable to a sound ear after 1–2 seconds of continuous impression." It is possible that as the lower limits of auditory audibility are approached, the time required for a stimulus to produce its full effect increases very rapidly. The pitch of the stimulus-tone may also affect the latent period. It would be interesting to carry out a series of experiments to determine within what limits the threshold of auditory acuity may be varied according to the duration of the stimulus.

One argument in support of the contention that sensory changes were partially effective in the longer reactions, rests on the fact of the relatively small variability of reaction-time that was shown. If no sound requires more than 1·5 seconds for its development, the further delay being due solely to the factors of identification and hesitation, we should not expect to find the length of the reaction-time increasing regularly with decrease in the intensity of the stimulus. The fact that an inverse relation of this character occurred justifies our belief that sensory changes may be operative to as much as four seconds or more.

Another explanation may be suggested. Even though the period of sensory development of the auditory stimulus is so short as not to exceed two seconds in the longest cases, there may be superimposed upon this a more extended period of attunement. Attunement appears to be a process intermediate in nature between mere sensory response and conscious identification. It marks a state of preparedness, on the part of the subject, to receive a particular stimulus, or class of stimuli. In most of the well-known examples of attunement, such as the motor attunement which occurs during the lifting of weights[2], the subject has repeated experience of a given stimulus. This kind of attunement may occur in

[1] *Text-book of Physiology*, 256–7.

[2] See C. S. Myers, *op. cit.* I. 207–209.

our experiments also. We later refer[1] to the fact that a subject is unable at first to pick up sounds which, after a very short time, prove to be well above his threshold. But attunement need not be restricted solely to cases in which the same stimulus is repeatedly experienced. It may occur within the limits of a single experience, provided that experience is sufficiently prolonged. Combining the effects of these two kinds of attunement, we may maintain that it is owing to this factor that the delay of response to weak intensities occurs. The latent period required before a stimulus is reacted to varies inversely with the prior experience of the duration of that stimulus.

At what point the limits of attunement are reached in the case of a single sensory experience, and when attunement passes over into adaptation and fatigue, do not now concern us. Attunement need not be considered as entirely replacing conscious identification, but rather as a predisposing condition rendering the subject receptive to a sound which might otherwise escape his perception. Particularly in the cases in which the sound is exceptionally faint, an observer may still require to go through a process of conscious identification before he finally makes the response.

6. *The effect of a warning signal.*

Two kinds of warning signal were used, (α) the click which accompanied the making and breaking of the circuit in Series I (see p. 105); (β) a light signal from a small electric lamp which was given coincidently with the auditory stimulus and remained on as long as the latter was continued. In the 'catch' tests the light signal was presented by itself.

(α) *The Click.* This was eliminated from the three later series, so attention must be confined to Series I. A characteristic of this series was the remarkably small number of responses to 'catches.' The clicks used in the 'catch' tests of this series, unlike those associated with the sound stimulus, were approximately of the same intensity throughout, and were louder than the click accompanying the weaker of the continuous sounds. B. soon became conscious of this fact, but S. never appeared to notice the difference. The small number of 'catch' responses, then, may have been due to the fact that the subjects were influenced, either consciously or unconsciously, by the difference in quality and intensity of the 'catch' clicks.

The clicks may also in some cases have helped to lower the threshold. B. reported: "I could not distinguish between the clicks to-day. When

[1] P. 128; also see p. 114.

the clicks are faint I know the sound is in, and feel a tendency to respond." S. believed that the warning afforded by the click was helpful, since the click itself seemed to be modified in a peculiar manner when succeeded by a sound. In this way she thought she was able to detect indirectly sounds which might otherwise have been below the threshold.

Again, the clicks were usually distinctly audible, even when the continuous sound was liminal or sub-liminal. Owing to this fact, the subject came to limit the period of intense concentration to definite intervals[1]. At present the evidence carries a slight indication that the periodic relaxation of attention thus induced was unfavourable to discrimination. This is another point on which a practised differs from an unpractised subject; for the latter distinctly prefers to have the warning click, or some equivalent signal.

The initial click, as a rule, appeared to be somewhat louder than the continuous sound it introduced. It has been shown earlier in this paper, that the introduction of loud sounds into a series, appeared to influence adversely the subjects' ability to detect fainter sounds[2]. In so far as this is the case the click signal does not make for the lowest possible threshold. This signal appears to exert two counter-influences. On the one hand, it provides an additional basis for discrimination and inference; on the other, it may tend to render the subject somewhat less sensitive to the stimulus.

The limiting of fixed attention to certain well-defined intervals seems unquestionably favourable to the reduction of the number of false responses, for with a signal of the present type clearly the number of such responses cannot exceed the number of 'catches' actually inserted by the experimenter. Where no such limit was imposed the false responses were not evenly distributed but tended to fall into groups[3]. This was

[1] B. reported: "I have an impression that I might not hear without the clicks, but with them no stiffening of attention comes immediately. They are a signal to attend, but unless the sound follows attention is not strained." When the clicks were discontinued, B. said: "The absence of the clicks seems to make it more difficult to attend, because the clicks stir one up."

[2] See p. 114.

[3] Definite silent intervals, lasting for five seconds, unless the subject responded before that time was up, were interpolated into the other series in the same proportion as the 'catch' tests were distributed in the series where a signal was used. The absence of a signal greatly increased the number of false responses. The latter were all recorded, irrespective of whether they coincided with one of the set silent intervals or not. In the case of one of the subjects, B., it was noticeable that a large number of false responses were made almost immediately after the cessation of what appeared to be subliminal stimuli. Cf. Knight Dunlap, *Psychol. Rev.* XI. 1904, pp. 308–318.

shown very clearly in the two-hour sittings. The use of the double click signal prevented the appearance of these clusters of perseverative reactions.

In another way the subject was more certain of the sound when warning signals were present. For if he suspected the presence of subjective sensations, he was able the more readily to identify their precise character by noticing their occurrence during the empty intervals. In this way subjective, as compared with objective, sensations often appeared to possess differences of quality, pitch and localisation, which were then used as criteria in the subsequent reactions. The observer thus felt himself to be more in control of the situation when an accompanying limiting signal was used.

(β) *The Light Signal.* In the present tests the light and sound were usually presented to the subject contemporaneously. Sometimes the light was given without the auditory stimulus, and this constituted a 'catch' test.

The effect of a light signal appears to vary according as the signal is anticipatory to, or contemporaneous with, the stimulus.

In the tests with the 'tube and screen[1]' audiometer the warning signal "Now," which had been spoken by the experimenter immediately before the presentation of a sound, was replaced by a flash of light. Complaints had been made that the spoken "now" proved disturbing, and rendered the subject less able to detect the weak stimulus sounds. But in no case, in this set of tests, did the substitution prove satisfactory. Practised subjects did not readily adapt themselves to the change in the nature of the warning signal, and it was objected that the light was troublesome because of:

(i) the necessity for holding the eyes open, many subjects preferring to listen with closed eyes;

(ii) the switching of the attention from one class of stimulus to another;

(iii) the necessary fixation of two different positions, that at which the light appeared, and that from which the sound was expected[2].

The first objection could not be urged in the later tests, which all took place in a dark room, so that the light could be perceived even when the eyes were held lightly closed. Moreover, since the light was kept shining

[1] P. 103.

[2] In many cases the sound was definitely localised. Most subjects found it an aid to concentrate attention on this position, and expressed great surprise when the sound appeared to come from a different region.

for five seconds unless the subject responded earlier, it was possible for the listener to ignore it more or less, during the greater part of the test, only referring to it in cases of doubt. This plan was adopted by S., who, in consequence, now ceased to be disturbed by the flash of light, but was, on occasion, able to make use of it as a basis of inference concerning the presence of the sound.

Thus when the light signal was first discontinued S. remarked: "I think I missed a good many sounds. The series seemed long and unpleasant. I missed the light and found I had used it in two ways; (*a*) when I thought there was a sound, but was not sure, I looked to see whether there was a light or not; (*b*) if a particularly long interval elapsed during which I heard nothing, I looked for the light."

Table VI.

Subject	Series	Ear	Average Threshold: No Signal: Threshold* Resistance	Average Threshold: No Signal: No. of Sittings	Average Threshold: Light Signal: Threshold Resistance	Average Threshold: Light Signal: m.v.	Average Threshold: Light Signal: No. of Sittings	Average No. of Errors: No Signal	Average No. of Errors: Light Signal	Average No. of false responses: No Signal	Average No. of false responses: Light Signal
B.	I	B.	B 7	1	B 4·8	1·64	10	27	22·2	1	1·0
		R.	B 8·2	1	B 6·2	1·86	10	39	28·6	2	0·7
		L.	B 11·3	1	B 12·12	0·20	10	18	37·2	3	—
	II	B.	D 2·1	3	D 3·33	1·11	10	$11{\cdot}\dot{3}$	16·8	$1{\cdot}0\dot{3}$	2·0
		R.	D 2·3	3	D 3·22	0·82	.10	$13{\cdot}\dot{6}$	16·2	$0{\cdot}\dot{6}$	1·8
S.	II	B.	D 2·2	3	D 3·01	0·7	9	13·0	12·5	$2{\cdot}\dot{6}$	1·0
		R.	D $2{\cdot}5\dot{3}$	3	D $3{\cdot}\dot{7}$	1·08	9	$13{\cdot}\dot{3}$	19·0	$0{\cdot}\dot{6}$	$0{\cdot}\dot{7}$
		L.	D 2·6	3	D $3{\cdot}8\dot{1}$	0·91	9	13·0	15·0	$2{\cdot}\dot{3}$	1·0

* The figures in each case represent the threshold as indicated by the scale reading on the variable rheostat, the approach to zero representing diminishing intensity.

B. confirmed the effect of the light in increasing confidence. For when the light signal was discontinued he said: "At first the general attitude was quite different. There was a great deal more uncertainty. But this quickly passed off and afterwards I noticed very little difference except that it was more necessary to concentrate on the actual listening" (cf. clicks, p. 119). After a further period with the light signal it was again discontinued and B. reported: "Not very different from the series with the light. But the sound seems to come in more explosively, and it seems more important to catch the sound at the start[1]."

[1] It is perhaps interesting to record that B. several times, and S. once, remarked that the warning light appeared definitely brighter when no sound was present. For example, B. reports: "Light repeatedly seemed more brilliant when there was no sound, though this was not invariably the case." This increased brightness may perhaps have been due to objective variation of the light. But see Ward, *Psychological Principles*, Cambridge, 1918, p. 69.

Only a few tests in which the conditions differed solely in the presence or absence of a light signal are available for purposes of comparison. The results obtained are summarised in Table VI (p. 121).

There is little or no evidence of any pronounced tendency towards opposite results on the different occasions. But, leaving aside B.'s results in Series I which are, as will be seen, very meagre, there is a slight tendency for the errors to increase in number when the light signal is used.

It appears, therefore, that neither the sound signal nor the light signal can be regarded as of much importance to the listener. They may be used to confirm a judgment as to the presence or absence of a sound, but beyond that have little influence. An anticipatory light signal may however, under some conditions, prove detrimental to listening.

7. *The effect of light and darkness.*

It was arranged in the early tests that the subject should sit in an absolutely dark room, as it was assumed that this condition would favour concentration of attention upon the sound. Later, however, this supposition was called in question. It was then decided to try the effect of illuminating the experiment room by light from a bright electric lamp[1]. In order that the light should produce its full effect the listener was requested to keep his eyes open throughout the test. Conditions of darkness and light were alternated for a short period, but the tests were not prolonged, since very little by way of modification of results seemed to be observable. No signal was used in this series of tests. The results obtained are given in Table VII.

In Series III the experiment room was much more brightly lighted than for the earlier series. Considering first the results from Series II, we find that when tested in the dark both subjects show a distinct though slight tendency to a lower threshold, to a diminution of errors, and possibly to a slightly increased tendency to false responses. Both subjects preferred listening in a lighted room, though B. was convinced that darkness produced a more favourable condition for hearing faint sounds. Clearly the number of tests is so small that all of these suggestions must be accepted with reserve.

Partly with a view to seeing whether variation of hue as well as of brightness of illumination might have any effect on sensitivity to sound,

[1] The floor, walls and ceiling of the room in which the subject sat had a dull black surface, which considerably reduced the effect of the light. In the later series, however, the room was entirely covered with white cartridge paper; hence a much more brilliant effect was produced.

and partly to secure favourable conditions for observing the effect of variable moods on listening, we carried out a number of trials using differently coloured lights in the subject's room. Interspersed among the colour tests were two trials in which very bright white illumination was used[1], and one in which each subject listened in complete darkness. The whole set of these experiments constitutes Series III[2].

Table VII.

Subject	Series	Ear	Average Threshold*				Average No. of errors		Average No. of false responses	
			Dark room		Lighted Room					
			Threshold Resistance	No. of Sittings	Threshold Resistance	No. of Sittings	Dark room	Lighted room	Dark room	Lighted room
B.	II	B.	D 1·7	2	D 2·$\dot{3}$	3	9·0	13·$\dot{6}$	2·0	0·$\dot{3}$
		R.	D 1·9	2	D 1·8$\dot{3}$	3	11·5	11·0	1·0	0·$\dot{6}$
		B.	D 1·5	1	D 1·75	2	10·0	10·5	1·0	1·0
		R.	D 1·7	1	D 1·9	2	8·0	10·0	0·0	1·0
	III	B.	D 2·5	1	D 3·0	1	21·0	22·0	2·0	5·0
		R.	D 3·0	1	D 3·1	1	16·0	21·0	2·0	3·0
S.	II	B.	D 2·5	2	D 2·3$\dot{6}$	3	10·5	14·0	2·5	0·$\dot{6}$
		R.	D 2·35	2	D 2·70	3	13·0	11·$\dot{6}$	3·0	1·0
		L.	D 2·30	2	D 2·50	3	11·0	14·0	2·5	2·0
		B.	D 1·9	1	D 3·0	2	10·0	13·0	2·0	2·0
		R.	D 1·9	1	D 2·4	2	9·0	13·5	1·0	4·0
		L.	D 1·2	1	D 2·6	2	4·0	13·5	6·0	5·0
	III	B.	D 3·0	1	D 2·8	1	19·0	20·0	3·0	0·0
		R.	D 2·6	1	D 2·3	1	16·0	15·0	1·0	2·0
		L.	D 3·0	1	D 2·3	1	17·0	15·0	2·0	1·0

* The threshold is given in terms of the variable rheostat reading, the approach to zero representing a diminishing intensity.

When we consider the results of the two subjects in this series we discover an interesting individual difference. There is some suggestion, in the case of S., that whereas darkness produces a more favourable condition for listening than moderate illumination, the reverse may prove true when a brilliant light is used[3]. Thus S. did unusually well in the bright light and badly in the dark (Table VII). B.'s result does not confirm this. He did exceptionally well in the dark, but his performance in

[1] The illumination was provided by three ordinary electric lamps, while the white surface of the ceiling, walls and floor heightened the effect.

[2] These tests were unavoidably cut short.

[3] This subject complained very much of the prolonged sittings in the dark, and was greatly irritated by the experience. She felt discontented and suffered from a lack of stimulation. For concentrated mental work S. always prefers very intense sunshine or a brilliantly illuminated room.

bright light was distinctly poor, and showed a striking increase in the number of false responses.

As regards listening in coloured light the results are entirely inconclusive. The blue and green seemed possibly favourable to a low threshold, and the yellow and violet unfavourable, with the red intermediate. In the case of the yellow and violet illumination both subjects agreed that the feeling tone accompanying the whole of the test was unpleasant, and the high threshold may perhaps be attributed to this fact. So far as objective results go however, very little of interest is to be gathered from the few trials which we have been able to make on the influence of light, darkness and colour.

8. *Distribution of errors and false responses.*

In this section we shall continue to distinguish as before between errors and false responses (p. 113), though treatment of one, apart from reference to the other, will not always be possible.

We shall consider first the distribution of errors.

(i) In the determination of a given threshold fewer errors occur in the first half of each day's test than in the second half (see Table VIII, p. 126).

(ii) The smaller number of errors in the earlier half of each test is largely due to a short initial period, confined almost entirely to the first presentation of the series of intensities, for which the percentage of errors is remarkably small (see Table IX, p. 126).

(iii) Fewer errors result when a method of gradual descent is employed as contrasted with a method of gradual ascent (pp. 113, 114).

Before discussing the interpretation of these results it will be well to indicate the chief facts concerning the distribution of false responses.

(i) In the determination of a given threshold a definite decrease in the number of false responses is shown in the second half of each day's test (see Table X, p. 127). This decrease is more marked in the case of S. than in that of B. The phenomenon is also more pronounced in Series III, where no signal was used, than in that of the earlier series.

(ii) The tendency to respond when no sound is present appears to be greatest at the beginning of a sitting; but it diminishes after five to ten minutes, rapidly at first, and then more slowly (see Table XI, p. 127).

(iii) There is some indication that the decrease in false responses is of a temporary character only.

(iv) In tests where no signal is used the false responses show no regularity of distribution but tend to occur in groups, the tendency to such responses recurring at intervals during a prolonged sitting.

Comparing these two sets of results it is evident that a rough inverse relation holds between errors and false responses. Thus the number of errors is smallest in the first half of a day's test, the number of false responses greatest: while the reverse is true of the second half. There is no inconsistency in this opposition; but though various suggestions offer themselves it is difficult to determine, with any exactness, the factors at work.

Thus it seems natural to attribute the increase in the number of errors in the later half of a sitting to fatigue. But the fact that the increase in errors is coupled with a decrease in the number of false responses suggests another explanation. Possibly both results are the outcome of increased carefulness and greater determination not to respond except in the case of complete confidence.

Or again, the result in question may be due not so much to the adoption of a definitely cautious attitude, as to the attunement which comes about as the result of prolonged experience of a given set of conditions. In all probability, the complete explanation involves all three factors. It is very doubtful, however, if sensory fatigue really enters in to any appreciable extent: we suspect rather, that such fatigue as occurs is central in origin, for the following reasons: (*a*) that it is generally agreed that fatigue of the auditory end-organ is extremely evanescent, and that recovery is very rapid; (*b*) that it was repeatedly found in our two-hour sittings (see p. 109) that the longer the duration of a liminal sound up to ten minutes, the greater the chance of its being perceived and of the subject's reacting to its outgoing; moreover, the lag of the reaction was very much shorter after the longer sounds than after sounds of briefer duration[1].

The occurrence of groups of false responses at irregular intervals may, perhaps, be regarded as a fatigue effect. It is conceivable for instance, that the strain of prolonged attention in time overcomes the inhibition normally present; and that in this condition the subject does not discriminate between entotic sensations and those produced by the stimulus proper. Or the releasing of the response by a very faint sound may induce an over-confident attitude in the subject, and set up a whole

[1] Attempts have been made to develop a general fatigue test on the basis of auditory experience. The whole trend of our results goes to show that any such test must be extremely unsatisfactory.

series of responses because the normal control has been temporarily broken down. The prolonged strain may even *induce* subjective sensations. Or in some cases the phenomenon may be motor in character, depending mainly on the kinaesthetic strain set up in the tense arm and fingers. Motor strain in hand or arm cannot be the sole, or even the main, cause however, since it was involved to a much less extent in our last set of experiments, Series IV, which show the phenomenon of clusters of false responses in its best form.

Table VIII.

		Both Ears		R. Ear		L. Ear		
Subject	Series	1st half	2nd half	1st half	2nd half	1st half	2nd half	No. of sittings
B.	I	118	104	123	163	169	203	10 days
	II	132	159	146	141	—	—	21 ,,
	III	157	203	115	150	—	—	15 ,,
	Total	407	466	384	454	169	203*	
S.	I	127	115	118	110	148	165	10 ,,
	II	119	133	149	159	132	137	20 ,,
	III	121	133	122	141	109	114	15 ,,
	Total	367	381	389	410	389	416	

Total number of errors in 1st half of each day's test as compared with the total number in the second half.

* The figures for B.'s left ear represent the results of Series I only.

Table IX.

		Both Ears			R. Ear			L. Ear		
Subject	Series	1st 'set'	Average of 'sets' 2–9	m.v.	1st 'set'	Average of 'sets' 2–9	m.v.	1st 'set'	Average of 'sets' 2–9	m.v.
B.	I	16	22·2	3·60	15	28·6	5·12	19	37·2	4·56
	II	22	29·1	3·32	22	28·7	3·1	—	—	—
	III	27	36·0	4·60	19	25·4	2·72	—	—	—
S.	I	30	24·2	3·20	21	22·8	2·40	27	31·3	2·90
	II	17	25·2	2·56	24	30·8	4·36	21	26·9	2·90
	III	14	25·4	2·56	17	26·3	3·04	17	22·3	1·36

Total number of errors in the 1st 'set' of each day as compared with the average number of errors for the remaining nine 'sets.'

In tests of this kind it is very difficult to distinguish between the effects of fatigue and the loss of attunement. Thus the groups of false responses may be primarily due to temporary distraction, for example, a change of position, or a deep breath. We have already seen that the number of false responses is greatest at the beginning of the day's test, when fatigue is, presumably, minimal, but it is noteworthy that attunement cannot, at this stage, have begun to exercise any pronounced influence. In this connexion reference may be made to the fact that a lower threshold was secured when the stimuli were presented rapidly and at even intervals. This is readily accounted for by the better attunement induced by such conditions.

Table X.

Subject	Series	Both Ears 1st half	Both Ears 2nd half	R. Ear 1st half	R. Ear 2nd half	L. Ear 1st half	L. Ear 2nd half	No. of sittings
B.	I	1	9	5	2	2	1	10 days
	II	21	10	13	13	—	—	21 ,,
	III	26	19	40	26	—	—	15 ,,
	Total	48	38	58	41	2	1*	
S.	I	—	1	6	2	4	3	10 ,,
	II	18	12	16	11	23	20	20 ,,
	III	22	10	16	4	17	13	15 ,,
	Total	40	23	38	17	44	36	

Total number of false responses in 1st half of each day's test as compared with the total number in the 2nd half.

* The figures for B.'s left ear represent the results of Series I only.

Table XI.

Subject	'Set' 1	'Set' 2	'Set' 3	'Set' 4	'Set' 5	'Set' 6	'Set' 7	'Set' 8	'Set' 9	'Set' 10
B.*	32	28	22	18	8	16	16	18	18	12
S.	28	31	26	15	22	14	12	14	12	24

Total number of false responses made in each 'set' from 1 to 10; the totals for the three series, I–III, are massed together for the purpose of this table.

* In B.'s case these totals are diminished by the fact that his left ear was not tested in Series II and III.

The relatively large number of false responses occurring in the earlier part of a day's test, together with the remarkably small number of errors on the first presentation of the range of stimuli, rather point to an attitude of eagerness at the beginning of the sitting. Frequently some excitement is present, the listener is anxious to do well and has not had time to become discouraged. His attitude is apt, also, to be less critical at the start and his responses more impulsive. As a rule this state passed over into a more stable or persistent attitude, such as indifference, passivity, a calm and critical mood, or even discouragement and vexation. Certainly the fresh impulsiveness often present at the start soon suffered a check of some kind.

The very small number of errors in the first 'set[1]' of each day's tests

[1] The various stimulus-intensities selected for use on a given day were each presented ten times. There were thus ten 'sets' (*i.e.* repetitions of the selected stimuli) for each day's threshold. The order of testing followed was irregular (cf. p. 108).

as compared with the average of the remaining nine 'sets' is all the more striking in view of the fact that although the method of procedure adopted in S.'s case[1] favoured a small number of errors (p. 115), the difference is just as marked in B.'s case where no such explanation can be sought.

9. *Practice and improvability.*

Improvability in auditory acuity is almost wholly attributable to the subject's increasing familiarity with the conditions, and to his adoption of a more consistent method of response; it is not to be regarded as primarily sensory in character. Improvement may be manifested in two ways, either by a lowered threshold or by a more sharply defined threshold and by the diminution in the number of false responses.

It was repeatedly found, in the various forms of acuity tests, that unpractised subjects failed to respond to sounds that proved later to be well above their threshold of hearing. This initial disability to detect faint sounds was least marked in those tests in which the sound was electrically produced, the subject listening through telephone receivers. Undoubtedly the ability to recognise a sound plays a most important part in determining the threshold. This fact will be brought out in much greater detail in a section in the second part of this paper dealing with the evidence from the introspective reports.

The second kind of improvement mentioned also depended largely on increased familiarity with the sound and with the conditions of the test. It consisted in a more sharply demarcated threshold and in a drop in the number of responses given when no sound was present. Improvement of this kind was observed in a few cases only. Some of the chief contributing causes were—the assumption of a more comfortable position by the subject; increased power of concentration, due in part to the elimination of unfamiliarity from the situation; and the adoption of a more consistent criterion in border-line responses. Improvement of this kind invariably manifested itself early in the tests, if at all. Our results, already quoted, show that a sudden drop in the number of false re-

[1] In S.'s case the method of gradual descent, which, as we have already seen, is conducive to very few errors, and a low threshold, was employed a much larger number of times for the first 'set' of each day's tests than for any subsequent 'set.' The exact figures are: S. method of gradual descent employed 71 times out of 135 for the first 'set' as contrasted with 55 times out of 1215 for the remaining nine of each day's 'sets.' Neither this method nor the method of gradual ascent was ever used in any first 'set' in B.'s case, yet the superiority of the first 'set' as judged by the small percentage of errors, is no less marked. In B.'s case the method of gradual descent was employed 72 times in 960 'se

sponses often occurs early in a sitting, and this appears to constitute one of the most consistent and best defined modes of improvement.

We had hoped to obtain some data as to the nature of the improvement, if any, which follows from long practice. The fact that several different settings of apparatus were used, makes it difficult to deduce any reliable conclusions on this point. By comparing the average thresholds of the two subjects for the various series however, it is possible to get a rough relative measure of improvement. When this procedure is adopted, there seems to be definite evidence of the occurrence of fairly well-marked improvement in the case of S. as compared with that of B. The subjects' introspections also confirm these findings. We give the actual thresholds for comparison in Table XII.

Table XII.

		Both Ears		R. Ear		L. Ear	
Subject	Series	Average threshold	m.v.	Average threshold	m.v.	Average threshold	m.v.
B.	I	4·80	1·64	6·20	1·86	12·12	0·20
	II	2·62	0·83	2·62	0·70	—	—
	III	3·82	1·50	2·89	0·77	—	—
S.	I	8·08	1·0	8·20	1·40	9·55	0·90
	II	2·68	0·66	3·05	0·82	2·98	0·84
	III	2·63	0·57	2·78	0·39	2·44	0·39

It is evident from these results, that whereas B.'s acuity was unmistakeably greater than S.'s in the first of these series, S. approaches much more nearly to B. in this respect in the two later series. It is open to objection that this approach to equality is due not primarily to any improvement on S.'s part, but rather to a falling off on B.'s. There is reason to believe however, that such deterioration is seldom so prolonged as must have been the case if it were the sole cause at work.

It may be concluded then, that some degree of practice is necessary before the lowest threshold of acuity can be secured, but it still remains doubtful if prolonged practice produces any further effect of this kind.

(*Manuscript received* 5 *August* 1919.)

PUBLICATIONS RECENTLY RECEIVED.

Mind and Medicine. By Dr. W. H. R. RIVERS. Manchester: University Press. Pp. 23. 1*s*. net.

This lecture, like the author's *Dreams and Primitive Culture*, was delivered in the John Rylands Library and is here reprinted from the Library *Bulletin*. It is full of interest to the psychologist and sociologist as well as to the physician whom it may help to educate in the modern methods of truly psychological medicine. After showing how the struggle between the material and the mental schools of healing dates from the earliest stages of society, Dr Rivers proceeds to enunciate the important principle, and the practical consequences, of 'psychical determinism,'—the principle that "every mental symptom has its mental antecedent."

The Experimental Psychology of Beauty. By Prof. C. W. VALENTINE. Revised Edition. Edinburgh: Nelson and Sons. 1919. Pp. 128. 1*s*. 3*d*. net.

The second edition of this unique little work contains two new chapters dealing with music and rhythm. It gives an admirable resumé of experimental work in aesthetics, particularly that published by British and American invéstigators.

Shell Shock and Its Lessons. By Profs. G. ELLIOT SMITH and T. H. PEAR. Second Edition. Manchester: University Press. 1919. Pp. xv + 135. 1*s*. 6*d*. net.

This admirable little book, now in cheap form, the second edition of which was first issued in 1917, and reprinted in 1918, has clearly proved its worth. No better introduction to the subject has been written for the general public.

Studies in the History of Ideas. Edited by the Department of Philosophy of Columbia University. Vol. I. New York: Columbia University Press. 1918. Pp. 272.

This volume contains the following essays: *Appearance and Reality in Greek Philosophy*, by M. T. McClure; *The Meaning of ΦΥΣΙΣ in Early Greek Philosophy*, by Walter Veazie; *An Impression of Greek Political Philosophy*, by Wendell T. Bush; *Francis Bacon and the History of Philosophy*, by John J. Goss; *The Motivation of Hobbes's Political Philosophy*, by John Dewey; *An Attempt of Hobbes to Base Ethics on Psychology*, by Herbert G. Lord; *The Psychology of Ideas in Hobbes*, by Albert G. A. Baly; *Truth and Error in Descartes*, by Robert B. Owen; *Spinoza's Pantheistic Argument*, by William F. Cooley; *Berkeley's Realism*, by Frederick J. E. Woodbridge; *A Note on Dr Thomas Browne's Contribution to Esthetics*, by Adam Leroy Jones; *The Antinomy and its Implications for Logical Theory*, by W. P. Montague; *Old Problems with New Faces in Recent Logic*, by H. T. Costello.

As the titles indicate the contributions are of philosophical rather than of psychological interest.

Mathematical Psychology of War. By L. F. RICHARDSON. Oxford: William Hunt. 1919. Pp. 50. 5*s*. net.

This is an interesting attempt to express in mathematical language the behaviour of peoples at war. The principal factors here subjected to equations are the "vigour-to-war," warlike activity, vengeance, conquests, casualties and the destruction of wealth, business advantages, war as a source of income, fear, pain, fatigue, and the prospects of success.

Manual of Directions for Administering the Otis Group Intelligence Scale. With an introduction by Dr L. M. TERMAN. Yonkers, New York: World Book Co. 1919. Pp. 37. 25 cents.

This Point Scale was devised by Dr A. S. Otis in 1915 for the measurement of the mental ability of groups of individuals simultaneously and was applied by him during that year to thousands of Californian school-children. The publishers claim that "it does not require the services of a trained psychologist either to apply the scale or to obtain dependable results." The material supplied consists of (i) an examination booklet, in two forms, each containing ten tests, suitable for children of the "fourth grade" and upwards, and adults, (ii) individual record cards, for the tabulation of all relevant data obtained, (iii) log slips for entering the data of each test, (iv) the examiner's Manual, giving general information and instruction in regard to the tests, and (v) the examiner's Key, for correcting the answers by the use of stencils. The tests are of the usual character, *e.g.* 'opposites,' 'disarranged sentences,' 'analogies,' 'narrative completion,' and the relative standing of individuals is given by the 'intelligence quotient,' 'percentile rank' or by the 'coefficient of brightness.' The most successful are classified as "'near' genius or genius"!

La Psychologie du témoignage. Par J. VARENDONCK. Gand: Maison Ad. Hoste. 1914. Pp. 196.

The author is lecturer in 'the international faculty of pedology' at Brussels. He gives an account of the work of Stern, Lipman, Binet, Claparède, Jung and others on the psychology of evidence, and a bibliography of nearly two hundred papers.

Psychologie générale tirée de l'étude du rêve. Par ALBERT KAPLOUN. Lausanne: Payot et Cie. 1919. Pp. 205. 4 fr. 50 c.

The writer makes free use of the following terms, *moi central*, *pointe*, *fonction explicatrice* (referred to as F.E.), *moi automatique*, to construct a fantastic 'psychology' devoid of foundation on actual experience.

The Dream that Comes True. By J. NAPIER MILNE. London: The Epworth Press. 1919. Pp. 192. 5*s.* net.

Occupational Therapy Applied to Restoration of Movement. By Major B. T. BALDWIN. U.S.A. Occupation Therapy Department. 1919. Pp 67.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

General Meetings.

May 31, 1919.	On Listening to Sounds of Minimal Intensity, by F. E. Bartlett and E. M. Smith.
	The Relations of Aesthetics to Psychology, by E. Bullough.
July 12, 1919[1].	Instinct and the Unconscious, by W. H. R. Rivers, C. G. Jung, C. S. Myers, J. Drever, Graham Wallas and W. McDougall.

Sectional Meetings.

(*a*) *Educational Section.*

April 11, 1919.	Psychology and Education, by T. P. Nunn.
May 7, 1919.	Mental Tests for Vocational Guidance, by C. Spearman.
June 18, 1919.	The Mental Factors involved in Memory Drawing, by P. B. Ballard.

(*b*) *Industrial Section.*

April 25, 1919.	Psychology and Industry, by C. S. Myers.
July 17, 1919.	Some Impressions of Industrial Psychology in America, by B. Muscio.

(*c*) *Medical Section.*

May 15, 1919.	Psychology and Medicine, by W. H. R. Rivers.
June 11, 1919.	The Generation and Control of Emotion, by A. E. Carver.

[1] In conjunction with the Aristotelian Society and the *Mind* Association.

VOLUME X MARCH, 1920 PARTS 2 AND 3

ON LISTENING TO SOUNDS OF WEAK INTENSITY

BY E. M. SMITH AND F. C. BARTLETT.

(*From the Cambridge Psychological Laboratory.*)

PART II.[1]

D. VARIATIONS IN THE SOUND AS EXPERIENCED.

1. *The Classification of Sounds.*

IN the early stages of the experiment both subjects frequently endeavoured to classify the sounds heard. The value of this classification as an aid to listening will be considered later (see p. 139).

S. began by distinguishing two classes of sounds: (i) the aggressive and (ii) the diaphanous. Aggressive sounds "have a kind of weight," and are "in the ear." Diaphanous sounds are characterized as "far away." "When an aggressive sound comes, you react very quickly so as to brush it away."

Such attribution of weight to sound recalls a similar characterization in the case of colour. Just as the darker colours appear heavier than

[1] The first part of this paper appeared in this *Journal* x, 1919, 101–129

the lighter, so to S. an aggressive sound seemed to possess weight as compared with other sounds. Bullough suggests that the apparent weight of colours may be due to increased pigmentation[1], and the weighted sound may be another direct illustration of the same principle; that is, an aggressive sound may appear to possess 'more of' a certain quality than the diaphanous sound. But neither in the case of colour nor in that of sound is this an adequate explanation of apparent weight. The specific quality may be pigmentation in the case of colour, and it may be a blend of intensity and mode of production in the case of sound. But the real problem in either case is as to why particular qualities become connected with apparent weight.

It is also unsatisfactory to dismiss the association of weight with a perceived 'moreness' of a certain quality as due to "unreflective and immediate inference." This is what Bullough is inclined to do. But everyone must have had experience of the extreme readiness with which the adult observer, working under experimental conditions, tends to speak of "unreflective and immediate inference." Many such cases are merely instances of unanalysed modes of response which are both complex in themselves, and the result of a long process of development. Extremely careful inspection is needed to render their complexity apparent, but an observer who is both practised and cautious will find himself seldom forced to admit an inexplicable simplicity in his experience.

The attribution of weight to colour is probably inferential only in the sense in which any compound sensory experience whatsoever is inferential. There are many conditions under which experience belonging to one sensory mode may give rise to or be accompanied by experience belonging to another sensory mode. The most frequently discussed illustration of this is the synaesthesia which occurs in many persons between sound and colour. But there is no good reason for holding that this is the only type of synaesthesia[2]. Thus it is possible that the attribution of weight to sound may be another illustration of the same type of experience. If this is the case the problem falls into line with the general question concerning the precise relation between sensations belonging to different modes.

At the same time a further possibility is open. Sound and colour are known to be closely related in experience, and so also are weight and colour. It may be that it is owing to the close sensory connexion which holds between sound and colour that an attribute far more commonly

[1] This *Journal*, II. 151. [2] Cf. *e.g.* 145.

applied to the latter came to be assigned by S. to the former. The question awaits further investigation.

In S.'s early tentative classification which was speedily elaborated, varieties in the sound were related to characteristic modes of the response. This gave rise to the following scheme:

The Sound.	The Response.
(i) Aggressive;	(1) Automatic.
(ii) Certainly present, yet	(2) Has to be decided upon.
(iii) Definitely heard as a sound, appearing objective, but not recognized as that of the buzzer (cf. p. 138).	(3) Varied, sometimes of class (1), and sometimes of class (2).
(iv) Something of which I am entirely uncertain: "There is merely something doing and oscillating" (cf. pp. 143 *seq.*).	(4) Generally not made.
(v) Positive silence (cf. pp. 145 *seq.*).	(5) Not made, but definite relaxation of strain.
(vi) Absent, and yet no sensation of positive silence.	(6) Not made.

In the next classification attempted, attention was concentrated on the characteristics of the sounds themselves, not on the nature of the response. The sounds were then classified as:

(i) Thrusting, (ii) definite but not thrusting, possessing colour, weight, body or volume, and warmth; (iii) very diaphanous, recognized as a sound, but *not heard*, and possessing none of the qualities of (ii), or "if they are present they are not marked"; (iv) cases in which there is complete uncertainty as to whether the sound is present, but "something" is present.

Two points are particularly noteworthy in reference to this classification. The first is the reappearance of an apparent weight of sounds. Here weight definitely appears together with colour, while it is at the same time distinguished from volume. So far as could be ascertained, the weight characteristic was more closely connected with the experience of the onset of a sound than with anything else, and it appeared to be very intimately related to a form of tactile experience.

The second point is that for S. differences of quality (or timbre) were particularly striking. The observer commented on this herself. "A lot of the differences I have described," she said, "are probably differences of timbre. The sound becomes decidedly more complex as it becomes louder, and particularly it gains new low components. The most important thing is the *force* of the sound, by which I mean its initial intensity." And again: "I can't say how far the aggressiveness of sounds is due to (i) intensity or (ii) timbre. If one particular component of the sound is present I get aggressiveness, no matter how faint the sound may be."

Very much later in the course of the investigation, S. gave a further description:

"A loud sound has two parts: (i) a nucleus, and (ii) a penumbra. The nucleus takes longer to develop than the penumbra, the latter being present all the time. The two seem to have nothing in common except the fact that they are both sounds. With faint sounds the nucleus is absent. To this is due any hesitation and wondering whether to react or not. The penumbra is present and is perceived, but it only prepares you for the nucleus, which does not emerge."

It is clear then that in the case of a complex sound, such as the tone of a buzzer, a variation in intensity involves many other variations also. To S. these are distinctively variations of quality, but they appear to be intimately related to forms of experience other than auditory.

B.'s first classification and analysis of complex sounds of decreasing loudness were as follows:

(i) Some sounds are easily perceptible, but are complex. There is a low part, like a kind of hum, and a high part, like a kettle beginning to boil. The higher part is slightly more discontinuous than the lower part. These sounds are localised as a little above and behind the ear.

(ii) A second class is still definitely perceptible as sounds, but is more discontinuous; or rather it is a wave effect more than definite discontinuity. The sound never disappears completely, but it comes and goes. The low component becomes less marked. At this stage, the sound seems to move somewhat upwards; a result which may be associated with the loss of the low element[1].

(iii) Then the sound becomes yet more definitely discontinuous and does not develop until some time after the signal has been given.

(iv) At times the low component now disappears entirely[2]. The high part remains, but loses the characteristic of being localised.

[1] It is interesting to note that Rayleigh maintained that high pitches produce a relatively discontinuous or fluctuating effect. B. was inclined to attribute the apparent discontinuity in this experiment to the weak intensity of the sound, but the fact that high components in loud sounds appeared relatively discontinuous may suggest that pitch has something to do with the matter.

[2] Hancock, in "The Effect of the Intensity of Sound upon the Pitch of Low Tones" (*Psychol. Monog.* XVI. 161–8), reports that with a tuning-fork of 128 v.d. as source of sound, the louder sound is always judged lower than a fainter sound of the same pitch. He found the effect much less marked as the sound was raised in pitch. The tone of the buzzer

(v) Next there is no sound at all; but only tactile sensations in the ear, felt but not heard. (This stage appeared for the left ear only, which, in B., is distinctly of subnormal acuity.)

(vi) Finally the rattling effect of the tactile sensations is lost, but there remains something indescribable.

B. reported that in passing from stage (i) to stage (ii) there was a sensible diminution of intensity, but that in the subsequent stages it became very hard to say whether any further changes in intensity were taking place.

Very little was added to this early analysis. On several subsequent occasions B. remarked that faint sounds appeared higher in pitch. He felt that some relation subsisted between this and his tendency to localise faint sounds high up, but he could not satisfy himself as to the nature of the relation.

When B. turned his attention to the actual attitude adopted in listening, however, his suggested classification had several points in common with the attempts made by S.

(1) "Some sounds," he said, "come in with a burst; there is something explosive from the outset. The attitude is 'here it is!'" These, no doubt, are the sounds which S. called aggressive.

(2) "Some sounds appear merely to slide in. The attitude is 'here it comes!'"

(3) "Some sounds are merely 'there,' in a ghostly sort of manner, and you don't know how they got there. They seem to melt in. The attitude is a slightly hesitating: 'Yes, here it is.'" These appear to coincide with S.'s diaphanous class.

(4) In some cases there is absolute uncertainty: "If the sound is there at all, it seems to be there all the time. The attitude is 'Well it *may* be here.'"

Thus both observers agreed in attempting to classify the sounds presented. In each case the classification was usually effected by reference to characteristics other than that of differences of intensity. To B. differences of quality by themselves appeared perhaps less striking than to S., while specific differences in pitch and discontinuity received from him greater emphasis. Differences in the mode of response and in the listening attitude occurred in both subjects.

The significance of this particular selection of characteristics as a

used in the present experiments is not really comparable to that of a tuning-fork, but it is interesting to find that both observers remark that the low component is most marked when the whole complex of sound is loud.

basis for classification will become clearer when a further point is considered.

2. *The Identification of Sounds.*

There can be no doubt that whether a response is, or is not, made to a presented sound largely depends upon whether the sound can be recognized. Auditory acuity, at least as determined by our experiments, is by no means a function merely of successful sensory adaptation, but of ability to identify what is presented. Both S. and B. noticed this fact again and again in the course of the investigation. "In this series," remarked S. on one occasion, "the sound changed in quality, and I missed some on that account"; and again: "Several times I was convinced I heard a sound, and yet it was so different from the real sound that though I followed its coming in and going out, I did not respond." Later on she became more definite still as to the importance of identification: "I am certain," she said, "that my response depends very much upon familiarity with the sound, and this is particularly the case with the fainter intensities. I am convinced that with the weaker tones to hear is to respond to some sort of impression which is remembered, and not to the mere sound stimulus itself." B. was in entire agreement, and reported quite independently: "The sound was very irregular, and different from what it usually is. A good deal of the difficulty of the response was a difficulty of *recognizing* the sound"; and again: "Much of the difficulty of dealing with faint sounds is due to a difficulty of identification....In many of the instances in which I did not respond at all, I think I did hear a sound which, nevertheless, I was not able to identify."

It is interesting that both observers should agree that identification tends particularly to be called upon in the case of faint sounds. This, indeed, is precisely the conclusion already suggested by a study of the actual results secured in relation to the fineness of grading and the range of the intensities used[1].

Now it is fairly clear that for purposes of recognition, mere differences in intensity must be entirely inadequate. Intensity itself, save perhaps at the extremes, is but a faulty individualising mark, so that to attempt to identify a sound by intensity alone would be something like trying to identify a man by the length of the hair on his head. What is required are the most clearly marked specific characters of the sound to which response has to be made. It is no wonder, then, that a classification of

[1] Cf. Part I of this paper, p. 114.

sounds proceeds largely upon a basis of differences of apparent quality, of correlated differences in the impulse to respond, of the varied manner in which sounds appear to break in upon the silence, and of divergences in apparent localisation.

The marks just enumerated are the very characters by means of which a sound may be individualised most definitely, and discriminated from other sounds. There seems therefore to be no doubt that the tendency to classify sounds (which was particularly active in the early parts of the experiment) was determined by the necessity for identification. It is especially noteworthy that, while the classification was carried out from the very beginning, the value of the recognition of sounds was not commented upon till much later. That is, individual characteristics were singled out and made use of long before their peculiar function in hearing had become clear to the listener. The process forms a very clear-cut illustration of how in experience practical analysis often precedes intellectual, and factors may even come to be used in an almost mechanical way, before the actual value of their use is made apparent. This is one of the processes according to which reactions come to be spoken of as 'natural' and 'inexplicable' when actually they have a perfectly definite history and explanation.

3. *The Localisation of Sounds.*

The preceding remarks on the classification of sounds have already indicated that localisation occurred in many instances. Three different questions must be considered:

(i) What were the determining factors in binaural localisation under the conditions of this experiment?

(ii) Were all sounds given like treatment in this respect?

(iii) Apart from lateral displacement from one ear to the other, were sounds accorded distance and direction of movement, and if so why?

In considering the evidence collected with respect to these three points it has to be remembered:

(*a*) That the sensitiveness of the telephone diaphragms used was not quite the same, that of the right-hand one being very slightly superior;

(*b*) That there were well-marked differences between B.'s two ears, the left ear being very subacute;

(*c*) That S. appeared to be subject to prolonged reversals of the ratio of acuity of the two ears, so that the right ear was the more acute in series I, the left ear slightly better in series II, and the left ear very much better in series III[1].

A careful analysis of the reports shows unmistakeably that while localisation is extremely common, there is very little constancy of reference of sound to a given position between the two ears or in either ear. Selections from some of the remarks made by the observers will demonstrate this.

Taking S. first, we find that on various occasions the following statements were made:

"With the clicks (see Part I, 105) I heard sound between right ear and middle of head. With continuous sound heard definitely in left ear."

"Heard continuous sound in right."

"First heard sound at back of head, then loud continuous sounds on left. Clicks remained localised at back of head."

"Sound in left ear generally, but continuous sound far less definitely localised than click; former tended to move over head from ear to ear."

"All sounds on left."

"All loud sounds on left; weaker ones inside head."

"Sound first on left; then passed to various positions in centre of head. Loud sounds nearer centre than faint sounds, which were in or outside left ear."

"Adjusted left-hand receiver at the beginning. This made sound appear to pass over to left. Then localised it above head."

The variability of localisation which was thus evidenced in the case of S. was equally marked with B., if allowance is made for the fact that the acuity of his left ear is far below normal.

"Faint clicks localised at back of head. Louder clicks in ear or towards front of head."

"Clicks are more definitely localised than continuous sound. Localisation appeared to depend on quality of click. Sometimes sound appears at back of head, sometimes just in front of ear and sometimes in ear."

"The variety in the localisation of the clicks depends on the quality of the sound."

"Clicks localised at back of ear; such clicks have a hollow sound."

"About middle of series suddenly realised I was localising sound at back of head; did so repeatedly."

"Faint sounds appeared far back near the centre of the head, loud sounds in the right ear."

[1] Cf. Part I of this paper, 110.

From a study of these reports it appears that in answer to the first of the questions above formulated we may suggest:

(*a*) That the tendency to localisation which arises under the conditions of this experiment is not determined by any simple and constant factors, but by a complex of conditions which produce different results on different occasions[1].

(*b*) That one of the most important of these conditions is differences in sound quality, although it may possibly be the case that differences of quality operate only in producing a general tendency to localise, and not in fixing a definite position[2].

(*c*) That there is little evidence of lateral displacement as a consequence of such relatively small differences of intensity as were secured by the variations in the sensitiveness of the telephone receivers used.

(*d*) That the localisation ascribed to a sound may be very definitely influenced by contrast when some other sound has already been localised. This at least is proved to occur when one sound is continuous and the other discontinuous, or when one is loud and the other faint.

The answer to the second question is very definitely in the negative. It has been maintained that the adult leaves no auditory impression unlocalised[3], and that every sound impression carries some 'place idea' with it, though the latter may be extremely vague. Our observations suggest that this is far too general a statement. Conclusions arrived at in auditory experiments have always to be considered definitely in relation to the particular character and mode of presentation of the sound used. The sound stimulus employed by McGamble was a telephone click, and our observations have shown distinctly that in telephone-listening discontinuous sounds appear to be much more readily and definitely localised than continuous sounds. Indeed, in the latter, localisation appeared often to be entirely absent.

[1] Starch's introspections concerning "The Perception of the Distance of Sounds" (*Psychol. Rev.* XVI. 1909, 427–31) tend to confirm the complexity of the determination of localisation. Differences of intensity, of pitch, and of quality, for different distances, were all operative, and, particularly when one sitting was compared with another, did not always appear to operate in the same manner.

[2] Pierce (*Studies in Auditory and Visual Space Perception*, 1901) made tests with organ pipes and tuning-forks which suggest that judgments of distance are affected by tonal complexity, the richer tones being judged nearer than those more nearly pure.

[3] See E. G. McGamble, "The Perception of Sound Distance as a Conscious Process," *Psychol. Rev.* IX. 1902, 7–13, 353–73; and cf. Geissler, "Sound Localization under Determined Expectation," *Am. Journ. of Psychol.* XXVI. 1915. Geissler found that only three of his observers attributed a spatial characteristic more or less explicitly to sound. For the others sound localisation was an indirect function depending on the spatial characteristics of secondary criteria, such as associated visual or kinaesthetic processes.

But besides being given a position in, or between, the two ears, sounds were often referred to a distance from the observer, and were frequently said to possess directed movement.

Some of the reports made may be quoted in illustration:

S.: "To-day I interpret any variation in the character of the sound chiefly as a distance mark. The sound is nearer or farther, not louder or fainter[1]. Definite sounds seem to be directed straight at the ears; while the faint sounds appear to be moving at right angles to the ears."

"All sounds have direction, movement, and speed. The definite sounds move more rapidly, the faint sounds less so, both coming towards the ear. There is a further class of sounds moving at right angles from front to back. Sensations which are not sounds (cf. pp. 143 *seq.*) appear as small particles moving up and down in a streaming manner."

"The distance marks of sounds are very definite. If the sounds are muffled in quality, they may possess double distance marks. They seem submerged as well as at a distance in a horizontal direction. As they become more intense they rise to the surface."

"The upward and downward distance marks are most pronounced in a regular ascending series. As the sounds become louder, they gain in buoyancy. When the sounds are given irregularly, the inwards and outwards marks are more prominent. When the sound is excessively loud it is regarded as inwards."

B. reported experiences of the same kind: "The faint sounds," he said, "are higher up. The strong sounds are lower down but I can't determine the real basis of this judgment." And again: "A faint sound is farther away and higher up than a loud sound[2]."

It thus appears that the third of the questions formulated at the beginning of this section receives an affirmative answer. But

[1] Contrast E. G. McGamble's results: She gave pairs of stimuli which either were given at the same distance from the subject but were of different intensity, or else were of the same intensity but were placed at different distances from the subject. McGamble reports: "Judgments of 'nearer' and louder,' of 'farther' and 'softer' proved to be particularly interchangeable. The subjects showed a marked tendency, however, to say more often that a sound was louder when it was louder only in virtue of being nearer, than to say that it was nearer when it was merely louder, and so also, *mutatis mutandis*, with the judgments of 'softer' and 'farther.'" "Intensity as a criterion in estimating the distance of sounds," *Psychol. Rev.* XVI. 1909.

[2] Angell in "A Preliminary Study of the Significance of Partial Tones in the Localization of Sound," *Psychol. Rev.* X. 1903, 4, remarks: "The issue is really somewhat ambiguous. Richer sounds may ordinarily be judged nearer than those more nearly pure. The upper partial tones of a complex sound may be relatively more prominent when the sound is heard from a distance, and still the total sound effect be poorer and less full than when the sound is heard near at hand."

whether a distance mark is to be regarded as an original characteristic of certain sounds, or whether it should be treated as a secondary characteristic applied to sounds by virtue of their possession of other qualities or attributes, is a question hard to decide. S. had some tendency to experience sounds as active, and it is not difficult to see how a distance mark might grow out of an attributed movement. Sometimes also she was inclined to endow a sound with some sort of visual form, but in this case it appears more probable that the form was dependent on the distance mark rather than that the distance mark was dependent on the form. What seems quite clear is that the distance mark was not solely determined by differences of intensity. The tendency present in B. to assign a higher position to a weaker sound may have been due to the fact that a weaker sound almost always appeared slightly higher in pitch. But this explanation by association did not appear to the subject to be entirely satisfactory.

On the whole, the evidence tends to show that while, under the conditions of the experiment, position, distance, and movement marks may be very readily attached to the sounds, they are probably secondary characteristics only and arise through an interpretation of other perceived characters. Whether this is so or not, such marks certainly serve an important function in aiding identification. Indeed, in the opinion of one of us, there is no more effective or more frequently used aid to identification than the position, distance, or movement marks of a presented sound.

4. *Sounds which are perceived but not heard.*

When a stimulus is too weak to evoke an auditory sensation, nevertheless its presence is often appreciated. This of course has been noted by other workers and we made many attempts to determine precisely the character of the experience.

To S. the sound which was perceived but not heard often appeared as a temperature sensation, especially in left-ear listening during the early series. Now at this time the left ear was in a temporarily subacute condition, and possibly there may be some connexion between this fact and the thermal sensation[1]. Sometimes the temperature sensation appeared as a preliminary stage in the development of the auditory sensation proper: "In the ascending series," S. reported, "temperature sensations preceded auditory." The thermal sensation was most marked near the threshold of acuity, and was often present when no auditory

[1] Cf. the tactile sensations experienced by B. in his subacute ear, 137.

experience could be discriminated: "There was a period when I heard nothing....Then I realised that there were differences within my experience. I realised that something formless was present which was not a sound at all. It seemed to be a kind of distention of some part of the ear, but of what part I do not know. In these cases the slight temperature sensation, as if something just warm was put on the ear, was noticeable." The sensation was said to "resemble a slight flush of excitement more than anything else, as if a small sound caused an increased blood supply."

But again the non-auditory perception of sound appeared as the upsetting of a balance. "I often," said B., "responded to something that was not a sound, but yet was something positive. Such cases are expressed as a kind of balance inside the head which seems to be chiefly motor in character, and is probably connected with a tendency to jerk the head over to one side or the other." This experience may be of the same order as the one described by S.: "I had a feeling that the sound was there, and trying to get through, but could not. The effect was that of a 'frustrated' sound"; but perhaps the epithet is here transferred from the hearing to the sound. Both of these descriptions show clearly that the experience in question is not that merely of a tendency to concentrate attention, but of a tendency determined from without[1].

A third subject, in describing the experience, used as an analogy the case of touch: "It was as if somebody should take a fine hair, and pass it over the surface of the skin. You know that there is a touch, but you can't feel it."

There is an unquestionable positive basis for this class of experiences. Response to sounds not heard was reported in twenty-three different series of trials. On only one of these occasions was there any reaction when no stimulus was presented.

When we turn to a consideration of the precise character of the experience in which a sound which is not heard is yet in some way perceived, we are at once reminded of the fact that auditory sensation may take an appreciable time to develop its full effect[2]. A certain summation of effects is required before an experience becomes definitely recognizable in terms of sound. But before that point is reached *some* effect is certainly produced, and the evidence clearly indicates that this effect may be sensory in nature. Hearing, that is to say, is really a

[1] Cf. *e.g.* Knight Dunlap, "Some Peculiarities of Fluctuating and of Inaudible Sounds," *Psychol. Rev.* XI. 1904, 308–18. Eight subjects examined by him responded to physical intensities after the sound had psychologically disappeared. His results are confirmed in a later article, *Psychol. Monog.* X. 1909, 16–25.

[2] Cf. Part I of this paper, 116–8.

process having a definite summation point, and only when that summation point is reached is the experience unmistakeably one of sound. The summation or blending of sensory effects, however, which at a certain point produces sound, may be expressed in other ways before that point is reached. The present evidence is to the effect that prior stages in the process may be marked either by thermal or by tactile sensibility. But under the conditions of the experiment, any such sensory experience gives rise also to a stiffening of the attention. This expresses itself on the motor side as a 'balance,' which, in B.'s case, seems to be inside the head. The suggestion then is that when an auditory stimulus is as yet too weak to give rise to auditory experience, it may produce effects referable to other sensory modes. That B. should emphasize the 'balance' factor was merely due to the fact, incident to these experiments, of a stiffening of the attention, and to the further fact that B. tends definitely to be of a motor or kinaesthetic type. We conclude then that in all cases in which a sound is perceived but not heard we have undoubted evidence of a form of synaesthesia.

This has an important bearing on the determination of the characteristics of a good listener. In various ways an inferential element enters into the process of listening to sounds, particularly to those of weak intensity. Clearly then the successful listener must be a person who possesses not only first-class auditory acuity, but who is able readily to appreciate the non-auditory signs of the presence of sound stimuli.

5. *The Experience of Positive Silence.*

Investigators have often called attention to the fact that the absence of a stimulus commonly induces an experience which cannot be adequately described in negative terminology. This received very definite illustration in the present experiments. One of our observers frequently noticed an effect which could be described only in the terms 'positive silence,' and which was very clearly distinguished from that accompanying the mere absence of sound.

Positive silence proved a compelling, absorbing, and satisfying experience. The subject S. was just as convinced by it that the stimulus was absent, as she was that the stimulus was present when she had perceived a sound that was definitely supra-liminal. Just as the attention aroused by the warning signal was relaxed after the reaction to a loud sound, so it was immediately slackened after the experience of positive silence. On the other hand, in cases where positive silence was absent, but the subject was unable to perceive a sound, the attention was strained either until the sound was perceived, or until the outgoing signal warned the subject of the end of the trial. It is true that under the conditions of the test, especially in the series where no accompanying signal was given, the subject after a time always became uneasy if no sound occurred, and attention was once more strained. Thus S. remarked that if there was a relatively long period of positive silence, the relaxation of strain which marked its early stages passed into a definite heightening of tension later on: "You feel that your eyes and ears are open. You hold your breath, and feel generally stretched towards the expected sound." This however was due mainly to the listener's knowledge of the routine of the experiment, the renewed strain being regarded as an intrusive and disturbing necessity, and as having no direct relation to the positive silence itself.

Positive silence possessed to an outstanding degree the characters of finality and of attractiveness. The subject wished to 'soak it in,' and to enjoy it to the full. Various writers, referring to what appears to be a more or less similar experience, all speak of it as compelling attention. Typical remarks are made, for instance, by Latzko, when he is describing the experience of a soldier home from the trenches: "There is nothing but a glorious quiet that you can *listen to* as to a piece of music! The first few nights I kept my *ears cocked* for the quiet, the way you try to catch a tune at a distance...it was so delightful to listen to no sound[1]." The phrase "a silence that could be heard" is, in fact, by no means unusual, though Titchener urges that 'felt' is the more correct verb to use[2]. It is however by no means clear what 'audible silence' precisely involves. Ward says that "the cases in which we might refer to the cessation of sound as audible are...cases of perception where consciousness when there is nothing to hear may still be auditory[3]." The suggestion appears to be that listening may continue when hearing has ceased, and may confer upon the resulting experience a positive character. In our case, however, the strain of listening was definitely relaxed on the occurrence of positive silence, and it does not appear to us that the positive character of the experience is to be correctly considered as perceptual only.

Titchener[4] has attempted definite experiments on the positive character of silence by subjecting observers for thirty seconds or more to

[1] *Men in Battle.* By Andreas Latzko, Eng. trans. 1918, 19–20: italics, own.

[2] *Journ. of Phil., Psychol.* etc. XIII. 114.

[3] This *Journal,* VIII. 1916, 216.

[4] *Op. cit.* 114.

the noise of machinery in his laboratory workshop. At the end of the set period, the noise was cut off as abruptly as possible. Various organic and kinaesthetic sensations were reported, and silence was experienced as "something *else* than sound or the cessation of sound." Our own experiments did not, of course, in any way repeat Titchener's conditions. The sounds presented were not loud, and they were of much briefer duration. The "dizziness," "change of breathing," "oppression in the region of the drumskins," "boring in the ears," and so on, reported by Titchener's observers, were doubtless due to the fact that his experiments involved the abrupt cessation of a loud noise. No such marks were present in our case. Nevertheless, we also observed that silence is "something else than sound or the cessation of sound."

For an explanation it is natural to turn first to the facts of contrast, and to explain positive silence as a heightened contrast effect. But by itself such explanation is inadequate; for what really calls for further analysis is the mode by which the contrast is made so peculiarly effective as to give rise to the experience of positive silence. In point of fact, contrast, in one form or another, comes in very frequently indeed throughout the whole course of experiments of the kind here reported. Consequently if, on occasion, contrast is found to be accompanied by so markedly new an effect as that of positive silence, such contrast obviously calls for some special analysis.

One character of the experience was very constant and arresting. Positive silence always appeared with notable suddenness. It did not always occur immediately upon the cessation of sound. But even when it was interpolated into the midst of a long empty-appearing interval, as often happened during the two-hour sittings, it came with extreme suddenness. In those tests in which a signal was used, the listener repeatedly reported positive silence immediately after the appearance of the warning signal, and the experience apparently took no appreciable time to develop.

Now it has several times been observed, both by us and by other investigators, that, in listening, the outgoing of a stimulus is more definitely and sharply marked than its initiation, much as in electrical conduction a break in contact is cleaner than a make. The suddenness of development of positive silence suggests that it is the result of the cessation of some sensory experience. But it is at the same time more than a positive *perception* of cessation. Change abruptly experienced very commonly indeed induces an emotional or feeling state, and when a sound which is *expected* is not experienced, the emotion is all the more likely to occur[1]. Expectation is not essential, indeed, for positive silence occurred sometimes in intervals between the signals. In these cases it may be suggested that there is a momentary suspension of some continuous excitation, possibly in the inner ear. When the situation is such that expectation also comes into play, the effect of this momentary suspension is the more marked. But in either case, the positive character of the silence experience, under the conditions of the present experiment, appears to be contributed largely by an attendant feeling or emotional factor, and not to be purely perceptual in nature.

It is not urged that this emotional character constitutes the whole of the experience here called positive silence. Were this the case, it would be possible to obtain an experience precisely similar, even when stimuli other than the auditory were being employed. There is good reason to believe that an experience which is emotionally similar can be obtained, but a part of the total result is contributed by the changes in the functioning of the *particular* sense organs involved. Further investigations are needed to show definitely, for example, how the total mental reaction to the cessation of light differs from that occurring in the case of positive silence.

6. *Subjective Sensations.*

The existence of false responses, together with their necessary accompaniment, the subjective sensation, has already been demonstrated. But it is of some interest to consider them further solely with reference to the observers' remarks concerning their nature.

The tendency towards subjective sensations may no doubt have been increased by the conditions of the experiment, in which sounds were produced at more or less regular intervals. Thus S. once remarked: "After a certain number of sounds which were easy I noticed regular pulses of attention. Then it was difficult to refrain from responding to subjective sensations, when the time came for directing attention to the sound." Again B., whose natural type of reaction is muscular rather than sensorial, found that merely having his thumb and finger on the

[1] On some occasions such absence may even give rise to intense fear. This was the case in some experiments carried out by one of us on guinea-pigs, where light and sound stimuli were simultaneously presented. After prolonged experience with the combined stimuli, the use of the sound stimulus was discontinued, the light alone being presented. In every case, the animals at first exhibited symptoms of the greatest fear, and the reaction time was enormously reduced. Even the experimenter, although controlling the situation, and hence in some measure prepared for it, always experienced a certain amount of shock the first time that the light was presented without its associated auditory stimulus. The experience was more startling than was that of the actual sound itself.

reaction switch predisposed him to subjective sensations: "I found it a good plan, during an unfilled interval, to take away my finger from the switch. This dispelled any tendency to subjective sensations which I might have had." To B. it seemed that subjective sensations appeared more readily in the dark.

Although both observers were sure that their experience was properly sensory, there was no evidence that it could be considered as analogous to visual after-sensations. On the whole, relatively loud sounds were less likely to be followed by subjective sensations than were those of weak intensity.

It is interesting to consider whether with practice a listener can distinguish between subjective and objective sensations with any degree of accuracy, and on what the attempt to do so is based. B. reported nineteen sittings in which he was much troubled by subjective sensations. During these sittings he responded to forty-eight catches in all. On seven occasions he definitely reported "no subjective sensations." These seven sets of trials produced nineteen responses to catches. This gives only the very slightest support to the suggestion that when the subject is on the look out for subjective sensations his response is likely to be more accurate. S. reported strong subjective sensations twelve times, and during these twelve sittings responded twenty-one times to catches. Only twice did she definitely assert that she had no subjective sensations, and on one of these occasions she responded six times to catches. Again we receive but a very slight support to the suggestion that a listener who is on guard against subjective sensations may be somewhat less likely to respond when they occur.

The basis of such discrimination needs further investigation. B. often relied on the perception of the oncoming of the sound: "A subjective sensation," he said, "has no exact point at which it breaks into the silence." S., on the other hand, found the outgoing of an objective sensation easier to notice than its incoming[1]. Consequently she would

[1] Knight Dunlap, *Psychol. Rev.* XI. 1904, 308–18, reports that a sound so physically weak as not to be heard may yet be heard to stop. He states further that the stoppage of a minimal sound is more easily observable than its starting. S. confirmed this observation on a large number of occasions.

It should further be remarked that in the two-hour sittings we found far less lag in the reaction to the outgoing of very weak sounds, even when they had continued for five minutes or more, than to their incoming.

The fact that the outgoing of a faint sound is frequently noticed, even when the sound has not been heard, in some instances leads to response after the stimulus has ceased. This fact undoubtedly accounted for a certain proportion of the 'false' responses in the tests where no signal was used.

note the outgoing of a sound of a particular intensity, and then endeavour to identify the sound when it reappeared. As the character of subjective sensations was usually constant for a given set of trials, this plan appeared to work fairly well. Another observer used the same device: "Sometimes," he said, "I felt there was no sound, and then I noticed when it stopped, and knew that there had been something. The absence of the sound was more noticeable than its presence."

B. once reported: "I find that the subjective sensation appears to be further inside the ear than the objective sound. Also this morning subjective sensations were very distinctly higher in pitch and different in quality." But the appearance of such differences cannot always be relied upon.

It is thus clear that subjective sensations definitely occur, that they may possess all the vividness of actual sounds due to external stimulation, and yet that by careful scrutiny they may to some extent be prevented from producing catch responses.

E. The Chief Determining Factors of the Reactions.

1. *General Determining Factors.*

The analysis of the character of the sound experience has already demonstrated that, under the conditions of the present experiments, the response of the subject may be determined by many factors other than that of the bare perception of the sound itself. It is interesting to consider these factors in detail, to study the manner in which they function, and to enquire at what period in the course of the experiments a given factor attains its greatest influence.

We shall consider first the evidence of the direct reports furnished by the observer B.

(i) In the early stages of the test this subject tended to confirm his perception of the sound by vocalisation, and not to react until such confirmation had been given.

"There is," he said, "always a tendency to vocalise during the listening period. Having heard the sound I feel inclined to say: 'Yes, there it is.'" But a month later the observer reported: "The vocalisation which accompanied the early stages of the test has almost entirely disappeared."

(ii) At the beginning the response was partially determined by an awareness of the nature and extent of muscular contraction. "The amount of muscular contraction present," said B., "seems to be considerable. There is an impulse which starts in the ear, and seems to travel across the shoulder and down the arm. This may be fanciful, but I have an impression that something is going on, and all the time there is consciousness of movement in the throat. These things all come only when there is a sound." And again: "When the sound comes, first I pinch the switch, then my fingers get tense, then the switch is pushed over."

The awareness of muscular strain became far less prominent with practice, and in so far as it remained it ceased to be widely distributed, and was localised simply "in the head."

(iii) Variations of mood, or of conscious attitude, played a large part, particularly in the early stages of experimentation.

References to moods appear very frequently indeed in the reports, and the moods themselves are very varied: "When I get an unmistakeable sound, something seems to happen at the back of the head. There is excitement, and a feeling of sudden release, but no consciousness of making up my mind. If two or three sounds of which I am certain follow one another, I get a feeling of confidence, a pleased feeling, a feeling of doing well. This does away with the strain, and also with the expectation of what is coming next. When I am uncertain, no response can be taken as complete in itself. As soon as one trial is over I am looking forward to the next....Sometimes I make a response just because I want to respond. This particularly occurs after several cases in which I feel tolerably sure a sound was present, although I am not able to hear." Again: "The whole of my attitude is dominated by a desire to be able to hear, and if I don't hear the desire is carried over to the next trial. If I do hear, I am more satisfied, and in a more favourable position for the next trial. The feeling of certainty appears to follow on a consciousness of muscular contraction in the shoulders, arms, and throat."

Far more than is the case with the other factors noted, the conscious attitude may vary its character within the limits of a single set of trials. It seems, however, to be very readily determined by something that occurs towards the beginning of a series. Seeing that conscious attitude may certainly exercise a definite determining influence upon the character of judgments passed, it may well be that the fact that the mood is frequently set up during an initial period may have something to do with the dominant effect of the first few of a series of judgments. It has often been remarked that when a number of judgments have to be made successively the character of the first two or three is very apt to set the direction of all of the others. Suppose some early judgment is chiefly determined by a mood or attitude having a marked accompaniment of feeling-tone. When the perception of a situation gives rise to feeling, even when the situation changes somewhat, the feeling often tends to remain. Further, feeling which properly attaches to certain definite cognitive details easily spreads over associated details. Moreover, what is thus true of feeling-tone is true, in precisely the same sense, of the mood to which the feeling may be attached. Thus if an early judgment in a series is largely determined by mood, the persistence and spread of the latter readily account for the fact that successive judgments tend frequently to be like the first. B. once reported: "My mood was somewhat reckless, and I responded to more than usual. This I believe was started by a response early in the series. Then, more or less involuntarily, I replied to what I immediately realised might have been a catch. As I was not sure, however, I simply fell into the attitude: 'Well, I might as well keep it up'."

Except under conditions which will be considered later (see pp. 160–2) the conscious attitudes became less prominent factors in the determination of B.'s reactions as he became more practised in the task of listening.

(iv) In the case of a continuous sound, differences within the sound experience itself, at different points of its presentation, often determined the response.

"There are two maximal points for the sound," said B., "first the moment of onset, and second a moment or so after the onset, when the sound appears to swell out."

(v) The length and frequency of empty-appearing intervals were important from first to last.

This factor was particularly appealed to in the cases of uncertainty as to whether a sound were present or not: "What constituted the difference between cases in which, in spite of uncertainty, I did respond, and cases in which I did not, I can't say for certain, but I think whether I have been hearing sounds or not just before is important. When I get a fairly loud sound, followed by a pause, and then a sound which I am not able to identify, I respond simply on the ground that it probably is the sound, because the long pause suggests considerable adjustment of the apparatus[1]. It seems to me that it is this sort of unexpressed reasoning which has a good deal to do with whether I respond to the uncertain sounds or not."

[1] The experimenter always endeavoured to make the intervals of presentation as irregular as possible, so as to counter this tendency on the part of the subject.

(vi) At first no response appeared purely mechanical, but after long practice the process of reacting occasionally quite escaped attention.

It is interesting to trace the development towards automatization throughout the reports. Very early B. said: "I could not clearly determine the conditions of the response, but am convinced that no reaction was purely automatic." Then a little later came the remark: "In the case of sounds heard very distinctly, the response appears almost automatic. But there is some kind of factor present which it is very difficult to catch or to describe." A month later he said: "Two or three responses were absolutely automatic, and I knew nothing about them till they were over." To the very end, however, after fairly constant practice extending over six months, B. remained doubtful whether complete automatization was reached. One of the latest reports states: "I was thinking of other things. I find however that though it appears that the whole process goes on mechanically, yet whenever a sound—even a loud sound—comes in, there is a swinging back of the attention. So I don't think it is a perfectly automatic response."

(vii) Occasionally a tendency to rhythmic response was aroused.

"At one period," said B., "I got into a regular rhythm of response." This tendency appeared to be a mode of reaction more or less specific to a particular sort of mood: "When one gets into a dreamy attitude, it appears to be a kind of rhythm which determines the response. One alternates from the case of responding to most things, to that of responding to nothing unless one is dead sure, and so back again. This sort of condition is unfavourable to listening." The concluding remark of this report appears to be well justified, and is borne out by a study of the results. The tendency to rhythmic response leads both to false reactions, and to a high threshold.

Taking all these different factors, (i) and (ii) were operative at the beginning of the experiment; (iii) was present throughout, but was chiefly important in the early stages; (iv) and (v) were present from start to finish, and were, if anything, more important in the later stages; (vi) came only after considerable practice; (vii) was not present to any great extent, but tended to be more important in the later than in the earlier stages.

When we turn to S.'s reports, we find that very much the same factors are referred to, but that they are not by any means arranged in the same order, either as regards time or apparent importance.

(i) There was throughout no reference whatever to vocalisation.

(ii) Awareness of muscular strain entered a month after the onset of the experiments. Even then its chief function appeared to consist in the determination of mood or attitude, the latter being directly related to the response. Also S. was never conscious of strain localised in the shoulder, arm or hand, but only in the head, ear, eye, and, in the two-hour sittings, in the neck.

(iii) Moods and conscious attitudes appeared as determining factors from the beginning, and persisted as such till the end.

S. had a far greater wealth of mood than B., and her reactions also appeared to be more readily determined by the attitude factor. In her second sitting she said: "I am sure that the responses are determined largely by a feeling of confidence, or of diffidence. Hence after a loud sound I reply the more readily, because I feel buoyant then." Again remarks such as: "When a moment of acquiescence becomes a moment of conviction, the response comes automatically," were frequent. And at the end of the experiments the importance of attitude continued: "I was very eager to respond to all sounds. As a rule, I took the least hint of a sound, and responded without deliberation. There was a pleasant absence of boredom, and a general sense of well-being."

On the other hand, it is certain that S. experienced great variety of moods which appeared to produce no effect on the results. The most striking illustration of this occurred on a day on which a somewhat restricted range of fairly weak intensities was presented. Throughout the whole sitting the subject had very few sounds of which she could feel in the least degree certain. Her report described the moods thus induced in great detail:

"The strain was terrific. There was a period during which I heard nothing, and wondered if I was going deaf. I then realised that a formless something was present which was not sound. At the same time I felt as if nothing really was present. I gripped the table, and presently got used to the experience, and imagined I was having subjective sensations only. Then I got a fit of absolutely choking rage. I had a feeling of rigidity all down my hand, and my fist was clenched....This lasted for a while, and I could not attend at all. There was a most extraordinary feeling of being thwarted, accompanied by a horrible feeling of hopelessness which took all my attention away from the sound, and would have prevented my hearing it anyway....The alternation between hopelessness and more or less indifference came frequently. Sometimes I did not know when I was listening, and when not. For the first time the consciousness of my own breathing seemed to hinder listening. The fact that I was angry with what I could not alter, intensified the anger."

In spite of all these apparently unfavourable moods, the observer's performance on this occasion was approximately normal (cf. pp. 156–7).

(iv) S. never reported fluctuations of the intensity of sound while she was listening, and found little or no help in attempting to note the onset of sounds. From the first the factor of recognition was important, and in this respect she repeatedly referred to the help afforded by the outgoing of a sound (cf. p. 149).

(v) S., like B., was influenced by the length of the pauses, and the effect of earlier sounds. These factors produced an attitude of expectation in which the observer prepared to receive a sound of a certain intensity. In S., as in B., the length of the pause and the character of the just-presented sound exercised their chief importance in the later stages of the experiment.

(vi) S. settled down to a mechanical mode of response much more quickly than B. In the first month she reported: "I merely had to wait, and something inside decided the matter for me." And then a little later: "My responses were almost wholly automatic. Sometimes I wondered why and to what I was responding, and yet I was impelled to respond." Later still, the same experience was repeated with curious vividness: "It seemed to me that somebody else was doing the responding. I felt quite emotionless to this other person, whose hand I definitely visualised though I did not see my own hand, and whose antics in responding to the sound struck me, the detached onlooker, as supremely ridiculous and puerile....I was in no way called in as arbiter, and had no personal responsibility to the sound."

In S.'s case, in particular, the automatization of the response appeared to produce a lower threshold, and more accurate results. Of the seven occasions on which automatization of response was reported by S., her performance was four times distinctly above normal, and never below normal.

(vii) The tendency to rhythmic response was occasionally present, but was not at all prominent. "After a certain number of easily heard sounds," S. reported, "I noticed pulses of attention, and found it very difficult to refrain from responding, when the time came for the sound."

When we turn from the description of the determining factors of the reaction to their analysis, two interesting questions at once arise: (*a*) how do such factors come to be present at all in experiments of this type, and (*b*) in what way do they exercise their influence? These two questions may best be discussed together.

Every one of the factors just described is either itself of the nature of a habit, or else tends to give rise to a habit. But while some of the habits take their rise within the limits of the experiment itself, others are preformations carried over to this special set of conditions from other contexts. The second type is illustrated by the factors dominantly present in one observer, and absent, or almost absent, from the other. The vocalisation, for example, which seemed valuable to B., was never found in the case of S. B. is, in fact, so dominantly of a vocalising type that, whatever the task he may have to perform, there is a strong tendency for inner speech to occur and to exercise influence on the determination of the reaction. The same is true of the factor of muscular strain which was relatively prominent in the case of B. but relatively slight in the case of S.; while S.'s greater wealth of mood must undoubtedly be referred to intellectual and affective habits which go far beyond the limits of this experiment.

Two remarks may be made in regard to factors of the second class. Either they tend to be chiefly effective in the early stages of the acquisition of new modes of reaction and to disappear later; or else, if they persist, they lose their important determining influence on the reaction. The first is precisely what might be expected to happen, and is illustrated by the vocalisation and muscular strain factors in the case of B.; the second is particularly interesting, and finds illustration in certain of the attitudes in the case of S.

An attitude may be the direct outcome of some specific character of a presented situation. In this case it undoubtedly largely determines the nature of the response made to that situation. But it may equally be the outcome of the *general* nature of the situation, and then it appears that the attitude is a less efficient determinant. For example, an unmistakeable sound always tended to set up in B. an attitude of confidence which certainly made him far more ready to respond. But the succession of apparently unfavourable moods set up in S. when a series was presented containing but very few readily audible sounds had little or no effect on the nature of her reactions. The former was a mood induced by the specific character of a particular occurrence, the special loud sound in question being an outstanding feature of the external conditions of the situation to which reaction was demanded. The latter, on the other hand, was not an attitude induced in this manner. There was no particular prominent detail of the presented situation which could be clearly indicated as having given rise to the mood. What was present was not so much a feeling of satisfaction or dissatisfaction as regards any particular response, but rather a general feeling of ill-success, such as might equally well have accompanied any situation in which reactions of a hesitating and tentative character had occurred.

A further suggestion may be made. Probably all, and certainly most, conscious attitudes possess to a marked degree the capacity for transference[1]. That is, when a mood has been evoked by some specific occurrence or situation, *whatever* occurrence or situation follows more or less nearly often tends to be accompanied by a mood of precisely the same character. There is no doubt whatever that many of the moods reported both by S. and by B. in the course of these experiments were only to a slight extent attributable to the actual nature of the experiment during which they occurred. Supposing the latter had been of a very different type, provided it had been inadequate wholly to absorb attention, very much the same series of moods would in all probability have been experienced. It is tempting to suppose that when the moods which surround a series of reactions of a relatively mechanical nature are of a transferred character, they have little effect in determining the nature of the reactions. This is clearly a matter of some importance, seeing that there are no events and actions of daily life but have their halo of moods. Further investigations are called for to demonstrate precisely when, under normal conditions, the conscious attitudes are real determinants of action.

Meanwhile the importance which moods certainly possess under some circumstances makes it yet more clear that no matter how simple may be the form of response required from the human subject, the analysis of the character and efficiency of the response often demands reference to factors the history of which goes far beyond the limits of the definite conditions of the reaction in question.

When we turn to the habitual modes of reaction set up within the limits of this experiment, we find little that calls for remark. They were all based on a knowledge of the conditions of the experiments, and illustrate clearly the inferential character of much of our response to sensory stimulation. Factors of this type were found in both observers, they were brought into play somewhat less readily, but were definitely determining influences to the end.

Obviously the different determinants cannot all have operated in the

[1] Transference must be distinguished clearly from the persistence and spread spoken of on p. 152.

same manner. Vocalisation, for instance, was confirmatory; but motor experiences were anticipatory. Moods, being persistent elements, usually confirmed one response, and in so doing helped to direct another by anticipation. Both the 'transferred' mood, and the mood determined by the *general* character of the presented situation seemed to have anticipatory functions from the beginning of a group of tests. Rhythm was clearly anticipatory, while the occurrence of maximal points in the sound experience, and the length of intervals, played their parts chiefly by producing expectation.

The anticipatory character of motor sensations may probably be set down to the effect of a sound before it is heard[1]. In this respect it is interesting that much of the strain of which B., the vocalising subject, was conscious was localised by him in the throat. It is as if there may be an incipient vocalisation even before the sound is at all clearly experienced. In point of fact it appears highly probable that we have here come upon a type of determination by sensory experience which is very frequent in everyday life. Very many of the extremely swift reactions of daily life must surely be due to the fact that sense stimuli produce definite, but normally unnoticed, effects before they give rise to their full result. The anticipatory function of the mood however can hardly be explained in this manner.

What the whole of this study of the determining characters of the reaction most clearly shows, is that many factors other than those of sensory acuity and adaptation help to determine reaction to sounds of slight intensity. We are confident that the same result would have been brought out, had the investigation been directed upon any other sensory mode. It is a truth far too often forgotten, but abundantly illustrated by this attempt at a qualitative study of the factors which enter into listening, that no absolute measure of any mode of sensitivity is possible. The sense response is, even in the very simplest cases, carried out amid a mass of conditions not themselves wholly reducible to sense terms. Precisely what these conditions are depends on the specific character of the experiment under which the response is made. And not only is any measure that may be secured relative to the conditions of the experiment, but it is relative also to the whole attitude and preformed habits of the responding subject[2].

[1] Cf. pp. 143–5.

[2] The point is well brought out by S. W. Fernberger, "The Effect of the Attitude of the Subject upon the Measure of Sensitivity," *Amer. Journ. of Psychol.* xxv. 1914, 538–43. "One cannot measure sensitivity in absolute terms, one can only say that a given sensitivity has been found to exist *under certain given experimental conditions*....Inasmuch as

2. *Effects of Specific Changes in External Conditions.*

(i) *Light* versus *Darkness.*

We made some attempt to determine the external conditions for listening that are conducive to the best results, and definitely varied some of the factors that seemed of importance. The first question that arose was as to whether an observer's reactions may be affected favourably or unfavourably according as the tests are carried out in a dark or lighted room.

We found that both S. and B. preferred to listen in a light room, but that in the case of B. very bright illumination was not pleasant. S. preferred light from the first, but B. was at the outset convinced that darkness, which appears to favour concentration, was the better condition. However, after several trials, he reported: "There is no question about it: I prefer the light. In the light I feel greater ease and comfort, and the strain is less." S. was of the same opinion: "In the light there is much less strain. My attitude is passive. The ability to look round and notice things in the room prevents the feeling of strain, without hindering the response." When listening in darkness followed the series of coloured lights, B. was inclined to revert to his earlier decision: "My judgment is that after all darkness is the best of all conditions for listening. The thoughts and ideas that come have nothing much to do with your immediate surroundings, and consequently from the point of view of listening are less distracting. I suggest that either excitement or depression produced by sense experience of some other kind is unfavourable." A third observer independently reported: "With the light I have a different background of attention. All the room, and the things in the room are open to me. This distracts my attention, and I do not hear so well as in the darkness." But S. was of one view throughout: "I don't like the darkness, which distracts my attention."

When an observer was listening in the darkness, there was a greater tendency for his reports to be concerned with an analysis of the characters of the sound, and of the reaction[1]. This may perhaps suggest that during the early stages of learning to identify a sound, darkness is a

we are dealing with the total psycho-physical organism, and not merely with sense organs, it is after all not surprising that the attitude of the subject should have a profound effect upon this sensitivity."

[1] Cf. David A. Anderson, *Psychol. Monog.* XVI. 150–6, who also points out that to his subjects, while the quiet and freedom from distraction of a silent dark room were soothing, they also served to make the observer more critical.

favourable condition. But when once the sound has been learned, light should replace the darkness, since in the light most observers are probably more at their ease, and more completely comfortable.

(ii) *The Effects of Varied Colours.*

We varied the colour of the illumination in the observer's room, largely with a view to getting further information concerning the possible effects of mood or conscious attitude. By this time both observers had become thoroughly practised, and it has been mentioned already that one of the effects of growing familiarity with any situation demanding a certain type of reaction is that conscious attitudes tend to become much less marked. But with the variation of colours in the room, moods and attitudes were revived in all their vividness. Thus with red S. "feels indifferent"; was "conscious of being weighted down from without"; had "a feeling of complete hopelessness"; while B. "feels stimulated"; had "a mood of 'it doesn't matter much if I have made a mistake'"; possessed "a sense of freedom." With green B. "feels depressed," but S. had "a mood of repose and steadiness." Blue made B. "dissatisfied on account of the coldness of the light"; while S. experienced a "sort of excluding influence, unfavourable to the play of fancy"; and again, felt "restful, calm and soothed." Violet gave S. "a jaded impression, a stale feeling," and induced "an artificial feeling attitude." Later she reported of violet: "I hate this cold light. It gives me a feeling of confinement, and every now and then an asphyxiating feeling, and an attitude of general mental and physical stuffiness." To S. the yellow light was "stimulating," but it produced "nauseating suggestions." B. regarded the yellow as "detestable, bright but not stimulating, and I felt as though I wanted to get away from the yellow all the time."

On the whole there is considerable consistency of mood for the same person, for a given colour of light. Though the order in which the colours were used was varied, the attitudes induced remained very much the same. The suggestion is, then, that the moods in this case were actually induced by the colours of the illumination. Probably *any* outstanding change of external conditions would tend to revive the influence of the conscious attitude. Supposing this to be true, we have here illustrations of attitudes directly induced by a specific character of a presented situation. And if the suggestion made earlier is to be substantiated, we should expect to find evidence of definite influence of attitude upon response.

The experimental data are not unambiguous, and it cannot be expected that they should be, since moods are only one set of determining factors of the response. But on the whole they confirm the suggestions already made. For example, in green light, which he found "most depressing," B.'s results were uniformly bad; while with the same illumination, which she found "restful and steadying," S. did unusually well. Blue light, which displeased B., was associated with a very bad performance, and a large number of catch responses; while S., who considered the blue "most pleasing," obtained good results. On the other hand, B. found the violet light pleasant, and obtained results which were better than usual; but S. was greatly disturbed by this illumination, and her performance was below average. Finally S. did well in yellow light, which she found "stimulating"; while B., who found the illumination "detestable," did badly. On the whole, therefore, the suggestion made already tends to be borne out by a study of the results with vari-coloured illumination. When a mood or attitude accompanies the carrying out of a certain task, that mood or attitude being the direct outcome of some specific character of the situation which is present, the efficiency of the response tends to be definitely determined, among other things, by the character of the mood.

Another effect which may be attributed to the variable colours of the illumination may be noticed. Both observers reported the presence of imagery of various kinds throughout the whole course of the experiments. S., who usually felt a need of some slight distraction, seemed somewhat more prone to irrelevant imagery than B. Normally she listened with closed eyes, but in order that the colours should be allowed a chance of producing their greatest effect, she was told to keep her eyes open when the coloured illumination was used. The need for distraction and the necessity for keeping the eyes open, seem together to have produced a very great wealth of imagery, which now became predominantly visual. The most fantastic shapes of people, animals, and occasionally of plants, were seen upon the walls. To some extent, B. shared in this effect of the colours, and he also experienced far more than the normal amount of imagery. But so far as could be observed, the great wealth of irrelevant imagery produced absolutely no disturbing effect upon the reactions to sound. Its impotence may, in fact, be compared with that of the wealth of attitudes already described to have occurred on one occasion in the case of S. (see p. 154). Transferred moods and irrelevant imagery are both perfectly normal occurrences, and only in those instances in which the mood or the imagery is definitely attributed to the character of a present situation, that is to say, where the subject is entirely unconscious of their irrelevance, do they seriously disturb the normal reaction.

(iii) *The effects of external distracting sounds.*

Research on the effects of external distracting sounds was conducted chiefly with that modified form of Politzer's acoumeter test which we have called the boxed acoumeter[1].

The distracting stimuli generally used were: a whistle blown continuously, Stern's tone-variators adjusted for pitches ranging from 200–1000 v.d., an electric bell and a buzzer. One or more of these stimuli would be kept in continuous action for periods varying from 3 to 45 minutes, during which they formed a background of sound against which the acoumeter clicks were given.

All the subjects who took part in this test were well-practised and reliable.

The degree of distraction caused by such stimuli seemed to depend on (α) their intensity, (β) their pitch, quality and uniformity, (γ) their affective character. The first was by far the most important determining factor, any sound, if sufficiently loud, enormously raising the subjects' threshold for auditory acuity. Four different ways in which the loud sounds rendered the subjects insensitive and unresponsive to the acoumeter click were noted. These were: by masking or obliterating the sound, by distracting the subject's attention from the click, by rendering the click unfamiliar so that the subject no longer recognized it, and by causing a condition of irritability in the subject which unfitted him for concentrated attention. Sounds which normally occasioned no disturbance were found to produce a pronounced effect if their intensity was increased. Moreover, within fairly wide limits, a direct relation was found to hold between the intensity of the distracting sound in question and the threshold of the subject under examination.

Certain kinds of sound were found more distracting than others, irrespective of their intensity. For instance, in most cases the electric bell was found much more disturbing than the buzzer. The whistle also was observed generally to exercise a most disturbing effect; this was usually attributed to its 'piercing' character. The tone-variators produced remarkably little disturbing effect with the particular pitches used. In some cases even, their use appeared to have a slightly favourable influence upon auditory acuity. This may have been due in part to the greater stability of attitude induced. Thus, it appeared that the

[1] Part I of this paper, 103.

presence of continuous sounds of this character interested the subject, and so proved unfavourable both to reverie and to discursive thought[1]. Distractors of this class served to keep attention more or less fixed and thereby ensured a certain constancy of mood: at the same time they rapidly produced a high degree of habituation which prevented them from becoming all-absorbing[2]. A second and more direct effect was also noted, for it seemed as though this particular sound-background in some way served to throw the click into relief. The same result was often observed when clicks in the neighbourhood of the threshold occurred simultaneously with the striking of the hour by a town-clock. Facilitation in this case is the more remarkable in that it occurs when the distracting stimulus is unexpected and, in a sense, novel; under such conditions the distracting effect would be expected to be particularly marked.

Musical tones certainly proved to be less distracting than noises. But the beats of two simultaneously sounding tone-variators, set at 222 and 225 v.d. respectively, raised the subject's threshold in several cases. That the beats were the disturbing factors in this case was rendered clear by the fact that when the tones were coincident very little effect of distraction was observed. These slow heavy beats closely resembled the throb of an engine. In these and similar cases it is difficult to determine whether the distraction effect is due entirely to the intermittent character of the stimulus, or whether, as seems likely, qualitative differences enter in to some extent. Cassell and Dallenbach regard the temporal character of the distractor as being of fundamental importance, and consider qualitative differences to be practically negligible. Our experiments, while they confirm the importance of the continuous or intermittent character of the distracting stimulus, and in the latter case of its regularity or irregularity, also seemed to point most definitely to the considerable part played by the quality of the stimulus. Thus continuous stimuli were found to produce the most diverse effects which could only be satisfactorily explained as due to differences of quality. The effects produced by different kinds of distractors were found to be fairly constant in several cases over a relatively wide range of intensities.

[1] Cassell and Dallenbach note a similar effect. Dallenbach on one occasion reports: "I seemed unable to inhibit certain associative trains during the control series, whereas they were inhibited more or less involuntarily during the distraction series." "The effect of auditory distraction upon the sensory reaction," *Amer. Journ. of Psychol.* XXIX. 1918.

[2] In this respect the tone-variator background may be compared with the use of coloured light in series III (see pp. 160–2). Cf. also S.'s attitude in a lighted room (p. 159).

In our experience the most distracting sounds, as a rule, were those possessing human interest (as, for example, a footfall or the whirr of a taxi-cab) and unfamiliar noises or sounds, the source of which could not readily be assigned.

The well-marked disturbing effect produced by the sound of the wind is probably to be attributed mainly to its irregular character; in addition, it generally produced a pronounced degree of irritability with a consequently decreased power of concentration.

Adaptation was rapidly effective in the case of most continuous distracting sounds, provided that they were not too intense. Indeed, with the tube-and-screen acoumeter, the employment of a background of continuous buzzing noise was found to give more reliable results because it shut out irregular accidental noises which would have been far more disturbing. Irregularly interrupted sounds proved highly disturbing even when repeated so often that the subject was well accustomed to them. Of external sounds of weak intensity, much the most disturbing were those closely resembling the stimulus sound in pitch and quality. Their presence quickly occasioned loss of confidence in the subject, and his reactions soon became markedly erratic under these conditions. In the tube-and-screen test the twittering of one particular bird outside affected the subjects very considerably. Its note closely resembled the click of the acoumeter, and subjects repeatedly affirmed that it "put them out" and made them extremely uncertain. No other normal, accidental distracting sound proved so troublesome, or produced so diffident a mood in the listeners.

Lastly, the disturbing effect of external sounds was found to be determined in no small measure by the subject's personal relation to them. Apart from the universal effects of intensity, quality, duration, etc., just mentioned, the aggressiveness, the pleasantness, or the indifference of the distracting sound seemed not infrequently to depend on factors peculiar to the individual, and in particular to the subject's temperament, or to association arising from his past experience.

An attempt was also made to determine the subsequent effect of prolonged loud distracting noises on auditory acuity. This was found to be much less than was anticipated. But in the severest trial, where for twenty minutes the subject sat listening to a loud electric bell ringing close to one ear, while a whistle was blown near the other causing a deafening noise, a considerable degree of insensibility to the acoumeter click was shown in the tests immediately following the cessation of these noises. Nevertheless recovery rapidly took place, and within from five to ten minutes the acuity became approximately normal again. One effect of such continuous distracting noises was to make the subject more fatigued at the end of a sitting, even though his response had remained apparently unaffected by the background of sound. Such fatigue is probably due mainly to the increased excitement or increased strain demanded.

F. THE OBSERVER'S JUDGMENT AS TO THE EFFICIENCY OF HIS REACTIONS.

Both observers often attempted to estimate the value of their results. Comparing judgments with performances we get the following record:

B.'s Judgments.	B.'s Performances.
Bad.	Below normal.
Good.	Normal.
Bad.	Above Average.
Bad.	Good.
Normal.	Normal.
Good.	Normal.
Bad.	Bad.
Normal.	Normal.
Normal.	Normal.
Below normal.	Below normal.
Bad.	Below normal.
Above normal.	Below normal.
Bad.	Good.
Below normal.	Normal.
Below normal.	Normal.
Good.	Good.
Bad.	Bad.
Below normal.	Below normal.
Bad.	Bad.

S.'s Judgments.	S.'s Performances.
Good.	Above normal.
Below normal.	Normal.
Bad.	Above normal.
Good.	Good.
Below normal.	Normal.
Bad.	Below normal.
Bad.	Below normal.
Below normal.	Below normal.
Below normal.	Good.
Good.	Below normal.
Below normal.	Normal.
Normal.	Above normal.
Good.	Good.

The order of the tables corresponds to the order of judgments made by each observer. It will be seen that B. has 47 per cent. and S. 23 per cent. correct judgments. There is a slight suggestion that B. is improving with practice, but no such improvement can be detected in the case of S. Possibly a 'good' judgment is more likely to be correct than a 'bad' judgment; for whereas only 27 per cent. of the 'bad' judgments are correct, 42 per cent. of the 'good' judgments are confirmed by the corresponding performances. Moreover while two 'bad' judgments are correlated with good performances, and two more with results above normal, no single 'good' judgment is passed on a bad performance, and one only on a result below normal. The suggestion is that while an observer generally knows when he is doing well, his belief that he is doing badly may very likely be unfounded. This is interesting in view of the fact that the judgment as to good or bad performance is commonly based on the conscious attitude of the listener. Possibly the attitudes which are felt as positively favourable have a more definite effect on the results attained than those which appear as uncomfortable and disconcerting. It may be that a compensatory effect, in the form of the exertion of extra effort, occurs in the latter case, and is not allowed for by the listener.

G. Summary.

The present work has developed from an attempt to devise and apply a series of tests for the selection of candidates for the Anti-Submarine Service during the war. One set of these tests concerned auditory acuity, and the aim of the research herein described was (i) to devise apparatus for and methods of application of a reliable auditory acuity test, using a continuous source of sound, and (ii) to determine the optimal conditions for listening to sounds of weak intensity. The continuous sound was used in order to approximate as nearly as possible to the conditions of submarine listening.

Earlier observations regarding the unreliability of drop methods of producing sounds were confirmed, and in most of the experiments the sound was electrically conducted, its intensity being expressed in terms of the strength of current used. At first an ordinary buzzer, later an electrically maintained tuning-fork, was used as a source of sound. With the setting employed, all trace of a warning click in the telephone receivers was eliminated, and the methods developed show that results with a continuous sound may be as definite and reliable as those secured by the more commonly used single intermittent sound.

The chief results obtained show:

(*a*) that variations are liable to occur in the relative efficiency of the two ears, such variations as were observed developing gradually and extending over a long period;

(*b*) that under the conditions of this experiment binaural listening appears to be more efficient than uni-aural;

(*c*) that at the same time the relative acuity of either ear, or of both, appears to be affected by the order of testing, the tendency being for the first threshold taken on any given day to be the best;

(*d*) that for a practised listener the condition which most favours a low threshold and few false responses is the employment of a small range of finely graded stimuli restricted to the neighbourhood of the threshold, and that usually the best period of listening is the initial period;

(*e*) that the order of presentation of the sound—*e.g.* of regular descent, of regular ascent or of mixed intensities—affects different observers in a different manner, so that no one method can be said to be universally the best;

(*f*) that sounds of weak intensity may take as long as four seconds to produce their full effect;

(*g*) that the use of a warning signal in experiments on the auditory threshold is on the whole unnecessary, and is in some cases actually detrimental when a practised listener is being tested;

(*h*) that conditions of diffuse illumination tend to induce in the listener an attitude which is judged to be favourable, but that in the early stages of learning to recognize a sound, darkness provides the more favourable condition;

(*i*) that the responses to imagined sounds (1) occur in groups; (2) are most frequent during the initial period, before attunement, and at a later period when fatigue may be operative; (3) in the later periods of a spell of listening, as compared with the earlier, show a decrease in number together with an increase in the omission to react to presented sounds;

(*j*) that there is very little evidence of sensory fatigue, and that such improvement as may be effected appears to be due chiefly to growing familiarity with the sound presented, and with the general conditions of listening.

An attempt to make a qualitative analysis of the process of listening to sounds of weak intensity shows:

(i) that a series of presented sounds tends naturally to be classified on the basis of differences noted by the listener, and that such classification appears to be effected for the sake of that identification of sounds which plays an extremely important part in hearing;

(ii) that presented sounds are frequently, but not invariably, localised, being accorded position, and distance; and in some cases movement also may be attributed;

(iii) that sound stimuli may often be perceived when they cannot be heard as sounds;

(iv) that the cessation of the physical stimulus frequently gives rise to a positive experience of silence;

(v) that subjective sensations of extreme vividness often occur, which however may to some extent be discriminated from sounds having an objective basis;

(vi) that many other factors besides the purely auditory and sensory help to determine reaction to sounds of weak intensity;

(vii) that external distracting sounds are most disconcerting when they are irregular, or like the test sound, or of a familiar character;

(viii) that on the whole a subject's judgment concerning the efficiency of his reactions is likely to be accurate only when that judgment is a favourable one.

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PSYCHOLOGY AND EDUCATION[1].

BY T. P. NUNN.

THE meeting over which I have the honour to preside marks an occasion of great moment in the history of the British Psychological Society. The Society has recently decided, after due deliberation, to depart in two important respects from the policy which has hitherto guided it. On the one hand, its membership will no longer be confined to professional students and teachers of psychology, but is to be thrown open to all persons whose interest in the science is suitably attested. On the other hand, following the Spencerian law of progress, it proposes that a large part of its work shall henceforward be carried on by autonomous sections, each concerned with a special department of psychological inquiry. The first of these two departures is clearly justified by the immense increase in prestige that has accrued to our studies during the war. The psychologists have been mobilised side by side with their fellow men of science, and have shown that they, too, had invaluable service to give to the nation in its hour of need. We owe it mainly to these men—of whom we are proud to claim a large proportion, and among them by no means the less eminent names, as members of our small Society—that the thinking public has become deeply impressed by the great practical value, actual and potential, of exact psychological inquiries. Without doubt, then, the time has arrived when our Society may safely come out from its academic seclusion, and invite the adhesion of all who recognise that in the field of modern industry, as well as in the territories occupied by the ancient professions of medicine and teaching, there are problems, of great moment to human efficiency and happiness, that cannot be solved except with the aid of psychological knowledge and psychological methods. The second new departure—the institution of special sections within the Society—follows almost necessarily from the first. I need, perhaps, say no more about it than to remind you that the Spencerian formula, to which I have already referred, specifies progressive integration as well as progressive differentiation as the law of evolutionary advance. In other words, we must take steps to secure that the several Sections, in per-

[1] An Inaugural Address, here in abbreviated form, to the first Meeting of the Educational Section of the British Psychological Society, 11 April, 1919.

forming their distinct functions, always work as organs of a single Society informed by the true spirit of scientific psychology, which is, and for ever must be, one and indivisible.

It happens that ours, the Educational Section, is the first of these organs to be constituted, and to-day begins its formal existence. In what is largely an accident of fortune, we may yet modestly note a certain appropriateness. Not only is it true that the researches of some of the most valued members of the Society have been, particularly in recent years, important contributions at once to psychology and to education; it is also true that, of the wider fields of work which the new Sections are intended to cultivate, ours is, perhaps, the one least choked with the weeds of ignorance and prejudice, and has, in fact, already been ploughed for the sower. A few years ago, one of my colleagues advised a student not to use coloured chalks to illustrate his lessons in a secondary school, for fear the headmaster should suspect him of introducing psychology. The witticism would have little point to-day. Most teachers have travelled far beyond the point represented by the discovery which startled the honest Monsieur Jourdain. They not only recognise that, without knowing it, they were always psychologists; they are anxious that their psychology shall have the sanction of accredited authority. Indeed, it may be said that there is now, in some quarters of the educational world, an exaggerated estimate of the value of psychology: a disposition to suppose that laboratory experiments and the measurement of correlations can and should supply the whole basis of educational procedure.

To think thus is, in my judgment, to misconceive the true relation between psychology and education. To make progress, education must, no doubt, accept increasingly scientific regulation; but it can never be reduced, any more than life itself, to an applied science. Education is a biological function more ancient even than man, for it is found in a rudimentary form among the animals. Whatever we do or abstain from doing, the basal conditions of human life will ensure its permanence as a tradition of social activity, and will determine its general form. In relation to that great vital function, the psychologist must always be contented with the position of a critic, whose primary business is not to determine the aims of education, but to secure efficiency and economy in the means by which those aims are pursued. He is concerned with the aims of education only in a secondary way, in as far as his criticism of traditional procedure may lead to what is, in effect, a revaluation of accepted ideals.

This standpoint may be illustrated by a couple of simple instances. The art of writing is not an invention of the psychologist, and he is certainly not to be held responsible for the irrational spelling which the English-speaking peoples have inflicted on themselves. As things are, the psychologist must, like the schoolmaster, accept the binding of this ridiculous burden upon the young as one of the minor ends of education. His immediate task is, by the analysis of the writing-process, to make its acquirement as economical and effective as possible. Yet in performing this task, in criticizing from the psychological standpoint the accepted procedure, he paves the way to a reconsideration of issues much wider than the one originally presented to him. Thus not only do both advocates and opponents of spelling reform appeal to psychology—as, for example, Dr Henry Bradley, a moderate opponent of 'simplified spelling,' has done in his recently reprinted article on 'Spoken and Written English,'—but also many teachers have been led, by the present concentration of interest upon this question, to revise their estimate of the place of the art of writing in elementary education, and to value it as contributory to another art—in the aesthetic sense—namely, the sadly neglected art of clear and beautiful expression in speech.

In this instance, then, the direction of psychological analysis upon a problem of ways and means has, concurrently with other influences, led to a revaluation of the aim of the process. Similarly, the study of the technique of memorising, begun long ago by Ebbinghaus, has not only led to improvements in the practice of the schools, but also, through a fuller appreciation of the difference between 'mechanical association' and 'memory of meaning,' has contributed to an important change of view about the place and value of memorising in teaching and study.

The protracted controversy about 'formal training'—a question which was made a burning issue in this country by the brilliant criticism of Professor John Adams, and has been the subject of important experimental researches by two of our younger members, Mr W. H. Winch[1] and Dr W. G. Sleight[2]—illustrates my principle still more aptly. For the accumulating disbelief in the transfer of acquired ability from its original field to another has had a most pronounced effect upon educational thought. On the one hand, it has led, we may believe, to much sounder ideas about the nature of the 'mental training' produced by school studies; on the other hand, by freeing the minds of educationists

[1] Cf. This *Journal*, II. 1908, 284–293; III. 1910, 386–405.
[2] Cf. This *Journal*, IV. 1911, 386–457.

from bondage to an ancient fallacy, it has encouraged them to look for, and, I think, to find, a much better theory of the curriculum than was formerly current.

The psycho-physical mechanism of writing, the laws of memorising, the transfer of acquired ability are all topics whose educational relevance is immediate and obvious; their educational relevance is, in fact, the main source of the psychologist's interest in them. This cannot be said of certain other questions around which psychological debate has largely centred during recent years. For example, the theory of education cannot claim proprietary rights in the ideas of Dr Wm. McDougall and Mr A. F. Shand on the nature of instinct and the psychology of the emotions—ideas that were, I believe, first unfolded before this Society. Nevertheless their quickening influence, which has affected all the sciences that draw from psychology, has nowhere been felt more strongly than in education. The reason is manifest. McDougall's theory of instinct, supplemented by Shand's doctrine of sentiments, has thrown a flood of new light upon the genesis and laws of development of human behaviour; it is not surprising, therefore, that the writings of these psychologists, though they make only scanty references to educational problems, are among the main pillars of modern pedagogy.

It is my belief that the same thing will ere long be as evidently true of the ideas we owe to the profound insight and patient labours of Professor S. Freud and Dr Carl Jung. You may think that, in coupling the names of these two men together I show an imperfect appreciation of the divergence between their views. I am aware that the divergence is as wide as it well can be—being, in fact, the expression, in psychology, of the difference between the 'mechanistic' and the 'vitalist' conceptions of life. Experience teaches us, however, that differences even so profound as this, may yet be composed. We have a happy omen of future reconciliation in the attitude of physiologists like Dr J. S. Haldane, whose firm conviction of the teleological character of such a process as respiration has not prevented him from making additions of signal value to our knowledge of the mechanism involved in it.

This is not the place to develop the belief I have just acknowledged, nor to discuss the features of the Freudian doctrine which prejudice it in the eyes of many educationists. I may, however, be permitted to say that, from the standpoint of education, it was unfortunate—though no doubt inevitable—that the doctrine was developed, primarily, to explain pathological phenomena. To this circumstance we owe, for instance, the fact that the term 'complex' carries, by definition, a connotation of morbidity, although there is, from the strictly psychological point of view, no difference between the activity of morbid complexes and the structures that underlie most of our normal behaviour. It is in my view unfortunate that a term so aptly fitted to describe a general and fundamental feature of mental life should, by the accident of early usage, be restricted to morbid phenomena. It is due to the same cause that the concept of 'repression' occupies what I venture to think is a disproportionately large place in the new psychological scheme. Undoubtedly the facts of repression have immense importance for teachers as well as for psychiatrists. Yet to think of 'the Unconscious' as containing nothing but repressed material is, I believe, to miss much of the biological significance of the idea. In my view, the chief value of the concept is to remind us that conscious activities are only one—though the highest—of the means by which the psycho-physical organism conducts that intercourse with the environment by which and in which it lives. Consciousness marks the growing-point of our higher activities, the edge by which they 'cut into reality.' Behind this point, this edge, there is a vast impulsive organization of which a great part is, I believe, never represented directly in consciousness, while, of the residue, much that has once been conscious can never normally, and in its own character, reach the conscious level again. Nevertheless, the movements of consciousness, subserving the organism's perpetual self-assertion in the face of its environment, are never wholly explicable apart from this organization, whose history and constitution they express in an endless variety of subtle ways.

The central feature both of McDougall's theory of instinct and of Freud's doctrine of the unconscious is the insistence upon the importance of the affective elements in human life. Parallel with these researches, there has been another remarkable development of psychological inquiry in a field which is more obviously educational. I refer to the inquiries into the nature of ability, and the modes of testing it, which have been the subject of so much patient and acute investigation, and threaten to convert psychology, to the discomfort of many, into a branch of applied mathematics. In this connexion the name of Alfred Binet will always be honourably remembered, and his attempt to establish a 'metric scale' of intelligence opened a chapter of experimental pedagogy that is as yet by no means closed. Binet was concerned, as you well know, with a practical problem which forces itself upon the attention of educational administrators in every great city—namely, the problem of determining whether the backwardness that so often makes it impossible for a child to keep pace with his coevals is, in a given instance, due to mental defect or merely to unfavourable conditions, such as constant removal from school to school. I will say nothing about this question except to remark upon the admirable way in which Binet's original work has been continued by Mr Cyril Burt, whose recent survey of educational abilities in a London borough is a shining example of the value of the new statistical methods.

For most of us there is matter of greater interest in wider inquiries that have, in part or wholly, grown out of Binet's. As Professor Spearman has pointed out, Binet tacitly assumed a certain view as to the nature of the abilities manifested in the various types of intellectual performance—namely, that they are, in the main, only different expressions of a single factor whose value is, so to speak, a coefficient which varies from person to person but is constant for a given individual. To the theoretical question here raised, the question whether abilities are 'non-focal,' 'multi-focal' or 'uni-focal,' Professor Spearman's own contributions are of unique and outstanding importance. He has defended his acceptance of the 'uni-focal' view in a series of memoirs that have attracted widespread attention, and have given rise to a vast amount of most useful discussion. It is true that the validity of the argument upon which he chiefly relies—an argument which is itself of the highest novelty and interest—has recently been called acutely in question by Dr G. H. Thomson[1]; but my faith is that Professor Spearman has a satisfactory reply even to so ingenious and penetrating a critic. At the same time, I think it possible that—as has so often happened in parallel instances in the history of science—his simple conception of the 'central intellective factor' may have to be qualified or elaborated.

Meanwhile Spearman's characteristic method has been applied, with results which are, to say the least, both interesting and promising, to the further exploration of the fundamental factors in mental activity. First we had Dr E. Webb's important and excellent research[2] into the psychological elements that determine the judgments men pass upon the character of their fellows, and his discovery that these judgments imply the presence of a second 'central' factor which he has called, provisionally, the 'persistence of motives.' And this year we have had from Mr J. C. Maxwell Garnett[3] the announcement of a third elementary factor, 'cleverness,' together with the fascinating suggestion that the three factors signalised by Spearman, Webb and himself may be regarded

[1] This *Journal*, IX. 1919, 321–336.

[2] This *Journal*, *Monogr. Suppl.* 1915, I. No. 3.

[3] This *Journal*, IX. 1919, 345–366.

as co-ordinates fixing the general character of an individual's endowment. While the field opened up by these later developments from Spearman's researches is, admittedly, one that demands further exploration, there can be no question of its great interest both for general psychological theory and for education.

My faith in the essential soundness of Spearman's doctrine of the central intellective factor is confirmed by the growing evidence of the practical efficiency of 'intelligence tests'; for it seems difficult to explain this efficiency on any other basis. By far the most impressive evidence of this kind is that afforded by the grandiose experiment in the American Army, in which about a million and a half recruits were submitted to tests designed to pick out the men of superior ability, to eliminate those whose inferiority would make them a source of hindrance or danger to their fellows, and to enable the authorities to equalise the intellectual strength of military units which were intended to work together. The comparison, in some thousands of instances, of the results of a fifty minutes' test with the carefully considered judgments of officers about the men's efficiency in the vastly different circumstances of the camp and the training school, is extraordinarily favourable to the value of the tests. It is not surprising that at least one American University is said to be adopting intelligence tests as a supplementary means of discriminating between the claims of its would-be alumni, and that in this slower-moving country the use of such tests as a means of grading County Council scholars and other entrants to secondary schools has received a very great impetus. Here is indicated a field in which our Section may find extremely useful work to do; for if this outburst of faith in psychology is not cautiously and capably guided, it may end in disillusion, and in bringing the use of intelligence tests into contempt.

The same thing is true of the growing belief in the value of 'vocational tests,' which may be regarded, speaking broadly, as related to Spearman's 'specific factors' in the same way as intelligence tests are related to his central factor. At least one local educational authority is reported to be willing to pay for expert psychological opinion about the capabilities of the young people in its schools. The movement thus initiated offers scope for much mischief as well as for good work. Teacher-members of the Section who have had some psychological training could hardly do more useful service than by taking in hand the cautious and systematic study of individual pupils from the standpoint suggested by the concept of vocational tests.

In an hour's space it has been possible to glance at a few only of the more important departments of educational psychology, and that only in the most cursory way. It is evident that in these, as in many other directions, a great deal remains to be done in order to perfect the control of our professional activities by scientific regulation. It is gratifying to our pride to observe how much of what has been done is to be credited to members of the British Psychological Society. The Educational Section has been constituted to widen and deepen this good and necessary work. May success attend its labours!

PSYCHOLOGY AND INDUSTRY[1].

By CHARLES S. MYERS.

Psychology must be ranked to-day among the Natural Sciences, standing in the same relation to the living Mind as Biology stands to living Matter. Biology, as is well known, has been served by two different methods of approach. There is the older 'observational' method, which has been employed in such general problems as natural selection, variation, adaptation, etc., and in compiling the Natural History of living organisms—their development, their generic, specific, individual and sexual differences, etc. And there is the 'experimental' method, which, in conjunction with the former method, has given rise to physiology, pathology, genetics, etc.; these have rapidly advanced to the position of Applied Sciences in relation to medicine, human and veterinary, to eugenics, animal and vegetable, etc.

In its earliest days Biology was a field of study for philosophers, who employed both the observational and the experimental methods of approach. Psychology, on the other hand, is only just emancipating itself from the tutelage of Philosophy. In Psychology we can likewise distinguish the observational method, which has helped to reveal to us the Natural History of mind, from the later experimental methods which have been characterized by a more thorough knowledge and a more perfect control of attendant conditions, and by a more complete freedom from metaphysical preconceptions and aims. The effects produced by varying conditions upon mental experience (or introspection) and upon bodily expression (or behaviour) have come to be studied in the laboratory, where those conditions can be simplified or complicated at will. Advantage has also been taken of Nature's own variation of such conditions, as in the clinical and laboratory study of *individual* mental differences, normal and abnormal, of excess or defect, including those produced by disorder, disease or injury, *racial* mental differences, *e.g.* the mental differences between the higher and lower races, and *generic* mental differences, *e.g.* the mental differences between the sensations,

[1] The substance of an Inaugural Address to the first Meeting of the Industrial Section of the British Psychological Society, 25 April, 1919.

intelligence, instincts, etc. of man and animals, of vertebrates and insects, and so on.

Still more recently another stage in the evolution of Psychology has been reached by the systematic study of unconscious processes and of their relations to consciousness. Whereas the earlier philosophical psychology, and the experimental school which arose from it, had been mainly intellectualistic, giving undue prominence to the play of reason, this later stage has been characterized by the emphasis it lays on the importance of instinct and the emotions, and by its devotion to the study of unconscious processes.

As in the case of Biology, the results obtained from experimental psychological methods, and indeed those methods themselves, have begun to be applied to practical purposes—first to Education, next to Medicine, and most recently to Industry, thus creating three Applied Sciences, those of Educational, Medical and Industrial Psychology; and the British Psychological Society is now instituting three Special Sections of the Society which are to be respectively devoted thereto.

In Industry (including Commerce) there are four main themes to which Psychology can be profitably applied, namely to fatigue, movement study, vocational guidance and management.

Fatigue has long been a subject of research both by physiologists and by psychologists. The physiologist has generally investigated it under the simplest experimental conditions. For example, he has isolated a single muscle of a frog from the body, and has studied the phenomena of fatigue produced in it by electrical stimulation of the muscle or of its nerve, and the effects of varying the strength and frequency of stimulation, the surrounding temperature, the weight lifted by the muscle, etc. He has also investigated the effects of muscular exercise on the general metabolism (the respiratory quotient, the excreta, etc.) of the organism. The psychologist, on the other hand, has conducted exclusively 'human' experiments, treating the organism as a whole in place of using 'muscle nerve' preparations. He has approached the problem from the standpoint of mental as well as muscular fatigue. He has devised tests of mental fatigue, constructing work curves of mental output, and analysing the psychological factors involved therein, such as spurt and practice, in turn analysing the latter into its psychological components. He has studied the effects of drugs, *e.g.* of tea, coffee, strychnine, and alcohol, on mental and muscular fatigue. He has examined the effects of rest pauses of different length, introduced after varying periods of work, on mental efficiency. He has shown the unreliability of certain interpolated tests as evidence of muscular or mental fatigue; he has shown the importance of a rigorous, precise training in the methods of experimental psychology, in order to avoid the pitfalls incidental to human experiment; and he has so prepared the way for a systematic investigation of the more important problems of industrial fatigue that future success must depend on intimate psychological and physiological coöperation.

Movement study has as yet been scarcely touched by the psychologist. It has hitherto been mainly the purview of the industrial 'efficiency expert.' But there is obviously a vast field of promising psychological research here. The present methods are largely empirical and guess-work. The expert pays a visit to a factory where he sees a worker making a series of seemingly needless movements. He believes that time would be saved by training the worker to another, an apparently 'shorthand,' method. He tries it and, we will suppose, he finds that time is saved by its adoption. He assumes that, because a speedier method has been devised, there is no increase, or there is a decrease, of fatigue. He assumes that because this method is found to suit one worker, it is therefore the one and best method, always to be adopted by all workers. Clearly there are numerous problems for psychological experiment here, by which the applied science of Industrial Psychology will be advanced to surer ground. Similar scientific work is needed to yield reliable information in regard to other matters which are intimately connected with movement study, *e.g.* the optimal load and posture, the optimal rate and frequency of lift, etc., in persons of different muscular power, age and sex.

The study of vocational guidance is founded on that of individual differences, for the basis of which we are indebted to pure experimental psychology. Some of the earliest psychological experiments on reaction time were devoted to a study of the individual differences observed. The advantages of selecting employees in the case of certain occupations according to their speed of reaction have been shown in a certain bicycle-ball factory, where, after the selection of the best workers as indicated by reaction tests, it was found that 35 individuals could now do the work of the previous 120, and that the accuracy of the work was increased by two-thirds.

Similar results have followed from the application of psychological tests to the selection of applicants for telephone-exchange work in America. It is obvious that their hearing, vision, speech, memory for figures, memory for the order of instructions received, and their speed and accuracy of reaction to signals are readily capable of experimental estimation.

It is easy to devise tests of manual dexterity and these are already being employed in America in the selection of employees for work in which such dexterity is important.

Psychological tests of foresight are also capable of construction, and have actually been applied with success in investigations on motor tram-drivers. A very close inverse relation was found to obtain between the degree of a driver's success in the laboratory test and the number of accidents recorded against him during his every-day work.

Tests of sensory acuity and discrimination, tests of artistic endowment, tests which measure fatigability, accuracy, neatness, distractibility, improvability, memory for names and faces, powers of observation, etc. are also available. Their application to those who offer themselves for designing work, clerical work, salesmanship, etc., is obvious.

Tests of the accuracy and speed of reasoning have also been devised. Tests of general information have been frequently employed. These and other tests are about to be used at Columbia University, New York, in place of the Matriculation Examinations, so as to select those who can best profit by a University career.

During the last year of the war, I happened to be concerned in the selection, at the Crystal Palace, of candidates for training in hydrophone-listening for hostile submarines. Tests were devised for keenness of hearing, accuracy of sound discrimination, memory for pitch, rhythm and quality of sound, power to discriminate between different pitches, rhythms and qualities, general accuracy, general information, ability to grasp complicated instructions, etc. The result of the application of such tests was that the training authorities at Portland reported that the first batch of lads sent them from the Crystal Palace was far away the best they had ever received, and that the next batch was even better still!

It is perfectly clear that by the aid of properly devised tests applied by properly trained persons those leaving school could be materially helped and usefully advised in their choice of a suitable vocation, and that by their application to candidates for any industrial or commercial post the fittest could be speedily selected.

An objection may be raised that these tests throw no light on the higher moral qualities of the candidate, such as perseverance, resourcefulness, good temper, loyalty, honesty, courage, self-control and 'presence.' But in point of fact, the presence of the first two of these qualities are revealed in many existing tests or in others that can be specially devised for the purpose, whilst much light is (or can readily be) thrown on the rest in the course of individual examination and cross-questioning.

None but those who have had experience in psychological tests can realise what a wealth of information in regard to the 'character' of a subject is incidentally gained from tests systematically and *individually* applied: although the 'lower' mental characters can be easily tested in groups of fifty or a hundred or more persons simultaneously.

Under the application of Psychology to management, I include the consideration of the psychological causes of industrial discontent and restricted output, the psychological advantages of different methods of payment and supervision, and other conditions which affect the efficiency and the happiness of the workers. During the last few years a flood of light has been thrown on the importance of the emotions and on the changes which they effect and to which they are subject. We now recognise how prone we are to rationalise, *i.e.* to give an intellectual reason for actions which are really prompted by emotional states, or by subtler influences which are unknown to us or which for good reasons dare not be faced. We now recognise that in order to avoid causing excessive self-depreciation an emotion may undergo a process of 'projection.' Thus instead of reproaching ourselves we may attribute the reproach to others; hence arise delusions of suspicion and even persecution. Or, for the same purpose, an emotion may be 'inverted,' *e.g.* shyness becoming concealed by an affected boisterousness, the desire for a person of the opposite sex by aversion, submissiveness by defiance. We understand now more fully the psychological basis of worry and anxiety, the importance of their early treatment, and the psychotherapy of the functional nervous disorders to which, if unresolved, they may give rise. The application of such new advances to the problems of industrial unrest is sufficiently obvious.

It may be asked whether a Special Section of the British Psychological Society is the most appropriate body for the promotion and communication of researches bearing on Industrial Psychology, or whether a separate society should not have been constituted in its stead for these objects. To this question, I think, there is a very clear answer. The British Psychological Society is henceforth to consist of three Special Sections devoted to Education, Industry and Medicine, and of a General Section devoted to the other aspects of Psychology, from which it is hoped in the near future to form further Sections for Social Psychology, the Psychology of Æsthetics, Animal Psychology, etc. The Society consists, therefore, of members interested in every branch of knowledge to which Psychology is applicable. It will afford opportunity for the holding of joint meetings between Sections, at which subjects of common interest can be profitably discussed. Thus, the Industrial and Medical Sections could advantageously combine in considering the psychology and psychotherapy of industrial neurasthenia: the Industrial and Educational Sections could profitably hold a joint meeting to discuss from the psychological standpoint the best arrangements for industrial education. Each Section of Applied Psychology must thus reap advantages by intercourse with the other Sections, while the presence and coöperation of those engaged in pure psychological research cannot fail to lift Applied Science to a higher plane.

PSYCHOLOGY AND MEDICINE[1].

BY W. H. R. RIVERS.

WE are met this evening to inaugurate the foundation of a Special Section of a Society which has hitherto attempted to cover unaided the whole field of psychological inquiry. The great increase of interest in, and knowledge of, the mental aspect of disease which has been one result of the abnormal strains to which modern warfare has exposed the soldier made it certain that something would be done to foster this interest and increase this knowledge. On the more practical side, and in its relation to medicine in general, the medical profession is already provided with instruments for this purpose in the Psychiatrical Section of the Royal Society of Medicine and the Medico-Psychological Association; but the work of these bodies is chiefly connected with the practical aspect of medicine. It is not their business to attend in any special measure to the theoretical aspect of psychology. Still less is it their function to deal with the relations of their work to other branches of psychology.

As soon as the declaration of the armistice became effective, there arose a widespread opinion that some organization was necessary to encourage the more theoretical side of psychological medicine. It became a question whether this purpose would be fulfilled more effectively by a society wholly devoted to this purpose, or whether the new organization should become part of the society which has now for many years been the meeting place of the psychologists of this country. This question has been decided in the second sense, and on this occasion, when the Medical Section of our Society meets for the first time, I cannot better employ the privilege you have given me of opening its scientific work than by pointing out some reasons which justify this decision.

I will begin with a general problem. One of the most vexed questions of to-day is concerned with the good and the evil of specialism. With the great advance in knowledge of which we are now enjoying the fruit, specialism is necessary. Owing to the vast extent of the field it is essential that workers in medical as in other branches of science shall not be con-

[1] The Inaugural Address to the first Meeting of the Medical Section of the British Psychological Society, 15 May, 1919.

tent with a general knowledge of the subject to which they devote their labours, but shall attend specially to some one of the many aspects which every branch of knowledge now presents. This specialisation has, however, in recent years reached such a pitch that it has become a serious evil. There is even a tendency to regard with suspicion one who betrays the possession of knowledge or attainments outside a narrow circle of interests. Scientific workers often deliberately confine their research to some narrow channel. They fail to see the bearings of work (including their own) which would be obvious if they lifted their heads and surveyed even cursorily the broad field of knowledge of which their own specialty forms one of the fertilising streams. The linking of workers in psychological medicine with other students as members of a society which covers the whole field of psychology should go far to prevent the evil of undue narrowness of outlook and limitation of interest.

It is proposed that the Society to which we now belong shall have several Sections, but owing to the fewness of their adherents students of certain subjects well adapted for Special Sections will for the present have to be content with the opportunities offered by general membership. I propose now to survey briefly some of these branches of psychology and to inquire how we may expect to benefit from association with those who study them.

I will begin with the parent, an aged parent, whose services in the past we are perhaps inclined to depreciate unduly. The older Introspective Psychology which came into existence by a long-drawn-out process of fission from philosophy, with which it still struggles to maintain its connexion, did great service in defining the problems of psychology and analysing into its elements the vast complexity of mental process. Into this process of analysis it brought from its earlier associations a clearness and exactness of thought which may well be emulated by the adherents of the later developments of psychology. The great fault of the older psychologists was that their liking for clearness of reasoning and exactness of definition led them to pay a far too exclusive attention to the intellectual aspect of mind, where exactness is possible. It led them to ignore, or to pay too little attention to, the subjects of instinct, feeling, and emotion, which are less susceptible of exact treatment. They also tended to neglect unduly mental process lying on the confines, or altogether without the confines, of the conscious. The adherents of the older academic psychology are now aware of the imperfections of their earlier position. But they have not lost the logical minds which they acquired from their special training, and there is the widest scope for mutual advantage if they will take part in our discussions and if we attempt to understand the point of view from which they regard our special problems.

Next in order, I may take the Experimental Movement in psychology. There is now a widespread, and in my belief a well-founded, opinion that this movement has failed to come up to the expectations of its founders and has proved unfruitful as a direct means of increasing our knowledge, at any rate in so far as it confines its attention to the experimental investigation of the normal human adult. It cannot be said to have done much more than provide suggestions and clues for investigation on other lines. It is not difficult to see the reason for this. In the case of the normal human adult, there is too little scope for the variation of conditions which is the essence of experiment. It is only such subjects as are open to the definite variation of conditions that have provided material for any great advance in knowledge.

While the experimental method as applied to the normal adult has borne little fruit, it would be difficult to rate too highly the importance of experiment in discovering and testing methods to be used in other lines of psychological inquiry where a wider variation of conditions is possible. The value of these methods is now generally recognised in the investigation of the developing mind of the child and of the mental processes of animals. These methods are also essential to the new department of industrial psychology, but there is no branch in which they are more needed than in psychological medicine. Thus, the clinical work of Head and his colleagues, which must take a foremost place in the history of psychology, has as one of its main features the utilisation of the methods of experimental psychology modified to meet the special needs presented by clinical material. Experimental psychology takes a place, in relation to the observation of data derived from behaviour, as important as that which is taken by the older introspective psychology, in relation to the logical processes by which these data are utilised.

It is not at present proposed that the two branches of psychology which I have just considered shall have Special Sections devoted to them. Members of this Section should be able to gain such help as they require by the attendance of general and experimental psychologists at the meetings of this Section, while they may also learn much from the papers and demonstrations given at general meetings of the Society.

The subject of Educational Psychology, for the study of which a Section has already been formed, is one from which members of this Section should have much to learn. It is now widely agreed that much of the general psychological state which predisposes a person to the occurrence of neurosis, and to a less extent of psychosis, comes from faults of education, using this term in a wide sense to include the whole life-history of the child from its birth. Opinions differ concerning the relative weight of these factors as compared with heredity in the production of the neurotic and psychotic constitution, but no one can use modern methods in the clinical investigation of psycho-neurosis without having forced upon him the vast importance of the mental traumata and faulty trends of thought and conduct which occur or come into existence in childhood, often in its very early years. Nothing is more needed to help advance in our special field of knowledge than close collaboration between those who deal with sufferers from the psycho-neuroses and those who have the care of children during the years when the seeds of these morbid states are sown. May we hope that joint gatherings, mutual attendance at the meetings of both Sections, and the general opportunities for interchange of views which this society should provide, may furnish material for rapid progress on lines which will bring great mutual benefit to both physician and teacher.

The other Section, which has been already inaugurated, will deal with Industrial Psychology. Within its scope will come the investigation of fatigue, interest, practice, and other mental factors which affect the efficiency of labour, both manual and mental. It will also deal with the mental qualities which fit or unfit a person for an occupation, and thus help in the highly important task of putting "round pegs in round holes and square in square." Investigations of this kind will not be limited merely to the needs of government departments or to the demands of industry, but will, doubtless, be extended to such subjects as the choice of a profession, and will thus closely approach the work of the Educational Section. Here, perhaps, the psychologist of industry will have to learn from the physician, and members of this Section will, perhaps, visit the Section of Industrial Psychology to teach rather than to learn, but it is very unlikely that the advantage will be altogether one-sided. Nothing but good can come from common membership of this society with its opportunities of collaboration.

The two Sections to which I have just referred will resemble our own in that they will be devoted to applied psychology. It is not at present proposed to found a Section for the branch of psychology which I have now to consider, though this cannot be long delayed. The science which deals with the behaviour of human beings when acting as members of a group, and not merely as individuals, is usually known as Social

Psychology, though it might also be called Collective Psychology. It attempts to explain the actions of men as members of a social group, whether nation, tribe, clan, church, profession, club, or any other kind of combination in which men unite for social purposes. Psychology is here removed from the possibility of utilising experiment as a means of eliciting truth and has therefore had recourse to the comparative method. Social psychology is largely devoted to the behaviour of human society in its simpler and cruder forms, whether of the past or of the savage and barbarous peoples who still occupy the less comfortable regions of the earth. Through the study of the culture of these peoples it is becoming possible to form some idea of the mental factors which have determined the course of human evolution and the outcome of the interactions, peaceful or otherwise, which come into being when peoples are brought into contact with one another.

It might seem at first sight that we have here a branch of psychology which can have little in common with the work of this Section. The knowledge gained in this new study, however, has already brought out some striking parallels between the behaviour of the ruder forms of human society and the thoughts and actions of civilised man under such abnormal conditions as come within the purview of this Section.

The outstanding fact which is daily becoming clearer to those who are studying the various forms of mental disorder is that these depend upon the re-entrance into activity of instinctive tendencies which have been brought under control in the normal healthy person, and have become subject to standards of thought and conduct in harmony with the needs of social life. In man many instinctive tendencies subserving the welfare and immediate happiness of the individual have been suppressed because they are incompatible with the needs of society, and with the ultimate happiness of the individual as a member of society. Neurosis and psychosis are essentially states in which the controlling and suppressing forces have been so weakened that conflicts are aroused in which the suppressed tendencies strive with more or less success to regain the predominance they once held, both in the history of the race and in the infancy of the individual.

The frequency of the psycho-neuroses in the great communities of the modern world is the direct consequence of the fluid and unorganized character of their civilisation. In savage communities where long ages of freedom from external influence have allowed culture to become organized and stable, the psycho-neuroses are absent or hardly to be detected, although the cruder forms of mental disorder, such as imbecility and idiocy, are not uncommon. In such communities there has come about a stable adjustment between instinctive tendencies and social ideals, leaving no room for the conflict which forms the essence of the psycho-neuroses. Similarly, I believe that a comparative study of the frequency and severity of neuroses among the great civilisations of the modern world, will show that this frequency and severity are definitely correlated with the fluidity or instability of the culture and with the extent to which national ideals call for repression of instinctive tendencies. The perfect social organization is one in which instinctive tendencies, out of harmony with social ideals, have so come under control that they no longer form the grounds of conflict or give occasion for it only in the presence of exceptional stress and strain.

Another interest of social psychology to the student of mental medicine, is that when men act collectively they tend to be moved predominantly by motives belonging to the instinctive sphere. Not only does each individual of the mass act more instinctively as a member of a group than as an individual, but when a number of men act together, the rational motives arising out of education, and other forms of social process, tend to cancel one other, leaving in power the instinctive tendencies which are common to all and of the same nature in all. It is this potency of instinct which is common to the behaviour of men in mass and the individual in disease which gives a common interest to the physician and the social psychologist. It furnishes a reason in itself sufficient to justify the inclusion of both in one organization for the advancement of knowledge.

Of even more interest to the physician should be the study of mental process in rude and backward forms of human society. Much as we may disagree in detail, there is general agreement that in neurosis and psychosis there is in action a process of regression to primitive and infantile states. Anything which helps the physician to a knowledge of the primitive and infantile in man should therefore come within his circle of interests. In so far as the thought and behaviour of savage man are primitive, they furnish material which helps us to understand and to deal with the regressive states exhibited by sufferers from disorder of mental functions.

From the point of view of the ethnologist the problem is far from simple. The customs and institutions of existing examples of savage man show such a bewildering mixture of development and degeneration that it is difficult to disentangle the primitive from the late. If it stood alone ethnology would be greatly embarrassed by the complexity of its problems, and it is driven to seek help in the study of other aspects of human behaviour. If regression is a constant feature of morbid mental states, and of such a normal process as the dream, it should furnish much material to help the seeker after the primitive in human conduct. In a recent publication[1] I have tried to show how the psychology of the dream may help us to determine the nature of the primitive, and quite as much is to be learnt from the psychology of neurosis, which, as we are coming to see, has so much in common with the dream. Medicine standing alone and ethnology standing alone may find themselves helpless before problems the solution of which will come from the union of the two lines of inquiry, at first sight so widely different from one another. It is difficult at present to say exactly how each can help the other, but the general trend of our knowledge justifies the brightest hopes if students of each will work together in such a spirit of coöperation as it is the object of this Society to foster.

Another branch of psychology is one which might at first sight seem even more remote from the interests of the physician than that I have just considered. The study of the mental processes of animals is one in which this country has had, and still has, great names, but the number of its votaries is at present so small that there does not seem to be any immediate prospect of the foundation of a Special Section devoted to this study. Work on this subject will for the present form part of the business of the general Society, but however it enter into the scheme of our activities, Animal Psychology should have a profound interest for the physician, and for reasons similar to those which justify his interest in the ruder forms of human culture. If instinct have the importance which we are, I think, all willing to recognise, animal psychology, from which issues our chief knowledge of instinct, cannot be neglected by this Section. At the present time there is much confusion and uncertainty in the use of instinct as a psychological concept. In clearing up this confusion we need the coöperation of those whose studies lead them to an interest in the primitive, whether it be exhibited by the animal, the child, the savage, or the subject of regression. We do not know from which of these will come the clue leading us to the truth, but my impression is that morbid psychology has the brightest prospects, and that this subject is destined to illuminate many of the dark regions in our knowledge of mental development. Here, again, the opportunities of collaboration offered by this Society should furnish an ample reason for our union with other branches of psychology.

[1] W. H. R. Rivers, *Dreams and Primitive Culture*, Manchester, 1918.

Thus far I have been considering the work of this Section in its relation to the general aims of the British Psychological Society and to the work of its other Sections. It is now fitting that I should say something about the special tasks which lie before our own Section. The most obvious task is to understand the functional nervous and mental disorders, and to make them instruments by which we may be helped to understand the normal working of the mind. Most of us are engaged, or have been engaged during the war, in the practical task of treating sufferers from disorders which, notwithstanding the apparently physical character of many of their manifestations, are now generally recognised as being wholly or in the main of mental origin. It is our special task as members of this Section to study these disorders in order that we may carry out that process of bringing them into relation with other fields of knowledge which we call 'explanation.'

Many of us have been brought into contact with the psycho-neuroses only through the accidents of war, and a word or two may therefore be said about the special interest of war neurosis from the psychological point of view. In my opinion, the keynote of the war neuroses is their simplicity. They are primarily due to the reawakening of suppressed instinctive tendencies which, in most members of our civilisation, are normally allowed to lie dormant because the mechanisms by which they are controlled and suppressed are subjected to no strain great or continuous enough to interfere with their efficiency. Moreover, the instinct which is mainly affected, the instinct of self-preservation, is one of great simplicity, while the social and intellectual elements which form factors in the process of control are also of a relatively simple kind. Through this simplicity the psycho-neuroses of war are well adapted to illustrate the essential characters, not only of the pathological states, but also of the normal balance between controlled and controlling forces which underlies the harmony of the healthy mental life.

Now that the war is over it becomes our task to utilise the knowledge we have gained from the study of a relatively simple and transparent form of disorder. We have to discover with equal clearness the nature of the psycho-neuroses which in times of peace take so large a place in the life and work of a physician. Just as the war neuroses are essentially the result of conflict between certain instinctive tendencies and the traditional sentiments and ideals of society concerning these tendencies, so can we be confident that a similar conflict will be found to form the essential factor in the psycho-neuroses of civil life. It will be our task to discover the nature of the instinctive tendencies and of the social ideals which form the opposing forces in this intestine warfare within the individual life. You all know how the most prominent school of students of the psycho-neuroses believe that the instinctive tendencies which stand on one side of the battleground belong exclusively to the instinct of sex. However repellent this may be to the traditions of the medical profession, we must be prepared to face this problem honestly and without prejudice. In turning from the practice of war to that of peace we must expect to find a great increase in the part taken by the sexual instinct, for the simple and obvious reason that the conditions of our civilisation make this instinct the special object of its repressions and taboos. We have found reason to believe that sex plays but a little part in the causation of war-neuroses, but it does not follow that this will hold good of the neuroses of civil life. On the other hand, we must be careful to hold the balance and not allow ourselves to give to sexual tendencies a prominence greater than they deserve. The sexual instinct is far from standing alone as the subject of the repressions and taboos of social tradition. It should be our working hypothesis that any instinct which needs repression in the interests of society may furnish that occasion for conflict which forms the essence of neurosis.

The psycho-neuroses form so absorbing a topic, and provide so vast a field for observation and speculation, that we may easily overlook another line of inquiry which should be of equal interest to our Section. There is an especial danger that we may overlook this second line of interest in that most of us have been engaged during the war exclusively in the study of those disorders of nervous and mental activity which we label 'functional.' Many perhaps may be hardly aware of the advance in psychological knowledge which has come from observations on the organic lesions furnished by the accidents of war.

The destruction of parts of the nervous system by injury or disease takes in the psycho-physiology of man the place which in the case of the lower animals belongs to experiment. We have to look to the investigation of organic lesions for light upon all those higher functions of the nervous system which give to man his distinctive place in the universe. Clinical investigation forms the chief instrument, sometimes the only instrument, which we can use in the investigation of speech or other of those higher reactions which are associated with mental activity.

The clinical investigation of the nervous system has in recent years thrown a flood of light upon all the sensori-motor processes which form the basis of mind. Pre-eminent in this respect have been the researches of Head. Working steadily through the nervous system from periphery to centre, he has now reached the cortex cerebri. In his recent paper on "Sensation and the Cerebral Cortex[1]" he has given us a clear and consistent account of the mechanisms by which the cortex exerts that selective and discriminative activity which we call 'attention.' It is only through such work that psychology can expect to gain any profit from the study of the anatomy and chemistry of the brain, whether normal or pathological. The neuron and the arrangements of neurons can have none but a purely speculative interest for the psychologist until the analysis of mental function has carried our knowledge far beyond the limits of the territory it has now conquered. It is only when such analysis as that carried out by Head in the case of cutaneous sensibility has been extended to other forms of sensation and to such manifestations of cortical activity as speech that we can expect to understand the nature of the relations between nervous and mental activity.

The experience of war has recently taken Head, working in conjunction with Riddoch[2], back to the spinal cord. It might be thought that there would be little here to interest us as psychologists, but the investigations of these workers have brought out many features of the functions of the spinal cord, when separated from the rest of the nervous system, which make a great addition to our knowledge. They contribute greatly to the solution of the problem, so vitally important to the psychologist, of the evolution of the reactions by which animals have adjusted themselves to the progressive increase in the complexity of their environment.

Especially instructive from this point of view is the discovery of the 'mass-reflex.' This is a mode of reaction of the isolated spinal cord, one of the most primitive kind, such as would ensure the bodily withdrawal of an animal from noxious stimulation. This reflex is so out of keeping with later modes of reaction that it has been wholly suppressed and buried, and its presence has only been revealed by one of the cruellest accidents of war.

Such clinical investigations as those of Head and his colleagues bring us to the physiology of the nervous system. Here again we have a subject which should take a prominent place in the deliberations of this Section. Especially through the work of Sherrington[3] we have been provided with a large body of knowledge which, though gained by purely physiological methods and purely physiological in character, should nevertheless be of great interest to the psychologist, for it reveals a general plan of neural

[1] *Brain*, 1918, XLI. 57.
[2] *Ibid.* 1918, XL. 188, 264.
[3] *The Integrative Action of the Nervous System*, London, 1906.

function with which any plan of mental function must be in harmony. In work on the activities arising out of the complex relations of binocular vision Sherrington[1] has shown that his integrative mechanisms apply to a psychical process of a relatively high order. This should point the way to other work designed to bring out the relations between physiological and psychological integration.

Another product of the recent physiology of the nervous system which I should like to mention is the 'all-or-none' principle, the demonstration of which we owe to Adrian and to Keith Lucas[2]. This principle, originally shown by these workers to characterize the activity of the isolated nerve fibre, holds good in large measure of the protopathic form of cutaneous sensibility and of the recently discovered 'mass-reflex.' Moreover, there is reason to believe that the 'all-or-none' principle applies to a large extent to emotional activity and to all those instinctive reactions which are emotional in nature. All these reactions have the character that, if they take place at all, they tend to appear in their full strength without that delicate discrimination of appreciation and graduation of response which are shown by reactions associated with intelligence.

The lines of inquiry from which these instances have been taken furnish so vast, and in some respects so highly specialised, a field that it might be thought they should form the work of a special Section of this Society, a Section of Physiological Psychology. I hope, however, that this step will not be taken, for I believe that it would be carrying the principle of specialisation to a harmful length. Rather would I advocate that this line of inquiry should be divided between this Section and that of Experimental Psychology, whenever such a Section is founded. I should like to see the physiology of the nervous system serving as a bond between the work of our Section and other branches of psychology. It has been a special feature of the experimental psychology of this country that, at any rate in its early days, it was clearly linked with physiology. I hope that this close relation will continue to form a bond linking our work with that of other Sections of the Society, so that they shall form an harmonious organization working with a common purpose, and with common principles, towards the better understanding of that which makes man what he is, which makes human society what it is—the Mind.

[1] This *Journal*, 1904–05, I. 26

[2] *Brain*, 1918, XLI. 26.

SOME MEASUREMENTS OF THE ACCURACY OF THE TIME-INTERVALS IN PLAYING A KEYED INSTRUMENT.

By W. B. MORTON.

(From the Physical Laboratory, Queen's University, Belfast.)

§ 1. *Experimental arrangement and method.*
§ 2. *Playing 'three against two.'*
§ 3. *Spacing as influenced by accent and grouping.*
§ 4. *Accuracy and regularity attained in single intervals and groups.*
§ 5. *Accuracy and regularity as affected by rate of playing.*

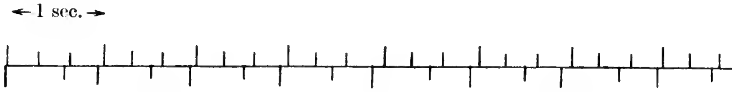
§ 1. THESE measurements were begun, some years ago, with the primary object of finding out the degree of accuracy attained by pianists in the feat of playing in double time with one hand and triple time with the other. The same experimental arrangement was later used to examine some other points which suggested themselves, in particular the influence of the accent on the spacing. The experiments were not carried out on any carefully arranged plan and were discontinued rather than finished. But some of the results obtained are perhaps worth putting on record.

The keyboard utilised for the purpose of the experiments was one of eight keys attached to a small wind-chest used for lecture experiments with organ-pipes. A brass pin was fixed into the end of each key-lever, and this pin rested in contact with a brass pillar, the contact being broken when the key was depressed. The contacts of the first four keys were included in the electric circuit of a chronograph tracer of the type used in physiological experiments, and the other four keys acted on a second tracer. The tracing points bore, side by side, on smoked paper carried on a drum revolved by hand, thus giving two close parallel lines. When a key was depressed the corresponding line was broken to one side. The two traces were crossed by the wavy line made by a point attached to a prong of an electrical tuning-fork making 128 vibrations per second. By counting, under a reading microscope, the number of tuning-fork waves between two breaks of the straight traces, the corresponding interval could be determined in periods of the fork and so in seconds. It was easy to estimate a tenth of a wave and so the time could be got to within about $\frac{1}{100}$ sec. The accuracy of the arrangement was tested by using a pendulum break and its errors were found to be small compared to the irregularities of the intervals measured. No attempt was made to reach extreme precision in the absolute values of the time intervals; so the fork was not tested for a possible deviation from its nominal rate. A metronome was used to give the player a preliminary idea of the rate at which he was to strike the keys, the metronome being stopped during the actual experiment. It was found that at M 120 for the bar, *i.e.* two bars per second, the number of waves counted to the bar did not differ sensibly from the theoretical value of 64.

It is of course obvious that the arrangement described could only reproduce very imperfectly the conditions of actual playing on a piano. The 'touch' was lighter and looser, and the sounds were wanting. It is doubtful how far this might modify the results obtained. I am inclined to think that the muscular movements concerned are too automatic to be much affected in their rhythm either by the sounds heard or by the reaction of the mechanism. It will be convenient in describing the experiments to ignore the dumbness of the keyboard and call each depression of a key a 'note.'

§ 2. In the initial experiments on 'three against two' I had the kind assistance of two friends who are professional musicians and good pianists. The two hands of the player acted on the two halves of the keyboard so that the movements were recorded on the two separate traces. The traces having been measured it was possible to obtain a visible representation of the playing by marking off the successive intervals along a line on squared paper. In this way the irregularities, even of the best traces, become glaringly evident to the eye. But even at the slowest rate adopted, of M 60 (one bar per second), the errors do not amount to more than about $\frac{1}{10}$ second and are, I suppose, quite unnoticed by the ear.

I reproduce a diagram of a portion of an average record which brings out an interesting point. This player's left hand was inclined to hurry on the right. In this case the right hand was playing in triple time (marked above the line) the left in double time (below the line). It will be seen that the first interval in the double time is longer than the second so that, in spite of the hurrying, the unaccented note of the double time bar shall come into place between the two notes of the triple time. This seems to indicate that the two hands are not really acting independently but that it is the pattern made by the combined systems of movements which is presented to the player's attention.



Right hand in 3-time. Left hand in 2-time. M 60.

§ 3. Coming to the general question of the regularity of spacing, one knows that the performance of a piece of music depends greatly for its expressiveness, and in particular for its 'phrasing,' on small variations of the *tempo* from the strict time-values indicated by the notes. This would seem to apply specially to a melody played on an organ where the performer is unable to give emphasis to particular notes by increased loudness. Considerations of this kind suggested the possibility of the spacing being affected by the fall of the accent, though it did not seem clear *a priori* in which direction the influence would be exerted. So experiments were made to test the point. At first I tried evenly spaced taps with one finger on a single key, the player grouping these mentally, first in twos and then in threes. The results were negative; no doubleness or tripleness could be distinguished in the sequence of measured intervals. However this failure may have been due to the conditions imposed by the defects of the apparatus. For it was necessary that time should be given for the key to make contact again between one tap and the next; this meant that the rate of playing had to be rather slow (about two notes per second) and the fingering very staccato.

To get over this difficulty the notes were next played by two fingers on the 4th and 5th keys so that the fingers acted on the independent circuits of the two tracers. Records were made by 21 different persons, who varied from practised pianists to one or two who required to concentrate their attention in order to bring the fingers down alternately. In general the second and third fingers were used, and the rate was M 120 for the bar, *i.e.* the interval between successive notes was about 32 vibrations of the fork. Each player made two records, accenting first the second finger and then the third. Examination of the traces did not disclose any universal rule. In some cases, generally those of the least expert players, the longer interval followed a definite finger, irrespective of the accent. But in the great majority of the remaining cases the shorter average interval followed the accent. In an extreme instance the average interval following the accent measured 26·5 vibrations and that preceding the accent 47·0, but generally the difference was not more than three or four vibrations.

After obtaining this result it occurred to me that the effect, if a real one, might be a matter rather of the grouping than of the accent. It is usual to regard the strong beat as beginning the group or bar so that the result might mean that a longer interval came between one group and the next than between the two notes belonging to one group. This seems a plausible supposition. Accordingly I tried a few experiments in which the player was told to group the pairs of notes in an iambic manner, weak-strong. The result was to confirm the above-mentioned surmise, for the shorter interval now preceded the accent. But it would require a more extended series of experiments to make certain of the precise relation of time-interval to grouping and accent.

§ 4. As regards the degree of accuracy and of regularity attained by the players an idea of this can be given by stating the average interval between the notes, the least and greatest values of this, and the 'standard deviation' (the square root of the average of the squares of errors from the mean). The accuracy of course varies very greatly with the expertness of the player. In the best case which I met with, the average interval (over 22 notes) was 28·7 vibrations, the least 26·7, the greatest 30·0, the standard deviation 0·96. At the other extreme I find an average of 32·7 varying from 22·9 to 46·6 with standard deviation 5·56 vibrations.

It is interesting to compare the regularity of the bars or groups of two notes with that of the single notes. Since the rhythm of the bars is what is given to the performer by the regular beating of the metronome before the experiment, one would expect this to be maintained more steadily than the rhythm of twice the rate, into which it is divided by the separate notes. This proves to be the case in every instance I have examined. There is a tendency to follow an interval which is shorter than the average by one which is longer and so to preserve the length of the bar more nearly correct. In terms of the standard deviations the greater regularity of the bars is shown by the fact that the standard deviation of the bars is less than $\sqrt{2}$ ($= 1{\cdot}414$) times that of the single intervals. To show this, let $a =$ the average length of a single interval, $2a$ of the bar. Let the actual lengths of the n intervals be $a + x_1$, $a + x_2 \dots a + x_n$, where n is the (even) number of intervals. Some of the x's are negative so that their sum is zero. The errors of the bars are $(x_1 + x_2)$, $(x_3 + x_4) \dots (x_{n-1} + x_n)$. Let s_1, s_2 be the standard deviations of single intervals and bars respectively. Then by definition,

$$s_1^2 = \frac{1}{n}(x_1^2 + x_2^2 + \dots + x_n^2),$$

$$s_2^2 = \frac{1}{\frac{1}{2}n}\{(x_1 + x_2)^2 + (x_3 + x_4)^2 + \dots + (x_{n-1} + x_n)^2\}$$

$$= \frac{2}{n}\{(x_1^2 + x_2^2 + \dots + x_n^2) + 2(x_1x_2 + x_3x_4 + \dots + x_{n-1}x_n)\}$$

$$= 2s_1^2 + \frac{4}{n}(x_1x_2 + x_3x_4 + \dots + x_{n-1}x_n).$$

If the positive and negative errors are distributed at random then when n is large the second term on the right is small and s_2 becomes approximately $= \sqrt{2} \, . \, s_1$. If s_2 is found to be less than $\sqrt{2} \, . \, s_1$ it shows a preponderance of negative terms in the bracket, and this implies that the consecutive x's multiplied together are opposite in sign; in other words, an interval which is too long is followed by one which is too short, so that the bar-length tends to be preserved.

Taking as examples the extreme cases quoted above, in the good case $s_1 = \cdot 96$, $\sqrt{2}s_1 = 1{\cdot}36$, $s_2 = 1{\cdot}12$; in the bad case $s_1 = 5{\cdot}56$, $\sqrt{2}s_1 = 7{\cdot}8$, $s_2 = 7{\cdot}4$.

§ 5. In addition to the experiments carried out at a prescribed metronome rate, I obtained some records made by pianists who played a 'shake' with two fingers at their maximum speed. The rates recorded were in the neighbourhood of 10 notes per second, the average intervals ranging from 10·8 to 13·2 vibrations (one vibration $= \frac{1}{128}$ sec. approximately). There is a very great falling off in the regularity of the fingering at these high speeds. As compared with the values obtained at the slower rates of about 4 per second the standard deviations are much greater, not merely relatively to the interval, but in absolute value. For example, the player whose deviation was only ·96 of a vibration with a time-interval of about 30 showed a deviation of 2·12 when the interval was 12·4, the extreme values recorded being 8·2 and 17·8. Traces made by another pianist gave, at the slow rate (M 120), an average interval of 31·1, extreme values 27·5 and 33·3, standard deviation 1·35; at the fast rate with average interval 10·8 the extreme values were 7·6 and 13·4 and the standard deviation 3·91.

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SOME EXPERIMENTS IN LEARNING AND RETENTION.

BY MAY SMITH AND WM. McDOUGALL.

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I. BERGSON'S DISTINCTION BETWEEN HABIT AND MEMORY.

THE following experiments represent an attempt to obtain by the method of experiment direct evidence of the influence of conation in some of the learning processes and to show in those processes its relation to mechanically determined associations. The investigation started from the contention of Professor Bergson that the word 'memory' is too frequently used as equivalent to the power of forming mechanical associations, whereas such a use overlooks those recollections which in their very nature are unique experiences, but which are not the less effectively reproduced. "The past survives under two distinct forms: first in motor mechanisms, secondly in independent recollections.... The memory of a lesson remembered in the sense of learnt by heart has all the marks of a habit...; the memory of each successive reading has none of the marks of a habit....It may be urged that these two recollections, that of the reading and that of the lesson, differ only as the less from the more and that...the lesson learnt is but a composite image in which all the readings are blended....But it is no less certain that each of them considered as a new reading and not as a lesson better known is entirely sufficient to itself....Of these two memories one is memory pure, the other is habit interpreted by memory[1]."

In most of the experiments undertaken to study problems of memory there has been an almost exclusive attention to those forms of retention

[1] Bergson, *Matière et Mémoire*, Ch. II.

which are described by Professor Bergson as being of the nature of habit, *i.e.* they have depended on the formation of associations by frequent repetition and have then been interpreted as holding true of all retention[1]. In actual experience it is not possible to fulfil all Professor Bergson's theoretical conditions, as the two factors called by him 'habit' and 'pure memory' co-operate in any given concrete case; but it is possible experimentally to bring about experiences which approximate to one rather than to the other. If all memory is of the habit type, then all tests which involve to any extent retention ought to correlate with those in which motor habit plays an obvious part, or at least they should correlate as highly with such as with others. If on the other hand Professor Bergson's distinction is fundamental, then we should expect the tests concerned with retention to fall into two groups; those of the habit type should correlate together and those predominantly involving pure memory should correlate together; but success in the one group would not necessarily carry along with it success in the other, *e.g.* we might find that people markedly good at tasks of the one type are weak at those of the other.

The tests chosen were intended to illustrate both modes of retention, *i.e.* reproduction of unique experiences and reproduction after repetition.

II. Description of the Tests.

The Blot Test. Ten cards on each of which was a large ink blot of irregular shape were shown to the subject in succession for 5 seconds each. These cards were then shuffled with 10 more of a like character which the subject had not seen. The 20 cards were then shown to the subject successively, at the rate of one card per two seconds, who was asked to say 'yes' or 'no' according as he had or had not seen the card before. Errors were of two kinds, *viz.* cards falsely recognised as having been seen, and cards unrecognised which had been seen. In working out the results the two kinds of error were not kept separate.

The Picture Test. In this a simple picture was shown to the subject for 10 seconds, at the end of which time he was asked to write a sufficiently detailed account of the picture to enable any person, with the requisite executive ability, after reading the written account to draw the picture. The pictures chosen had not previously been seen by the subjects. In selecting the pictures it was essential that they should be clear and unambiguous, without much detail and with definite colours; and it was found advisable to avoid anything which might offer an arithmetical

[1] See McDougall, *Body and Mind*, Ch. XXIV.

challenge, *e.g.* rows of nails on a box, as some subjects tended to spend all their time counting.

Most of the errors were errors of omission. For marking purposes the picture was divided into a number of units, and one mark was awarded for each correct unit.

The Prose Test. A passage of prose was read through slowly once, after which the subject was asked to reproduce the substance in detail, aiming at the meaning rather than at verbal reproduction. For marking, the passage was treated as the picture[1].

Learning of Nonsense Syllables. Lists consisting of ten nonsense syllables were used. The lists were clearly printed on a card and each syllable was presented through a hole in a sheet of cardboard of such a size as to exhibit one syllable only. The experimenter moved the card so as to present each syllable to the subject for one second, and the subject was instructed to read the syllables aloud as they appeared and to make an effort to learn them by heart. The number of repetitions required to learn the series was noted. The syllables were all constructed mechanically and pains taken to avoid those suggesting any meaning.

The Typewriter Test. The typewriter used was an old-fashioned "Hammond" with black keys resembling those of a piano. Three keys (*a*, *b* and *c*) were distinguished from the others by being painted a light colour. The subject was instructed to start at the middle key (*b*), then to strike the marked key to the right (*c*), then to strike the middle key (*b*), then the one to the left (*a*) and back to the middle key (*b*), keeping the elbow rigidly extended[2]. The two keys (*a*) and (*c*) were not equidistant from the middle key (*b*) which was the 'space key.' The result of these movements was to print two letters on the paper. After the letters had been printed five times, the subject was told to close her eyes and to continue the movements which, if correct, she was allowed to continue until the letters had been correctly printed five times. If, however, she made a mistake, she was told to open her eyes and to print the letters again five times and so on. The number of repetitions required before the movements could be repeated automatically, was taken as a measure of the facility of habit formation[3].

[1] Cf. Whipple, *Manual of Mental and Physical Tests*, 398.

[2] It was found necessary to insist on this as otherwise some subjects changed their method during the test; by insisting on this rigidity greater uniformity was obtained and higher reliability coefficients.

[3] This test was done first with the right hand and then with the left, but as there was great uniformity of place position in the results, the two have been taken together in working out the correlation.

III. Conditions of Experiment.

The subjects of these experiments were 41 women students and the experiments all took place at the same time every day, *viz.* between 1.30 and 3 p.m., each subject coming three days in succession and doing a complete set of the five tests each day. In all forty-one adults completed the sets.

IV. Method of Working out the Results.

The method of correlation used was the Product Moment Formula applied to ranks[1]. To get the reliability coefficient (Rel. Coef.) Dr Spearman's suggestion[2] was adopted, the results of the first and third day being amalgamated and their correlation with the second day's results worked out.

The probable error (in brackets) of the uncorrected coefficient was worked on the formula

$$\frac{\cdot 7063\,(1 - r^2)}{\sqrt{n}}.$$

V. Results.

	Blots	N.S.	Pict.	Pr.	T.	Rel. Coef.
Blots	—	·05	·53	·26	–·05	·50
		(·11)	(·07)	(·10)	(·11)	
Nonsense syllables ...	·05	—	·03	·48	·61	·68
			(·11)	(·08)	(·07)	
Picture	·53	·03	—	·22	–·11	·69
		(·11)		(·10)	(·11)	
Prose	·26	·48	·22	—	·34	·65
		(·08)	(·10)		(·09)	
Typewriter	–·05	·61	–·11	·34	—	·65
		(·07)	(·11)	(·09)		

The other results with a group of 12 children are:
Recognition test and 8 nonsense syllables ·03 p.e. ·19.
„ „ „ 10 „ „ –·03 „ ·19.

Of these tests the 'blots' and the 'picture' approximate to what Prof. Bergson calls 'pure memory,' inasmuch as each represents a specific experience unique in the history of the subject; there could be no question here of learning by repetition.

On the other hand, in the learning of the nonsense syllables and the fixing of the movements of the typewriter test there are all the characteristics of habit formation[3].

[1] W. Brown, *Mental Measurement*, 48–49.
[2] This *Journal*, III. Pt. 3.
[3] Bergson, *Matière et Mémoire*, Ch. II.

When we turn to the table of correlation we find that these two, the nonsense syllables and the typewriter, correlate together highly, *viz.* ·61, while the two tests where the habit factor is at a minimum, *viz.* the 'blots' and 'picture,' correlate together to the extent of ·53; but between each member of this latter group and either member of the other groups there is an absence of correlation, *viz.* ·05 and – ·05 respectively for the 'blots' with the nonsense syllables and the typewriter and ·03 and – ·11 for the 'picture' with the nonsense syllables and the typewriter. We thus seem to get two well-marked groups; (*a*) those tests of what Prof. Bergson calls 'pure memory' dependent on the meaning of the whole, which, treated statistically, show high correlation; (*b*) those tests showing all the characteristics of habit formation, which correlate with each other but not with either member of the other group.

Turning now to the 'reproduction of prose' test, we see that it shows an affinity with both groups, *viz.* ·26 with the 'blots' and ·22 with the 'picture,' and on the other hand ·48 and ·34 with the nonsense syllables and the typewriter test. In analysing the results there seems to be a considerable amount of literal reproduction: the passages chosen contained many well-known phrases, *e.g.* "a few years ago," which by the majority of subjects were reproduced *verbatim*, *i.e.* they represent verbal habits; and such reproductions amount to 39 per cent. of the total. The substance, however, as a whole was new to the subjects and so approximates to the non-mechanically determined type. This test is interesting as showing the interrelation of the two types in one experience.

One point of practical importance might well be considered here. To a degree greater than is generally recognised, learning at school resolves itself into the formation of mechanical associations between symbols, *e.g.* the association between the written and spoken symbol in reading, or between the corresponding symbol in another language, the learning by heart of tables, declensions, etc. All these must, for effective use, become mechanical by constant repetition. Now some children seem to find the making of such mechanical associations extremely difficult and hence will be handicapped in most of the subjects of a school curriculum (the exception will probably be scientific studies and handwork); such children may gain a quite undeserved reputation for stupidity[1].

Again it is possible that some of the discrepant results obtained in

[1] This will be particularly so when the curriculum gives a disproportionate amount of time to linguistic subjects; in such a case a child may learn to believe in its apparent stupidity and lose self-confidence.

the investigation of formal training may be due to ignoring this distinction: if the tests used depended mainly upon the power to form mechanical associations, a transference might be shown with this as a common factor.

VI. The part played by Conation in Learning.

Mechanical association having been shown to be but one factor in retention, it remains to be investigated whether the success of the learning process is affected by reliance on this factor. That is to say, is learning more successful when conation or the will to learn is operative, or when the influence of quasi-mechanical repetition is allowed full sway? The material forming the subject matter of these experiments was nonsense syllables, chosen because a sequence of such syllables has, as a whole, no meaning (hence it is possible to avoid the complication of the uncertain and incalculable operation of varying interest), and also because an indefinite number of sequences of equal difficulty can be produced. For learning purposes the syllables were printed on strips of paper fastened round a cylindrical drum which could be wound up to revolve at a given speed. A piece of cardboard with a window cut in it, placed in front of the drum, permitted but one syllable at a time to be seen; the machine was arranged to show each syllable for one second. The learner noted each reading of the set and kept a record of the number of repetitions required before the sequence could be reproduced by heart.

The standard row for these experiments consisted of twelve syllables and the writers of this paper were the subjects except when a statement is made to the contrary. The experiments fall into two groups according to the attitude of the learner; in one group the learner makes an effort to learn the sequence of syllables as quickly as possible, while in the other he relies on learning them by mechanical repetition, maintaining an attitude of passivity towards them except in so far as each one is read aloud as it appears. This passive attitude required much practice on the part of both subjects before it could be successfully maintained, and subjectively it was a less interesting mental state. Even the most rigidly constructed set of nonsense syllables acquires some meaning if the mind is allowed full activity; *e.g.* the place positions of certain syllables are noted, or some syllables are more interesting and pleasing than others. The learning is a problem the satisfactory completion of which is important, so that the total experience, while not perhaps to be described as thrilling, is nevertheless interesting. But in learning the series passively, all this excitement was absent, and the whole experience seemed 'flat.' After some experience both subjects could adopt the passive attitude at will.

Averaging about 20 experiments of each type we find in the case of one subject that 13 repetitions are required to learn rows of nonsense syllables when making the maximum effort to do so, and in the case of the other subject that 9 repetitions are required. When however no effort is made and reliance is placed only on the formation of mechanical associations by repetition, an average of 89 repetitions is required for one subject and 100 for the other. That is, from seven to eleven times as many repetitions are required to learn the same number of syllables by heart.

Again, if in the case of passive learning we compare each subject's performance at the beginning with his performance after some months' practice, we find a gradual increase in the number of repetitions required to learn each row; in the case of one subject the increase is from 39 to 165 repetitions and in the case of the other from 45 to 204, an increase dependent upon the increasing success with which the passive attitude was maintained. In other words, the greater the passivity and therefore the greater the rôle of mechanical determination, the greater the number of repetitions required, *i.e.* the less successful the learning.

But it may be that, when the learning has once been effected by this great expenditure of time, the material learnt is retained more effectively than when learnt actively; and there may be some such advantage in mechanical or passive learning. Now the test of effective retention is the success with which the material learnt can be recalled, and for experimental purposes the measure of that success is the number of repetitions required to relearn the same material after the lapse of a given interval. A comparison can thus be made between the number of repetitions required to relearn actively a set of nonsense syllables which has been learnt actively and the number of repetitions required to relearn actively a group which originally was learnt passively. If as many repetitions are required to relearn a group after an interval as are required to learn it at first, then the original learning has for practical purposes been of no avail: if the original learning has been perfectly retained then no repetitions would be required to relearn, *i.e.* the sequence learnt could be recalled at will.

Turning now to these experiments we find that, averaging 20 attempts in the case of one subject, nine repetitions are required to learn a set of syllables when an effort has been made and that four repetitions are required to relearn after an interval of 24 hours; *i.e.* there is a gain of five repetitions representing the effectiveness of the retention. On the other hand, we find that when no effort has been made, an average of 99 repetitions for learning requires an average of five repetitions to relearn; *i.e.* the retention is not as effective as when the original learning has been done actively, although there were considerably more repetitions. Averaging 20 attempts in the case of the other subject, we find that 13 repetitions are required to learn the syllables actively and 90 to learn them passively, and that to relearn the same series after an interval of 24 hours takes eight and seven repetitions respectively; *i.e.* the passive learning does in this case require one less repetition for relearning than the active learning, but the difference is not proportional to the extra time taken.

Subjectively there was, with many of the series learned passively, a feeling of absolute unfamiliarity when relearning began; whereas on relearning after having learnt a list actively there was always a recognition that the series had been experienced before.

This however is after a short interval; it may possibly be that, when the time interval is longer, the frequent repetition involved in the passive learning may have the effect of preventing the decay of the material learnt. To test this some experiments were made to measure the influence of the time interval. The contrasting time intervals were, (*a*) between 7 and 18 days for the long interval, (*b*) under 24 hours for the short interval.

(*a*) The records of two subjects were obtained. In the case of one subject 10 repetitions were needed to learn several series actively, and 6 repetitions for relearning after a long interval: 40 repetitions were required for passive learning and 9 repetitions for relearning[1]. The other subject required an average of 8 repetitions for learning actively and 5·6 repetitions to relearn the same after a long interval; when learning passively he required an average of 172 repetitions for learning and 7·5 repetitions for relearning.

With both subjects after a long interval the loss is greater when the initial learning has been passive than when there has been the will to learn; so that there is little difference between the number of repetitions required for relearning what has once been learned passively and the number of repetitions required to learn for the first time; whereas when the original learning has been active there is clear proof of retention. The effects then of passive learning wear off much more quickly.

[1] The figures state the averaged results.

(*b*) When the time interval is short, *i.e.* less than 24 hours, we get the following results. One subject requires an average of 10 repetitions to learn a series actively, and an average of 6 repetitions to relearn, and an average of 41 repetitions to learn a similar series passively and 4 repetitions for relearning; here there seems to be some superiority in the passive learning. The other subject requires an average of 7·5 repetitions to learn several series actively and 4 repetitions to relearn; and an average of 90 repetitions to learn similar series passively, and 5 repetitions to relearn; in this case there is an advantage on the side of the active learning.

Judged then by the standard of effective retention and recall over both longer and shorter intervals, passive learning stands condemned. Consequently the effort to learn plays a very real part in the learning process, and to assume that learning consists either solely or chiefly in the formation of verbal habits by mechanical association is to assign to the least important factor the dominant place. In only one case was there any superiority in the retentiveness after passive learning, and that was when the interval was short.

VII. IS THERE ANY IMPROVEMENT IN RETENTIVENESS?

All investigators of the learning processes have realised the somewhat irregular but general improvement which takes place if the same type of active learning is continued over some considerable period. This improvement, which may be due to improved concentration on the task, or to better realisation of the problem or of the means to attack it, is generally described as improvement of memory by practice[1]. Now it is easy to measure this improvement, as it is only necessary to compare the average of the repetitions required to learn several series of syllables at the beginning of an investigation, with the number required to learn the same number of series after some months of practice. But can we measure also whether with practice there is any improvement, not only in the power to learn but also in the power to retain? Improvement in retentiveness may conceivably prove to be a factor even in the improvement in learning; but if there be such an improvement it will show itself more purely in a measurement of retention. Evidence of it may be sought by comparing the repetitions required for

[1] The most elaborate investigation of this kind is that of Prof. Meumann and his collaborators. It is claimed that these experiments show great improvement of memory through practice. But the method does not attempt to distinguish between improvement in application to the task and improvement in retentiveness and we do not know of any attempt to measure improvement of retentiveness previous to our own.

relearning at the beginning and at the end of a given period of practice. We shall have, of course, the improvement due to improved methods of learning as a factor common both to learning and relearning; and if any improvement of relearning is exactly proportional to the improvement in learning, we may legitimately conclude that the improvement in both cases is the expression of the improved methods of learning, rather than to the improvement in retention. But, if there be a disproportionate improvement in relearning, so that relatively there is greater improvement in the relearning than in the learning, we may conclude that that improvement is due to a factor peculiar to the relearning process, which factor can only be improvement in the power to retain[1].

For these particular experiments six subjects' records are available. The period of practice for five subjects was six months, during which almost daily practice was given; and in the case of one subject the period was twelve months. The general method of procedure was as follows. On one day a group of syllables was learnt, and 24 hours afterwards that same group was relearnt, and so on. Below is a table of the results:

Subjects	Av. of 8 expts. giving no. of repetitions for learning at beginning	Av. of 8 expts. giving no. of repetitions for learning 6 months after	Gain	Av. no. of repetitions to relearn at beginning	Av. no. of repetitions to relearn 6 months after	Gain
A.	14	8	43 %	7	4	43 %
B.	16	9·6	40	5·6	3	47
C.	8	7	—	3	3	—
D.	9	5·6	38	2·8	1·2	57
E.	13	11	15	9	6	33⅓
F.*	12	8	33⅓	6	4	33⅓

* Interval 12 months.

It will be seen that the subject C. improves very little in either task; this subject was particularly good at all learning by heart and habitually relied upon the power.

Subjects A. and F. appear to make a proportional improvement both in learning and relearning, but in B., D. and E. there is more than a proportional improvement in the relearning; the improvement in learning being represented by gains of 40 per cent., 38 per cent. and 15 per cent. respectively; whereas the improvement in the relearning is

[1] It should be noted that no effort was made during the intervals to recall consciously the learnt syllables; any temptation there might be to do this rarely survives the first few experiences of learning nonsense syllables, and where the experiments extend over some years such extra repetitions can be ruled out.

represented by gains of 47 per cent., 57 per cent. and $33\frac{1}{3}$ per cent. It seems then as if in some persons at least, practice in memorising produces improvement not only in learning but also in the power to retain what has been learnt[1].

VIII. CONCLUSIONS.

In this paper we have adduced experimental evidence in support of Prof. Bergson's distinction between habit and memory; we have illustrated the great importance of effort or volition in rendering repetition effective in memorising; we have made experiments which constitute, we believe, the first attempt to investigate the question—Does practice in memorising produce improvement of retention as well as of the power to commit to memory?—and we have obtained results which seem to show that in some persons such improvement may be produced.

[1] For a discussion of the more philosophical considerations see McDougall, *Body and Mind*, Chs. XXIII and XXIV.

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THE PRESENT ATTITUDE OF EMPLOYEES TO INDUSTRIAL PSYCHOLOGY[1].

By SUSIE S. BRIERLEY.

I.

THE attention of psychologists has recently been drawn to the widespread opposition to the introduction of psychological methods into industry, existing among organized labour. It is found to be not the least strong among the more educated groups, and the importance of gaining the goodwill of these groups cannot be over-estimated, since they exert great influence upon Trade Union policy and general Labour opinion. The present paper is an attempt to state the workers' point of view, and to develop its psychological bearings, without making final judgment as to the rightness or wrongness of the detailed criticisms

[1] A paper read before the Industrial Section of the British Psychological Society on 28 November, 1919.

they offer. Their antagonism may be due in part to sheer misunderstanding; but it will be found also to raise many fundamental difficulties, which it is imperative that we should consider.

II.

It will be seen that it is impossible even to state the issues without taking into consideration certain controversies in ethics and economics. Some of the influences determining the attitude of the workers without doubt have their root in current social conflict. That does not, however, absolve the psychologist from stating and weighing them as psychological factors. It cannot be said that there is one set of events and issues which belongs exclusively to economics, another to ethics and another to psychology. There are no elements in human experience which have not their psychological value, whatever other values they may carry at the same time. In all that follows, then, I wish, for the purpose of this paper, to make no pronouncement as to the issues involved, save in so far as they are psychological issues.

It has to be noted at the outset, that it is by no means easy to get at the real minds and motives of the workers within the workshop itself, since there inevitably come into play instinctive defences again anyone, no matter how friendly and tactful, who does not belong to the workers' own social group, and still more so if he belongs in any capacity to the employing group. The workers themselves have assured me that this is in part deliberate; but there is no doubt that it is mainly instinctive and sub-conscious. Mr Ordway Tead has remarked[1] how he visited "a meeting of a local Trade Union in the Middle West, in the evening after an investigation of the shop in which nearly all the men worked: and the most noteworthy impression derived from the gathering was of the initiative and ability of men who in a conference held in the forenoon with the shop foreman had hardly dared to open their mouths"; and this is the experience of many students of working men. Whatever the psychological mechanisms involved, there is no doubt that the employer or investigator within the workshop is often unable to penetrate to any real extent the determining tendencies of the workers' minds.

(*a*) It may be replied to this that the psychological expert, as distinct from the employer, is exempt from the effective operation of these defences, by virtue of his appreciation of their mechanism; that while it may be true that the worker tends to regard him as belonging

[1] Ordway Tead, *Instincts in Industry*, London, 1919.

to the camp of the enemy, yet this hostility does not shut out the psychologist from an understanding of the worker's mind, since he is able to appraise that very hostility as indicative of conscious and unconscious motive. This indeed ought to be true. It is true in so far as the psychologist himself is free from certain assumptions inherent in the relations between capital and labour in industry; in so far as he insists on the obvious truth that the world of industry is not self-contained and self-sufficient, and cannot have a psychology unto itself; in so far as he claims not merely the right of impartiality between employer and employed concerning particular industrial problems, but the further right to consider these problems in relation to the psychology of the larger human world of society as a whole. If he is "above the battle" in this most comprehensive sense, the industrial psychologist will be able to penetrate the fighting defences of the workers and gain a reliable understanding of their motives and purposes.

If now we ask how far this essential independence and integrity are maintained, I am not sure that we are being entirely successful. You will understand that I am not accusing my fellow-psychologists of partiality as regards any conflicting interests of capital and labour in particular issues. Such a charge would be absurd and untrue, and I need scarcely trouble to disclaim it. The danger that I am inclined to suspect is from the point of view of our science perhaps a graver one. It is that we tend to allow the immediate practical needs of industry to dominate our outlook so much as to diminish our scientific autonomy. By so doing, it is true we achieve a simplification of our problems, convenient for temporary purposes. It is however a deceptive over-simplification, detrimental to the truth of our science and ultimately to its social usefulness.

(*b*) To consider an important example.—There is much to show that we are subtly influenced in our reading of industrial problems by a conception which is still generally current in the industrial world, and in many circles outside industry, the classic conception of the 'economic man.' That conception of a mannikin whose behaviour is actuated solely by one spring, the 'economic motive,' may or may not be a useful basis for the science of economics. It has been the traditional view that just as the mathematician, the physicist or the chemist must of necessity work with a single aspect of nature, considered in abstraction from all other qualities in the manifold of experience, so the economist must deal with this single abstracted aspect of human nature. Having worked out its implications and fully developed his science on this simple basis, he may then return to the complexities of actual experience, and modify his formulations in accordance with the full qualities of psychological fact. He may then make the necessary adaptations imposed by motives other than economic, and determined by the total of ethical and social purposes. Whether this high-minded procedure of the economist is sound and legitimate is however itself a psychological question.

I do not propose to develop here the question of its value for the science of economics. I am concerned with the fact that it is an actual working assumption, widely current in industrial thought, and one which appears to have exerted a real and perhaps unsuspected influence upon the psychologists' point of view. Its acceptance by psychologists is of course nowhere explicit, for merely to state it is to see its psychological falsity. The economist may possibly be able to argue *as if* that were true, but no psychologist can possibly subscribe to such a view, on deliberation. We may admit that self-preservation is one of the basic motives, possibly *the* basic motive, of human conduct. But the self that is jealously guarded, in a society so complex as our own, is much more than the mere bodily self that requires daily bread; there are ways of preserving it other than those that are referred to under the name of 'economic motive.'

Yet the industrial psychologist has not explicitly repudiated the economist's conception, and it is indeed possible to infer from his general pronouncements that he is subtly dominated by the supposed adequacy of the economic motive. For we do naïvely reply to inquirers as to the workers' attitude to our innovations, "Oh, they are quite content, for they have shorter hours and bigger wages!" appearing to imply that this is the essential key to the problem. I am not of course denying significance, and very great significance, to this factor, especially among the lower ranks of labour. I am deploring that we should give it single or undue prominence. I believe such a view to be a profound misunderstanding of the facts of the case, as regards those whose opposition will be the most telling, and the most difficult to remove.

(*c*) A further point in which I am not sure that we are maintaining the proper independence of our science, and one that lends colour to the plausibility of the economic motive, is in regard to our industrial aims. The partial purpose of 'increased production' has been too unquestioningly accepted as the reason for the psychologist's incursion into industrial method, and may too exclusively determine the scope of our studies. Again let me make a disclaimer. I am not challenging the necessity for increased production. No one can doubt its extreme urgency in the present social and international situation. Nor am I denying that the necessity is a legitimate and sufficiently good reason for the improvement of industrial methods. What I suggest is that our horizon as industrial psychologists must not be bound by this circumscribed end. We must put this into perspective, we must see it in its proper relation to the whole of which it is but a part. For from the larger social standpoint, the labourer is not merely a factor in production, but himself gives meaning to the process of production. Not only are his material needs to be met directly or indirectly by what is manufactured, but his life, the life of a human being, is to be expressed in the social medium that his work conditions provide. The office, the factory and the workshop are part of the field in which men and women live out their human relationships, part of the means by which citizens exercise their civic functions, part of the education by which they grow to full moral height. And the psychologist must remember these wider aspects if he is to understand the workers' responses. He must indeed face the whole problem of human nature in industry. He must not only serve partial ends, but must also ask, what is the significance for the worker's whole personality of his industrial actions and reactions. He must inquire how far labour conditions help, and how far they hinder the healthy and balanced expression of the deepest needs of a complete human being. The greater as well as the lesser purposes of human social life command the services of psychological science.

III.

We must, then, set on one side any presuppositions dictated to us by economic or other theories and practices, and approach our problem upon the broadest human basis, which is indeed the only basis for an adequate psychology. From this standpoint I wish briefly to discuss what appear to me to be the main factors determining the attitude of the workers to scientific improvements in management. I shall do so as briefly as possible, for I am anxious to have full discussion of the points raised.

The more straightforward issues I shall do little more than mention, as, *e.g.*, habit, and the general fear of the unknown. These are certainly operative, but their influence is not specific to our particular problem.

(*a*) A potent factor is suspicion, on the part of the workers, of the motives actuating the attempt to change industrial practices. The workers look for what they call 'the ulterior motive' behind every move on the part of the employer, or those working on behalf of the employer, to better the conditions of labour. It is often extremely hard to convince them that the ostensible motives of justice, efficiency and human decency are in truth the actual motives. The psychology of this is pretty clear. The suspicion is partly due to the normal functioning of the herd-impulse, when the group is in conflict with other groups, and partly to a reflective wariness,—the outcome of experience. The psychologist undertaking to reform industrial method thus enters upon an unfortunate legacy from historical conditions. This distrust is moreover subject to a pathological exaggeration, which itself serves to intensify the antagonism. Our concern cannot cease, however, with the recognition of a pathological element in the labourer's attitude. We must go on to ask what are its determining conditions; and in answering that question we shall inevitably be driven to survey the widest issues of social reactions in a complex industrial civilisation. Here we can only note in passing the suggestion made by Mr Graham Wallas, and developed by Mr Ordway Tead, in special reference to the life of the industrial classes, and supported also by many general considerations of pathology and physiology, namely, that it is the dispositions which that life 'baulks,' the instincts which it represses, that give rise to the pathological element in their reactions.

(*b*) A third factor determining resistance to psychological reforms in industry is the commonly recognised one of fear of unemployment. The interest of this motive for us, however, is not with regard to practical remedies, but concerns its relation to the larger and more intricate problem of the workers' solidarity. The term 'solidarity' often has a non-psychological meaning, that of the real or supposed unity of the social and economic interests of the wage-earning, as opposed to the property-owning classes. This is the significance which it frequently has for economic theorists and propagandists, although it may carry an indistinct psychological flavour at the same time. I am using it to refer simply to the psychological fact of actual awareness of unity, of behaviour loyal to those real or supposed interests. A study of this behaviour, of the manner, the emotional expressions, and the arguments of work-people in this regard reveals the familiar and characteristic working of the herd-spirit. In relation to the opposition to reform in industrial method, working-class solidarity manifests itself in two ways.

In the first place, there is with many men not only a fear of personal displacement as a result of changes in industrial conditions, but also a fine loyalty to those poorer in capacity and in market value than them-

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selves. Secondly, and perhaps more important from our point of view, there is a dread of anything that holds possibilities of the weakening of Trade Unionism and of danger to the hard-fought-for principle of collective bargaining. These are the sources of resistance to the introduction of, *e.g.*, differential rates of payment, premium bonus systems, etc. We cannot doubt that there is often an element of true generosity in the impulse to protect the weaker fellow. That is to say, psychologically speaking, its altruism has its unconscious roots not in the welfare of the particular limited and temporary group we are considering, but in wider, deeper, more ancient and permanent social pressures. Yet the student of human nature would find it hard to believe that a simple generosity towards inefficient fellows could ever prove strong enough to compel the highly skilled workman to resist, say, a system of payment that would be to his own distinct advantage. Such generosity would be merely quixotic if it occurred. The generous impulse must derive the greater share of its strength and poignancy from the fact that the inefficient workman *is* a workman. As such he has a significant bond with his high-grade fellow,—a bond considered to be more vital than that with any other economic group. Here again there is a rational element. The high-class and successful worker knows well the fortuity of those circumstances which, often independently of native worth, serve to bring about final differences in market value. He knows the enormous effect of early training, and the inevitable deterioration in social and economic worth that is brought about by long periods of involuntary unemployment, by diseases of occupation, by countless influences outside the will of the individual. The potency of the impulse of generosity is thus conditioned by the strength of the group-spirit of labour, and this again interacts with the impulse of self-preservation, and in fewer cases with devotion to the wider welfare of the whole community. The actual behaviour of the workers as regards the aspect we are discussing will be determined by judgments made at the behest of all these motives, which vary in relative strength among individuals. The clear demonstration of high personal advantage is sufficient to win the co-operation of many. Others will need to have it shown that the whole labour world will benefit permanently from the innovations. There can be no doubt of the relative ethical value of these two attitudes. My own experience leads me to think that it is precisely among the most intelligent and best-informed workers that the opposition to any method likely to weaken the general organization of labour will be the strongest and the most persistent. The sudden distraction of immediate self-interest for its leaders may suffice to disperse a mob. But where the reflective element is more prominent as a basis of grouping, the herd is likely to be more tenacious. The educated workers know how intimately the progress and welfare of their class have depended on group-organization, and they understand moreover that the general good of Society is enhanced by improvement in the standard of life of its labourers. The mere herd-impulse is thus with them largely transformed into an intelligent conjunction of purposes. In order, therefore, to win the goodwill of the most valuable sections of the working world, industrial psychologists need to take the widest possible survey of their problem. They need to base their exposition and appeal upon a consideration of these far-reaching issues, as well as on immediate and apparent advantages. I am indeed inclined to think, apart from the matter of the worker's goodwill, that the serious attention of applied psychologists should be given to the question of the influence of any particular industrial method on Trade Unionism and the group-spirit of the workers. This is one of the points in which we are bound to face responsibility for the social and economic effects of our suggestions, over and above their technical psychological merits and immediate effect upon production.

We are not, of course, directly responsible for the adoption or rejection of our methods. That is the concern of the industry and of the public. In our capacity as experts, we are responsible for making quite clear to the public mind the full psychological effects, in all directions, of our proposed developments of method.

From the point of view of political economy, there seems to be no doubt that a strong Unionism and a tenacious hold on the principle of collective bargaining is essential to the welfare of Labour, and hence to that of the community. And from the ethical standpoint, loyalty and devotion to the interests of labour groups is a fruitful education in public service. There is indeed a great field of psychological inquiry waiting to be entered upon here with regard to the relative social and educational values of the different minor groups to which the individual may give allegiance within the 'Great Society' itself. The question of the social value of those particular groups we call Trade Unions is much affected by the problem of craftsmanship which we are presently to consider. In so far as the Unions are Craft Unions, their function approaches that of 'professional' organizations of teachers, lawyers, medical men and so forth, the social usefulness of which cannot be doubted. For they are not concerned with economic relations alone, but maintain the honour and skill of their craft and provide a true education in the service of Society. But the tendency of developments in industrial technique is such as to destroy craftsmanship, turning Craft Unions more and more into Class Unions, concerned only with economic bargaining. The dangers and evils of sectional grouping are thus put at a premium and its benefits minimised.

(*c*) We come now to the last group of factors determining the workers' opposition. These are factors the discussion of which raises far the most fundamental difficulties, and whose significance we cannot begin to appreciate without going out beyond the bounds of the workshop into social and civic relations. We may perhaps summarise this set of influences by saying that the workman, stimulated by the growth of political democracy and by a measure of education, has arrived at and holds firmly to a conception of his own human dignity and of the basic value of human personality. This is now so vital to him that he dreads and resists any industrial development which appears to him to hold possibilities of increasing the already crushing effects of industrial life upon that personality.

To begin with one aspect of this issue, the problem of monotony. Monotony in work, arising from the subdivision and mechanization of labour and the stereotyping of operations, has been increasingly a characteristic of industrialism. It is widely believed by the workers that the analysis and re-synthesis of operations by motion study and time study will inevitably increase this tendency by further crystallizing the details; and that although energy may thus be saved, the increase in monotony will outweigh that economy in dynamic value. That may or may not be true. The psychologist usually makes the rejoinder that the monotony is there in any case, that the workers will fall into some stereotyped habit or other, and that it is better that that habit should be an intelligent one, based on science rather than on accidental circumstance. Such a reply appears to be sound argument, provided one takes the monotony to be inevitable. And it is extremely difficult to see how it can be obviated for the great mass of workers in large-scale industry. Yet it appears to me that in falling back on its inevitability we are rather shirking the psychological issues. It might conceivably turn out to be true that on psychological grounds the far-reaching effects of continued monotonous occupation were so dangerous to members of a highly complex civilisation as to make it worth while to re-organize our methods of production root and branch. I do not suggest that this is so, but only that it might be so; and that it is for the psychologist to develop its full implications. Münsterberg[1] meets the problem by showing that there are those who do object, and those who do not object to long-continued monotony in work. But that is a statement most provocative of further questions. Are those who find nothing irksome in repetitive work sufficiently numerous that with proper methods of selection there will be enough to go round all the monotonous tasks? More important, is that difference in response to monotony a simple innate difference? Or a complex psychological symptom determined by other more general tendencies and qualities of mind and character? If the latter, does it indicate an inferior or a superior type of personality? One that will respond adequately to the complex calls of a democratic community, which needs intelligence and responsibility among its members, or one that will be a dead-weight, a focus of indifference and even of evil? How far is that indifference to monotony a product of a particular type of education? And how far is that education one which we praise or condemn on other and wider grounds? How do those who do not object to monotony occupy their leisure? What contributions do they make to the moral and intellectual life of the community?

Another point made by some psychologists relative to this issue is that repetitive work can be carried out automatically, allowing attention to be given to thinking out other problems. To a greater or less extent, this is, of course, true. But is it a desirable thing? Is the condition of automatism and dissociation one that it is wise to induce for more than a very short period per day? It is, I suppose, true that small doses of it will harm no one and it may even give welcome relief from the strain of concentrated effort. But is it safe to build a complex social system upon such industrial conditions? What effect does such habitual dissociation for, say, eight hours per day produce upon other social reactions and upon the intellectual and emotional life of the worker? What indications we have are not in favour of this condition. Mr Wallas[2] records a case of one of his students throwing up a good job at Woolwich solely on grounds of the intolerable monotony. One of my own students, a man of admirable qualities, bearing his part well in all relations, a thinker, a steady self-controlled man, and moreover, one whose workmanly impulses gain much satisfaction in activities outside his office, has confessed how the monotony of his work in the Post Office has had, as he puts it, a "terrible effect" upon him, making him "feel he must shout or punch somebody"! How much more serious and insidious

[1] H. Münsterberg, *Psychology and Industrial Efficiency*, London, 1913.

[2] Graham Wallas, *The Great Society*, London, 1914.

must that effect be upon less stable personalities? These and many other questions fraught with meaning for the welfare of our civilisation, no one can yet fully answer. But upon this answer depends our final judgment as to the significance of monotony in work.

(*d*) Closely connected with the problem of monotony is that of loss of craftsmanship. There is a fear, widely held and clearly expressed, that time study, motion study and managerial methods based on these will, by the further sub-division and stereotyping of tasks, of necessity increase the inherent tendency of large-scale industry to destroy individual craftsmanship. To the expression of this fear one replies that the whole point of the analysis of operations is to increase skill and understanding, and that with this end in view the psychologists are the first to emphasize the need for the selection and training of workers. That reply, however, is met by the objection that it is a '*task*,'—a *job skill*—that is thus reached, not a *craft skill*; that, in fact, such analysis of itself leads to the greater specialisation and narrowing of operations. The selection and training of the workers, it is said, themselves lead further in this direction, when built upon time study and motion study. The point at issue, moreover, is not the desirability of skill and knowledge, but the question of the persons in whom that knowledge shall be deposited. It is a question of who shall be responsible for the planning of operations and the details of method. The workers fear that the scientific knowledge which underlies the choice of method on the new lines will in the end be divorced from the manipulative skill itself, and will become the possession of the management and the management alone. If to this one says that psychologists are stressing the need for the understanding by the workers of the modes of determination of time and motion, for the intelligent co-operation of the workman, and even for the opportunity and encouragement to improve and devise further methods themselves, the reply is made that this comparative freedom and knowledgeable co-operation appear only to be possible in the early stages of the application of these methods; that the full logical development of time study and motion study, of the control of rhythm and rest, is towards the complete specialisation of the knowledge on which the last details of repetitive movements are based. The mind of industry is then no longer to be found in office and workshop alike, as regards differing problems, but is concentrated entirely and in all respects within the office. The picture drawn by Mr Taylor of the group of pig-iron handlers, who, in his words, "more nearly resembled in their mental make-up the ox than any other type[1]," and who found their minds and wills lodged in the person of the man who held the stop watch, this picture is taken to represent the essentials of the end state of the industrial developments which are set going by psychologists.

It is to be realised that in these fears, English student workers are greatly influenced by their acquaintance with what actually has happened under American systems of 'Scientific Management,' and by the formulated objections of American Trade Unionists on these points. The considered opinion of organized labour there is that time and motion studies, combined with functional foremanship (which is itself a further instance of high specialisation), tend to "gather up and transfer to the management all the traditional knowledge, the judgment and the skill, and monopolizes the initiative of the worker in connection with the work; that it intensifies the modern tendency towards extreme specialisation of the worker and the task; that it splits the work up into a series of minute tasks, and tends to confine the worker to the continuous performance of one of these tasks, thus eliminating skilled craft, and depriving the worker of thought, initiative, sense of achievement and joy in his work[2]." Such a strongly worded summing-up of the experience of labour under particular systems cannot fail to have profound influence upon the attitude of English workers. Mr Taylor, himself, has, of course, openly avowed the purpose of transferring knowledge to the management, and when one urges that English psychologists are facing these problems, not with rigid fully developed 'systems,' but with the spirit and intent of full inquiry and research into all implications of their method, and that these evils may be by no means inevitable, one is met again by the expressed conviction that, once initiated, these processes will move on by their own momentum to the full logical development of extreme specialisation and mechanization of the workman. The workers inquire what safeguards against these essential tendencies we are able to devise. One that has been suggested is that no workman shall be kept for an indefinite period on one single task or narrow range of operations, but shall be shifted on to other tasks at periods, in order to develop adaptability and maintain interest and variety in work. It is replied that the economic interests involved are such that this shifting of the workman for reasons that are economically inadequate could never be more than occasional and sporadic. The reasons for such shifting are long-sighted reasons, and the practice

[1] F. W. Taylor, *Principles of Scientific Management,* New York, 1911.

[2] R. F. Hoxie, *Scientific Management and Labour,* New York, 1915.

would probably exert no immediate beneficial effect upon production. When a worker has by special selection and careful training been fitted for a particular task, to turn him on to another,—unless it were so closely allied that the change would have no value for our argument,—would rather involve an immediate and distinct loss to the employer, as well as multiplying records and rendering wage-accounting more complex. Employers being human tend to be short-sighted; and although it might be shown that in the long run the maintenance of interest and adaptability increased rather than lessened productive capacity, yet so long as the mere purpose of immediate production is over-emphasized by industrial philosophy, the employer has every encouragement to neglect the long-distance factors, to say nothing of the wide social effects of his practices. Thus, the workers say, there seems to be no way out of the dehumanising tendencies of the methods under discussion, in spite of their face value emphasis on the need for training and the reward of skill.

It will however be argued against these difficulties of the workers, and of course with every degree of justice, that the scientific study of industrial operations does not create the specialising tendencies of industry. Those are inevitable in any case. The real problem is no longer whether it is possible to return to mediaeval craftsmanship, but the detailed problem of how far and in what manner we can reap the fullest advantage of modern machinery, while avoiding its evils. And this full human control of machinery for human ends can only be gained when the science of the relation between man and machine is fully developed. We can only control what we understand; and it has been the blind wastes and inefficiencies of the past that have given rise to most of the evils that the workers deplore. I have said this to the workers in conference; and their reply brings us to the very heart of the matter. That reply is,—whether our science is able to serve the greater human ends depends entirely on how far we keep those greater ends in view. Science herself is impartial, and lends herself as easily to destruction as to construction.

(*e*) The workers ask, then, what are the ends which we are serving? When we speak of Production, they ask, "Production of what?" "Production of things or of men? Of goods or of human well-being and happiness?" It has been said to me over and over again, "There are things more important than mere production, and one of these is human personality." The criticism by these educated men of our emphasis on production is not on the fallacious ground of 'over-production,'—a fallacy they understand as well as ourselves; it is on moral and social grounds. They over-ride the artificial barriers which the sophisticated erect between economic, psychological and ethical questions, and ask that we shall view industrial processes in their proper relation to the full needs of human nature. They have even pointed out to me that our science is incomplete unless it deals with the wide social effects of technical processes. They do not deny the need for production, but demand some social guidance of that purpose in relation to moral ends.

Moreover, they are seeking to find in their daily occupation a true vocation,—one which shall develop them further in their manhood and employ the balanced powers of mind and body. It was asked on one occasion, "Is a man in industry sleeping or living? Is he just as in bed, marking time, existing, but not living? Or is he really living in the full human sense?" When one speaks to them of 'vocational tests,' one meets sometimes with derision. We wish to test, they say, not for a vocation but for a mechanical operation; and the term 'vocation' is meaningless in such a connection.

In response to this demand, one is able to urge that we are asking for a wider education, general and technical, in order that each worker may come to understand the part he plays in relation to the complete scheme of industrial activity. The workers admit that such an imaginative understanding of the relation of each bit of work to the great creative whole of industry is a most desirable thing, but they go on to urge that this cannot of itself satisfy the desire for creative work. It will rather aggravate the emotional dissatisfaction, unless there goes with it some measure of effective control of the industrial machine. If the worker himself and the part he plays in the industrial whole are directed entirely from without, the mere knowledge of how he is directed can be but an exasperation of his feelings of impotence and futility. Therefore, he argues, the assumption by the worker of some measure of genuine control of industrial processes is the only way in which it is possible to restore to the vast dehumanised machine of modern production any true satisfaction for the workmanly and creative impulses of the bulk of those whose destiny it controls. This is their answer to the problem of over-specialisation, to the question of how the technical psychology of industrial processes can be made to serve the greater human purposes.

IV.

Here, then, in the course of our inquiry, we are driven upon a controversial question of the first magnitude, one of those points in which psychology and ethics and economics are inextricably interwoven. The exact form or degree of control which can or should be given is not germane to our present discussion, and presents a problem bristling with difficulties of every order. I am concerned here only with the broader psychological aspects of the actual point of view of the workers, and shall not attempt to enter the controversy. It appears to me however very necessary that we should realise how much this demand for control is a psychological question, and how inevitably it grows out of the actual psychological conditions of the world as it is to-day. Even psychologists are not unknown to deal with this problem by labelling those who make the demand as mere 'agitators,' or at best as 'idealists'; but that is again a simple shirking of the issues. It is our business to inquire what conditions lie behind the demand, what are the psychological influences which have led to its formation. My own reading of the situation is that this conscious self-assertion and the desire for self-government in industry are the inevitable outcome of the growth of political democracy, of the complex demands that civic and national life is now making on the personality of all its members, and of the wider and freer education that recent years have brought to many. It is not so much a matter of the spread of this or that particular social philosophy. That is indeed an influential factor, but is probably itself but a conscious expression of deeper-lying actual tendencies. It is rather the actual calls upon individual and moral judgment, upon personal responsibility in the many-sided activities of our present-day social life, that are reacting upon that part of social life whose main function is the supply of material necessities.

Social psychologists have made clear what onus is thrown upon the individual moral judgment in modern ethical relations, and how increasingly the centre of moral gravity shifts from the external authority to personal responsibility. They have stressed the significance of the 'self-regarding sentiment' as the core of that type of character best adapted to the complex relations of our society. Far as the majority of people are from the highest type of character, the actual tendencies of moral development are all in this direction. Adequate response to political relations in civic, in national and international concerns shows the same call for individual judgment and responsible knowledge.

Students of political life, of electioneering methods, of the 'stunt' press, of the crowd spirit, of all the evil and dangerous aspects of democracy, stress again and again the need for a further development of personal intelligence and responsibility.

Pathological psychology joins its voice with social psychology in deprecating suggestion as a social method, and suggestibility as an individual condition. Educational psychology indicates that the way to avoid that condition is to call forth self-control, to give occasion for moral choice, even in the earliest years. The dynamic centre of education is now the individual, and the keenest minds are given to the problem of 'education for freedom.' This again is itself but an indication of the imperative call of the 'Great Society' for individual intelligence and responsibility. Analytic psychology, moreover, is making clear the dependence of personal and social health upon the satisfaction, in some degree and in some form, of the self-feelings; repression of the higher forms leading to their attachment to the lower. The simple doctrines of earlier social philosophers as to the essential egotism of human nature are now thrown into their proper expression; and the crude insistence on the instinct of self-preservation by those who base Society upon the 'economic motive' is now given its true psychological value. Self-fulfilment is shown to be a prime condition of mental and moral stability, for individuals and for social groups. There is, further, an increasing mass of evidence to show that this self-fulfilment is best achieved through creative activities in 'work.' It is interesting to students of social philosophy to observe how greatly the essential doctrine of Ruskin and Morris,—the doctrine of salvation through creative work in everyday life,—is being supported by so much in recent psychology. Whether one considers the psychology of little children as made clear by Madame Montessori's experiments, the analysis of the crimes and misdemeanours of adolescence, the emphasis now laid, for psychological reasons, upon constructive handiwork and art as an essential method of education,—everywhere one finds a stressing of self-expression through work in relation to everyday realities.

A similar weight is given to it by many physicians with regard to the re-education of the neurasthenic. In the words of Captain Brock, "Work is the one indispensable channel through which life has to be awakened in the young, or rendered back to the devitalized[1]"; but the work meant is creative activity which affords a real expression for the self, mechanical or routine occupation leading only to further dissociation.

[1] Capt. A. J. Brock, M.D., quoted in *Cambridge Magazine*, August 2nd, 1919.

V.

It is in the light of these widely garnered facts that the problems of monotony, of mechanization and specialisation in industry, and of the possibility of some kind of self-government by the workers appear so pregnant with meaning for the welfare of our civilisation. And it is for these reasons that we are impelled to view the problems of industry in their larger relations. One can see two diverging tendencies in modern life,—one is the increasing emphasis on salvation through work, more and more justified by psychological knowledge, the doctrine of 'vocation.' The other, the doctrine of 'leisure,' stresses the right use of the hours left over when work is done; a doctrine increasingly necessitated by the mechanical development of the material basis of life. The issue between these two is no academic one, but one most pertinent to the health and therefore to the permanence of industrial Society. It bears the closest relation to the detailed problems of the industrial psychologist, as, *e.g.*, the question of hours, and to the direction in which his methods shall be developed. And again we must say that it is at bottom a psychological question, one of the actual functioning of the human mind. Are the mechanical necessities of industry such as to allow, under conditions we can devise, the bulk of workers to find a vocation, a full healthy satisfaction for the creative impulses and self-feelings? If not, is it actually possible to have specialisation and mechanization within the 'Great Industry,' and response to the varied and complex calls of the 'Great Society' outside it? Is it psychologically possible to have docile, externally controlled workers in industry, who are yet free, intelligent and responsible members of a democracy outside it?

These are psychological issues we shall be driven to face sooner or later. And they are the actual issues presented to us by the thoughtful among the workers themselves. Everyone of these arguments has been put to me, often of course in crude and simple words, by student workers. They are not only able to articulate their own instinctive desires, but are also aware of the wide tendencies in education and even in psychology. As students of politics and social conditions, familiar with the evils and dangers of modern democracy, and with the efforts of education to create the right responses to social calls, they approach these problems of industry with the most intimate and earnest concern for their solution. But what they articulate, and are able to interpret in a scholarly form in relation to science and philosophy, is after all inherent in the attitude of the dumb driven mass, restless and dissatisfied, able to express its dissatisfaction only in terms of hours and wages. Education is often naïvely appealed to, and even by psychologists, as a panacea for 'industrial unrest.' The whole of history is an answer to such a view. Education is a safeguard against blind revolution; it does prevent the outbreak of merely destructive forces. But a wide and liberal education, such as will fit a citizen for his citizenship and a member of a great modern community for his part therein, will essentially militate against a routine function in industry, and will increase the strength and pertinence of the criticisms which the workers bring against monotonous over-regulated industrial life. This is again surely obvious on psychological grounds. It is known to be true by all those who have experience in the higher education of working people. It is just those who have read the widest and thought the deepest in literature, history, psychology, and I may forcibly add, in economics, who raise the most fundamental difficulties on the issues we have discussed.

It is then because I have found these arguments among the educated workers and felt the difficulty of answering them, that I have wished to develop the analysis of the worker's attitude. If we are to gain the co-operation of the most influential groups, whose goodwill would count most heavily in our task of psychologising industry, we must appeal on the broadest issues. Until we face, and show clearly that we are facing these larger, more vital problems, we shall not gain their full confidence in the smaller. Yet it is not only a matter of gaining the goodwill of the workers. It is that we do owe service to these larger purposes ourselves. The workers come to Psychology as to the human science, the science which, whatever else be prostituted to meaner ends, will of its essence consider the whole man, in all his relations. It is for us more than for any other science to lend our knowledge for the re-creation, not only of industry, but of human society. To do this we must see the lesser in relation to the greater, and keep our vision whole.

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SUGGESTION AND SUGGESTIBILITY[1].

By E. PRIDEAUX.

§ 1. DEFINITIONS OF SUGGESTION.

THE controversies between psychologists and neurologists as to the nature and treatment of the psycho-neuroses are largely due to the employment by both parties of the same words in different senses, and as a striking instance of this, the word 'suggestion' has been responsible for considerable confusion. For many neurologists suggestion is the beginning and the end of all diseases of psychogenetic origin, both as an aetiological factor and as a method of treatment. Even amongst psychologists there seems to be no real agreement as to the meaning of the word, and it is often used as if it were an explanation for a mechanism which is not understood.

The various definitions which have been given of the term 'suggestion' make it obvious that different degrees of the same process are being referred to. These definitions can be divided roughly into two classes according as they refer to (*a*) *Normal Suggestion*, which takes place in every-day life in all of us, or (*b*) *Abnormal Suggestion*, which takes place in psychoneurotic patients, and in normal persons under abnormal conditions. It is difficult to draw any hard and fast line between the two for the difference is only one of degree, and appears to depend on the individual tendencies of the subject, whereas the mechanism remains the same in both classes. The term 'suggestion' as used by Janet, Dejerine, Grasset, and Babinski refers to abnormal suggestion only. Janet defines it as "the complete and automatic development of an idea which takes place outside the will and personal perception of

[1] Read before the Medical Section of the British Psychological Society, 29 October, 1919.

the subject[1]," and Dejerine and Grasset hold a similar view. For Babinski the process is one of suggestion only when the idea conveyed is unreasonable[2]. The broadest definition is that of Bernheim, who defines it as "the process by which an idea is awakened in the mind of a subject and accepted[3]." This definition includes all varieties of suggestion, but does not clearly mark it off from other mental processes. The definition which is now often accepted by the English School of Psychologists is that of Dr McDougall given in his *Social Psychology*, *viz.*: "Suggestion is a process of communication resulting in the acceptance with conviction of the communicated proposition in the absence of logically adequate grounds for its acceptance[4]." This definition includes both normal and abnormal suggestion, but does not make it clear whether the action is limited to processes in which there is a relationship between two persons only, a limitation which seems to be unnecessary, as it excludes auto-suggestion. Dr McDougall could improve his definition and make it include every variety by making it read "suggestion is a mental process resulting in the acceptance with conviction of a proposition in the absence of logically adequate grounds for its acceptance[5]," and this is the definition which I put forward for the purposes of this discussion. Dr McDougall classifies suggestion amongst his general innate tendencies as a pseudo-instinct[6], and I think it would be profitable to discuss whether it is necessary to maintain this view or whether the process of suggestion cannot be explained in some other way.

We have then to explain why it is that we accept with conviction and act upon propositions made or occurring to us without any adequately logical grounds for so doing.

If there are logical grounds for accepting the proposition the idea is generally called *persuasion*, but it is difficult to separate this from suggestion, and it seems better to include it, at any rate when used in psychotherapy, as a form of normal suggestion, for there are often no logical grounds for accepting the proposition, but only logical grounds

[1] Janet. *Mental State of Hystericals*, 240.

[2] Quoted by Bernheim in *Automatisme et Suggestion*, Alcan, 1917, 55.

[3] Bernheim. *Hypnotisme, Suggestion, Psychothérapie*, 2e éd. 1903, 24.

[4] W. McDougall. *An Introduction to Social Psychology*, 12th ed. 1917, 95.

[5] Dr C. S. Myers in a communication to the discussion asks, "Do not inadequate logical grounds often act suggestively, *i.e.* induce a conviction far above their intrinsic merit?" He suggests a further improvement by substituting "apart from the intellectual outcome of pure judgment based on logical premises" for "in the absence of logically adequate grounds for its acceptance." With this I fully agree and would like to accept this improvement, for it at once removes the difficulty of separating persuasion from suggestion.

[6] *Op. cit.* 90.

on the part of the physician for persuading the patient to accept it; moreover to obtain conviction affective processes must come into play, for the way of saying a thing is more important than what is said, which is expressed in the statement "Manner is more important than matter."

There can be no doubt that *suggestibility* is the chief factor in the process of suggestion, and that the process is a subjective one; we have learnt as the result of psycho-analytical investigation that this state is not a passive state of receptivity, and that the mind cannot be compared to a vacant seat waiting for someone to fill it, as was originally held, but that it is the result of active mental processes going on in the mind of the subject and particularly of affective processes.

Dr Ernest Jones has called attention to the distinction between verbal suggestion on the one hand and affective suggestion on the other[1], and maintains that the latter is the more fundamental, and is the necessary basis for the former, which view accords with Bleuler's statement, quoted by Dr Jones, "Suggestion is an affective process[2]." This view seems to be by no means generally accepted, but a consideration of the facts with which we are familiar concerning suggestibility compels us to admit its truth, and an examination of those facts shows more clearly the general nature of the whole process of suggestion.

§ 2. Varieties of Suggestibility.

The chief facts are that (*a*) Suggestibility varies in different persons irrespective of the nature of the suggestion, and of the suggestor, (*b*) Suggestibility varies in the same person at different times and under different conditions, (*c*) Suggestibility may have reference to a particular system of ideas only, (*d*) A person may be suggestible towards one person and not towards another. I call these four distinct states of suggestibility: (*a*) Individual, (*b*) Conditional, (*c*) Specific, (*d*) Personal.

(*a*) *Individual Suggestibility.* The fact that suggestibility varies in different persons irrespective of the nature of the suggestion makes it important in psycho-therapy to be able to recognise what other characteristics are associated with exaggerated suggestibility. It is exaggerated in the child and diminished in old age; it is exaggerated in those whose egoistic instinctive tendencies are excessively developed and who make little attempt at self-control and so act on impulse, the class of persons originally described as having a sanguine temperament. This

[1] *Papers on Psycho-analysis*, 2nd ed. 1918, 319.
[2] *Op. cit.* 320.

class corresponds to the 'extrovert' of Jung, the 'motor' type of Baldwin, the 'objective' type of Bain, and the 'tough-minded' of James. Suggestibility is also exaggerated in crowds whose other characteristics are impulsiveness and incapacity to reason with absence of judgment and of the critical spirit. Le Bon points out that "the decisions affecting matters of general interest come to by an assembly of men of distinction, but specialists in different walks of life, are not sensibly superior to the decisions that would be adopted by a gathering of imbeciles[1]."

I think also that suggestibility is more marked in those who live in the South and warm climates than in those who live in the North and cold climates, and that those, whose associations of ideas take place by contiguity, are more suggestible than those who associate by similarity.

It is less marked in those who hold strong principles and ideals, in methodical thinkers, whose critical powers have been well developed, and in those with the so-called 'bilious' temperament, who correspond to the 'introvert,' the 'sensory' type, the 'subjective' type, and the 'tender-minded.' Suggestibility is very much exaggerated in the patient with 'conversion hysteria' and this has led Babinski to enunciate his conception that hysteria is due to suggestion. This conception has been accepted by many neurologists in this country, who have little knowledge of the mental processes at work in the process of suggestion and use the term in a very limited sense. When we recognise suggestion as an affective process, then we can agree with Babinski that hysterical symptoms are produced by suggestion, but we shall not be able to accept his view that hysteria be limited to the symptoms of 'conversion hysteria.' I therefore think it is unfortunate that Dr Rivers should have proposed the use of the term 'suggestion neurosis' as a substitute for 'conversion hysteria' in his paper "War Neurosis and Military Training[2]." Moreover so long as the present confusion exists in the meaning of the term suggestion the less we use it the better.

Investigations which I have been carrying out during the past year on the 'psycho-galvanic reflex' point to the fact that exaggerated suggestibility is always associated with a low 'emotive response,' and Dr Snowden informs me that similar results have been obtained by Dr Golla and himself at the Maudsley Hospital. If it could be shown that the converse is true, that a low emotive response is always associated with exaggerated suggestibility, then we should have a means of measuring suggestibility. More work needs to be done on this subject, for apart

[1] *The Crowd*, 11th impression, 1917, 32.
[2] *Mental Hygiene*, II. 519.

from the fact that we are not yet decided as to the physiological nature of the reflex, it seems certain from the psychological standpoint that two factors must be taken into account, the liberation of emotion on the one hand, and the stimulation of 'contrary' forces on the other.

I use the term 'contrary' forces in order to avoid the words 'repressing,' 'inhibiting,' and 'controlling.' I mean the forces which are brought into action by stimulation of mental processes on a higher level: they are 'contrary' as applied to suggestion, and act in opposition to the instinctive processes on the perceptual level. Physiologically they are the forces set free by stimulation of the cerebral cortex; following Dr Rivers in the symposium on 'Instinct and the Unconscious[1],' I might perhaps use Dr Head's term and call them 'epicritic.' It is possible that the psychogalvanic reflex may be an indication of the strength of the 'contrary' or 'epicritic' forces stimulated by the liberated emotion, and that it is not merely an emotive response.

From a consideration of these facts we can explain individual suggestibility as being due to the varying degree in which the egoistic instinctive tendencies are developed and the manner in which the sentiments have become organized to form ideals and act as contrary forces.

(*b*) *Conditional Suggestibility.* The variation of suggestibility in the same person at different times and under different conditions seems to depend upon the affective state in which the person happens to be, and the relation of the suggested idea to that state. I have found that even patients, who generally go into a deep state of hypnosis, are resistant to hypnosis on one day and will go off into their usual state on the next. Suggestibility is increased during hypnosis, fatigue, illness and prolonged emotional states, and by the effect of alcohol and certain drugs, conditions in which the 'contrary' forces are weakened. A wife, for example, recognises that a husband is more suggestible after a good dinner and chooses this time to get her propositions accepted.

(*c*) *Specific Suggestibility.* That suggestibility may refer to a particular system of ideas only is also an important fact pointing to the affective nature of the process. A person is specially suggestible to ideas that are pleasing to him and which satisfy his egoistic instinctive tendencies; according as those specific tendencies are developed so does his suggestibility vary towards ideas which evoke them, and we recognise that in the same family these tendencies are developed in each of the children in varying degree.

With the growth of sentiments and the appearance of complexes and

[1] This *Journal,* 1919, x. 4.

interests, both actual and dispositional, as the result of experience, so does the suggestibility vary according as the suggestions harmonize with the affective states induced by them. Thus each person has his own particular sphere of suggestibility, and even under hypnosis the suggestibility is not the same for all suggestions. This is more clearly explained by a quotation from a paper by Dr Jones on "Psycho-analysis and Education," "A desire that arises in a person's mind for the first time is not likely to be very effective or significant unless it becomes attached to others that are already present; in other words a motive appeals more readily to him if it is linked, by resemblance, to earlier ones that are already operative in him[1]."

(*d*) *Personal Suggestibility.* Suggestibility towards one person and not towards another depends on the affective processes operating between the two persons. Sympathy, respect, and confidence between the subject and the suggestor favour suggestibility. I have found it more difficult to hypnotize the patient of a colleague than one of my own.

Anything which tends to increase the authority and prestige, either personal or acquired, of the suggestor, increases suggestibility in the subject; thus a parent can produce suggestibility in a child, a teacher in a pupil, and a physician in a patient.

Dr McDougall has pointed out that the personality comes into play in virtue of the relative strengths of the two instincts of 'self-assertion' and 'subjection.' "Personal contact with any of our fellows seems regularly to bring one or other, or both of these instincts into play[2]," so that suggestibility is only evoked in us by persons who make upon us an impression of superiority of any kind in the particular situation of the moment. Dr McDougall relies on the strength of these two instincts to explain individual suggestibility; but although they are important—especially the instinct of self-assertion in virtue of the part it plays in the organization of the sentiments on a higher level—other instincts play an equally important part. It is probable that prestige owes its power to the complex emotions of admiration and awe, and often of gratitude and reverence, which are evoked by the instincts of curiosity, subjection, self-preservation, and the parental instinct. Whether we accept Freud's view, that the above tendencies are but sublimations of the sexual instinct, or not, we are bound to admit the influence of the sexual instinct, for we know that a sentiment of love or affection favours the sympathetic induction of emotion between two persons. According

[1] *Op. cit.* 583. [2] *Op. cit.* 99.

to Ferenczi: "Everything points to the conclusion that an unconscious sexual element is at the basis of every sympathetic emotion, and that when two people meet, whether of the same or the opposite sex, the unconscious always makes an effort towards transference[1]," and that this transference has its deepest roots in the repressed parental complexes. It is also significant that in the two classes of homo-sexuals, described by Ferenczi[2], the 'subject' and 'object homo-erotics,' the 'subject-homo-erotic' has an increased suggestibility, and although my experience of these cases is small, it is that the 'object-homo-erotic' is not at all amenable to suggestion.

It is outside the scope of this discussion to go further into the psycho-analytical standpoint, and Dr Jones has already set forth the Freudian point of view in his paper on the "Action of Suggestion in Psychotherapy[3]," to the effect that 'suggestion' is a special variety of transference, namely, that concerned with the transference of positive affects to the physician, and that suggestibility takes its root in the masochistic component of the sexual instinct. It is impossible for anyone to discover the truth of Freud's theories without psycho-analytical investigation: my own position is that I accept the greater part of Freud's theories in so far as the fate of the 'pleasure-principle' is concerned, and his theory of sexual development and sublimation has been confirmed by my experience in psycho-analysis, but I think that the development and sensitivity of the 'reality principle' is of much more importance than Freud seems to allow and that Janet is right in so far as he emphasizes its significance.

A consideration then of facts shows that all four forms of suggestibility, which I have described as individual, conditional, specific, and personal, come into play in the process of suggestion, and that these are affective states evoked by the stimulation of different instinctive tendencies, sentiments, interests and complexes.

[1] Ferenczi. *Contributions to Psycho-analysis,* 1916, 55.

[2] *Op. cit.* 253. Ferenczi uses the word 'homo-erotism' as being preferable to the ambiguous expression 'homosexuality,' since it makes prominent the psychical aspect of the impulse in contradistinction to the biological term 'sexuality.' He holds that of the two types of homosexuality, the passive form alone is one of true inversion, when homoerotism occurs through subject-inversion, and therefore he calls this type the 'subject-homo-erotic.' In the 'active homosexual' the object alone is exchanged, and so Ferenczi refers to this type as the 'object-homo-erotic'; he regards this latter form of homo-erotism as being an obsessional neurosis.

[3] *Op. cit.* 318 *et seq.*

§ 3. CLASSIFICATION OF THE RESPONSES TO SUGGESTION.

Any explanation of suggestion must explain not only why suggestions are accepted, but also the circumstances under which they are refused, and even strongly opposed. If we take the results of attempts at suggestion in everyday life we can classify them into three groups:

(*a*) Positive response when the suggestion is accepted.

(*b*) Negative response when the suggestion is opposed.

(*c*) Neutral response when the suggestion is refused.

These results depend on the relationship of the suggested idea to the different states of suggestibility already described.

(*a*) *Positive Response* may be immediate or delayed; the immediate response gives us the most typical example of the process of suggestion, for in the delayed response there are also at work other factors which I shall describe under 'neutral response.'

Most writers are inclined to the view that if a suggestion is accepted it is due to the inhibition of other ideas opposing its acceptance and that realisation of the idea takes place simply by ideo-motor action. This view involves the difficulty that it depends on the meaning of inhibition, and that we do not understand the nature of ideo-motor action.

Dr Hart, in his paper "Methods of Psychotherapy[1]," attaches great importance to inhibition, but recognises that it is brought about by affective processes; for him 'suggestion' is 'complex thinking,' by which he means thinking due to the action of a complex, using the term complex in a very wide sense. He speaks of the capacity of suggestion 'for inhibiting conflicting ideas and tendencies.' This seems to be an inadmissible use of the word 'inhibition': the verb 'to inhibit' is an active and transitive verb, and the word 'inhibition' thus conveys the idea of an active process. If I go downstairs, it might be true to say that by so doing I was inhibited from going upstairs, but it would hardly be a correct usage of the word; the idea of going up would never arise and would not require inhibition. Suggestion has no capacity for inhibiting ideas, but, if we speak in terms of inhibition, is rather the consequence of the inhibition of inhibiting forces normally involved in volition.

Moreover, the term 'complex thinking' lays too much stress on the cognitive aspect of the process of suggestion, and though this is the first stage in the process, more than this seems to be involved; for example, if, during a railway strike I merely thought of an engine as the means of transport for getting me to town, nothing further would result, but

[1] *Proc. R. Soc. of Med.* March, 1919.

if forces were aroused in me sufficiently strong to make me 'tip' the engine-driver, then I should be acting under the influence of suggestion. No other ideas would arise if the response was immediate, and no ideas would be inhibited.

The term 'ideo-motor action' is a relic of the old psychology of ideas; for example, for Hegel, "an idea is a force and is only inactive in so far as it is held in check by other ideas[1]." If the process of ideo-motor action be analysed it is found that the action depends entirely on the affective forces aroused by the idea and that no idea will realise itself, unless it is reinforced by some affective force. Ideo-motor action is thus equivalent to the expression of emotion. We know that emotion is expressed normally through the autonomic nervous system, and when excessive, through the central nervous system[2]. It is evident that persons, whose instinctive tendencies are highly developed, and whose sentiments have not been well organized to act as 'contrary forces,' will realise their ideas in action through the central nervous system without opposition, and I have shown elsewhere that this may be an explanation why a patient with conversion hysteria develops symptoms attributable to the central nervous system and a patient with anxiety hysteria develops symptoms attributable to the autonomic nervous system.

It follows from what I have already said that I here maintain the view that an idea is accepted because it harmonizes with some preformed interest, sentiment, or complex, that the affective forces involved give it the necessary reinforcing power to realise itself in opposition to all 'contrary forces,' and that it is these affective forces which produce conviction. Any condition which tends to weaken the 'contrary forces' on the one hand or strengthen the compatible affective forces on the other favours the process of suggestion, and we have noted that the conditions which cause 'conditional suggestibility' are those which weaken the volitional forces, and that individual suggestibility is exaggerated in those who are endowed with strong emotional tendencies and have a poor development of self-control.

[1] Hegel's *Philosophy of Mind*, 1894, 167.

[2] That emotions gain expression through discharges along the autonomic nervous system has been shown by the work of Pawlov, Cannon, Elliott and others, who have demonstrated the connexion between emotion and the physiological reflex reactions of the glandular secretions. In healthy adult individuals emotion may be experienced without any expression through the muscles supplied by the central nervous system, though as the emotion increases it requires a distinct effort to prevent it from being so expressed; when the emotion becomes more intense the control breaks down and at first only the facial and voice muscles are affected; finally, if the emotion becomes excessive, the muscles of the limbs and trunk are brought into action.

Dr McDougall has shown that it is the organization and strength of the self-regarding sentiment in relation to the other sentiments, which determines our line of action and constitutes our self-control; as this higher control, though it relies for its strength on the self-regarding sentiment, involves the formation of ideals and is perhaps influenced by the herd instinct, I have called it elsewhere the 'social ideal self' as a contrast to the 'individual self[1].' Individual suggestibility then depends very largely on the strength of the social ideal self, and the weaker the social ideal self, the greater the number of complexes that remain unsublimated, and the greater are the states of specific and personal suggestibility.

Mr Trotter has pointed out how one form of suggestion, 'herd suggestion,' is due to the action of herd instinct, and that "Anything which dissociates a suggestion from the herd will tend to ensure such a suggestion being rejected[2]." It is owing to the influence of the herd instinct that we may accept propositions in regard to religion, politics, and education. Such beliefs are non-rational and are accepted by us as the result of accumulated suggestions.

The exaggerated suggestibility of children, occurring when they have reached the age of paying attention, which is in its turn dependent on the interest aroused, is due to the evocation of the instinct of submission, the weakness of the social ideal self and the absence of resistance complexes. For opposite reasons old people are less suggestible.

(*b*) *Negative Response* is the response obtained when not only is the suggested idea incompatible with pre-formed sentiments and interests, but it arouses contrary emotions and sentiments. This is the process which is called 'contra-suggestion.' The mechanism is the same as for the positive response, but an opposing set of forces are set in action with the production of a state of 'negativism,' a state which is the direct counterpart to suggestibility. This state like suggestibility may have individual, conditional, specific or personal tendencies. It seems to be a form of overdetermination due to the presence of antagonistic complexes, which more than counterbalance the forces of a weak social ideal self.

Like suggestibility, negativism may be exaggerated and become pathological; it is most marked in dementia praecox.

Some people appear to adopt 'negativism' as a habit; such are the people we call 'cranks.' I look upon the action of these people as being

[1] Article on "Mechanism of Hysteria" in *Functional Nervous Disease*, 1920, IV.

[2] *Instincts of the Herd in Peace and War*, 3rd impression, 1917, 33.

that of overdetermination, owing to the formation of complexes associated with painful experiences in the past.

In psycho-therapy, when we have established an atmosphere of cure, negativism signifies an unconscious resistance to recovery, and when exaggerated, it must make us suspect dementia praecox, or malingering if it seems likely that the resistance is a conscious one.

(*c*) *Neutral Response.* It is hard to draw the line between the lower forms of volition and suggestion. If the idea is incompatible with the social ideal self and the social ideal self is strong, then the process is one of volition and we get a neutral response: this is what happens in those who hold strong principles and ideals.

If the social ideal is too weak, and the affective forces aroused are strong, the idea is accepted and the process is one of suggestion.

If the idea has not made sufficient impression, we get a neutral response, and this is due to the fact that there are no pre-formed interests or complexes to which it can attach itself: in this case the suggestion is ignored. This is seen most markedly in certain imbeciles. The suggestion may need repetition to give it the necessary amount of prestige for acceptance, a fact which is taken into consideration as a basis for all advertisements. The suggestion may be incompatible with such interests and complexes as exist, and the forces involved simply neutralise each other, which is one of the reasons for the fact that the suggestions of the younger generation are not easily accepted by the elder.

A neutral response may also be the result of a conflict of motives due to the incompatibility of the interests aroused, with the production of a state of doubt, which is seen in an exaggerated form in cases of anxiety hysteria: in such cases the suggestion may be accepted after deliberation either to relieve tension, or when a decision is brought about by the reinforcement of one side of the conflict by further affective forces; or the conflict may be forgotten and only at some later period will one side of the conflict materialise by the stimulation of affective forces which harmonize with it. A neutral response will also occur in cases of 'dementia.'

§ 4. Abnormal Suggestion.

The process in abnormal suggestion is the same as in normal suggestion, and depends on the factors already discussed, which increase the various states of suggestibility. The difference is only one of degree, and in abnormal suggestion the affective forces are stronger, and the 'contrary forces' are weaker, so that the person is less or not at all aware of their action and has little or no control over them. This is best seen in persons suffering from hysteria; when their affective states are dominated by the desire to escape from some irksome duty or future danger, to astonish, or to attract attention or sympathy, any idea harmonizing with this state becomes reinforced and realises itself.

We also know that hysterical patients can temporarily put aside their hysteria by a change in their affective state when the situation requires particular concentration on some interesting function or amusement. I think that the disappearance of hysterical symptoms as if by magic in the presence of danger and the sudden recoveries reported in the newspapers are explained by the fact that one affective state is substituted for another by the re-direction of attention under the influence of surprise. I regard hypnosis, in accordance with Bernheim's view, as an exaggerated form of suggestion. I cannot accept Dr Rivers's statement in *War Neurosis and Military Training* that " In the hypnotic state the individual responds immediately and without question or hesitation, not merely to the command of his hypnotizer, but even to a desire or impulse of the hypnotizer's mind which is not expressed by speech or obvious gesture[1]."

It implies the presence of some mysterious force, an idea which we are only just beginning to uproot from the popular mind; my own experience and I think that of all other observers of recent times are entirely opposed to it.

The explanation of suggestion I have given seems to make it incorrect to classify suggestion as an innate tendency. We have seen that there is no one single state which we can call suggestibility, but that there are several states of suggestibility, and that these are induced by the stimulation of different instinctive tendencies, sentiments, interests, and complexes.

We have seen that a suggestion is accepted because it harmonizes with these states, and that the affective forces aroused give it the necessary reinforcing power to realise itself, and that it is these affective forces which produce conviction. We have now to consider briefly how we can apply this conception to suggestion as a method of treatment.

§ 5. SUGGESTION AS A METHOD OF TREATMENT.

The object to be attained in treatment by suggestion is to produce a condition of mind in the patient which will set in action the right affective forces for the induction of those states of suggestibility, which

[1] *Op. cit.* 529.

will harmonize with and reinforce the ideas to be suggested and so get them accepted with conviction.

The first step in the process is for the physician to induce a state of personal suggestibility in the patient, and this he does by arousing in him the necessary instinctive tendencies, showing him sympathy and impressing him with a knowledge of and interest in his condition, so that the patient has respect for and confidence in the physician. In neurasthenic hospitals, where an atmosphere of cure is present, this state is induced in him on admission by the patients, who have been already relieved of their symptoms, for success is the greatest creator of prestige.

The physician then proceeds according to the type of patient he has to deal with and to the amount of personal suggestibility already induced; if the states of individual and conditional suggestibility are exaggerated then it will not matter what form the suggestion takes; but if there is only specific suggestibility then it is necessary, if possible, to find out its nature by therapeutic conversations and superficial analysis and so to arrange the suggestions as to be compatible with it.

In one form of abnormal suggestion advantage is taken of the effects of some emotional reaction to alter the affective state. An emotion frequently used is that of surprise, which "tends to free the mind from what before occupied it, and to increase the intensity of every emotion with which it blends, or by which it is rapidly followed[1]." If the affective state be that dominated by fear it may be counter-balanced by inducing a state of anger for "the emotions of fear and anger tend to exclude one another from simultaneous activity[2]." I think also that painful electricity, the use of which has done considerable harm, and isolation, depend largely for their results on the change of affective states induced by them.

Other methods depend on the fact that the suggestion will not be accepted with conviction unless it is associated with some specific treatment to account for the cure. This form of suggestion is 'indirect suggestion' and is often used quite unconsciously by the medical profession. It is used either by insinuation with the help of massage, drugs, electricity, or by deliberate deception, for example, the use of water without morphia as an injection, and the use of bread pills for functional vomiting. Hypnosis is in most cases unnecessary, and al-

[1] Shand, *Foundations of Character*, 1914, 422.

[2] Shand, *op. cit.* 260.

though at one time I used it very extensively, I now only use it to clear up an extensive amnesia.

Of the methods of 'suggestion treatment' there is no question that the method of normal suggestion by explanation and appeals to feelings is the best as the patient then realises that he himself is responsible for the removal of symptoms and he will know what to do in case of a relapse. In the other methods he relies entirely on the physician, does not understand his condition, and is much more likely to relapse.

But in any case it must be noted that the patient relies on the explanation given to him, and that the real cause of the condition may never have been discovered. Treatment by suggestion does not therefore conform to our ideal method of treatment, and is not here advocated as such, but it is very useful in practice as a method of removing symptoms in certain cases.

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THE SINGLE GENERAL FACTOR IN DISSIMILAR MENTAL MEASUREMENTS.

By J. C. MAXWELL GARNETT.

NOTE: *All the variables, whether dependent or independent, referred to below are distributed according to the normal law with the same standard deviation.*

I.

The following paper is concerned with the relations between n correlated variables each of which is distributed according to the normal law, $y = e^{\frac{-x^2}{2\sigma^2}}$, with the same standard deviation, σ; and with the expression of such variables in terms of n or more independent variables each of which is distributed according to the normal law with the same standard deviation (σ) as before. The investigation confirms the result of a previous inquiry[1], namely that whenever the Bravais-Pearson coefficients of correlation between n variables satisfy the conditions for

[1] Garnett, *Proc. Roy. Soc.* A, XCVI. 1919, 91 *et seq.*

a hierarchy[1]—a condition which tends to be fulfilled by the $\frac{1}{2}n(n-1)$ correlations between pairs of any set of n sufficiently dissimilar mental tests[2]—the n variables in question can be expressed in terms of $n+1$ independent variables of which one is a single general factor, while the remaining n independent variables are entirely specific. In particular, it is shown that when the scores obtained in such dice-throwing tests as those described by Dr G. H. Thomson[3] are measured in new units such that their measures, already distributed according to the normal law, have the same standard deviation, these measures are variables which, when the number of dice is infinite, satisfy the conditions for a hierarchy, and when the number of dice is very large approximately do so; and it is further shown that these variables, although as Dr Thomson has pointed out they appear at first sight to possess no general factor but only group factors, may be expressed in terms of a single general factor with specific factors only.

II.

It is easy to show that n correlated variables $q_1, q_2, \ldots, q_n$ can always be expressed as linear functions of n independent variables $x_1, x_2, \ldots, x_n$ by means of equations of the form

$$q_s = {}_sl_1 \cdot x_1 + {}_sl_2 \cdot x_2 + \ldots + {}_sl_n \cdot x_n \qquad (1)$$

where the l's are numerical coefficients such that

$${}_sl_1^2 + {}_sl_2^2 + \ldots + {}_sl_n^2 = 1. \qquad (2)$$

Thus it has already been shown[4] that, if the whole number N of independent variables involved in all the q's be $y_1, y_2, \ldots, y_N$, the q's may be expressed in terms of the y's by means of the equations of the form

$$q_s = {}_sm_1 \cdot y_1 + {}_sm_2 \cdot y_2 + \ldots + {}_sm_N \cdot y_N \qquad (3)$$

where the m's are numerical coefficients such that

$${}_sm_1^2 + {}_sm_2^2 + \ldots + {}_sm^2_N = 1. \qquad (4)$$

Now choose new independent variables $x_1, x_2, \ldots, x_n$ given by equations of the type

$$x_s = {}_s\mu_1 \cdot y_1 + {}_s\mu_2 \cdot y_2 + \ldots + {}_s\mu_N \cdot y_N \quad [s = 1, 2, \ldots, n] \qquad (5)$$

where

$${}_s\mu_1^2 + {}_s\mu_2^2 + \ldots + {}_s\mu^2_N = 1 \qquad (6)$$

[1] Namely that, if r_{ab} denote the correlation between the ath and the bth of the n variables, $\frac{r_{as}}{r_{at}} = \frac{r_{bs}}{r_{bt}}$ where a, b, s, t have any different values from 1 to n inclusive. See footnote 2 on p. 244.

[2] Garnett, *loc. cit.* 105.

[3] This *Journal*, VIII. 1916, 271 *et seq.*

[4] Garnett, *loc. cit.* equation (8), 94. Cf. also Bravais, *Mémoires de l'Inst. de France*, IX. 1846, 260 *et seq.*

and where, since the x's are independent,

$$ {}_s\mu_1 \cdot {}_t\mu_1 + {}_s\mu_2 \cdot {}_t\mu_2 + \ldots + {}_s\mu_N \cdot {}_t\mu_N = 0 \quad [s, t = 1, 2, \ldots, n]; \ \ldots(7) $$

for the expression on the left-hand side of this equation is the coefficient of correlation[1] between x_s and x_t, and this is zero since x_s is independent of x_t. In order that the n q's may be completely expressible in terms of the n x's we must be able to write

$$ q_s = {}_sl_1 \cdot x_1 + {}_sl_2 \cdot x_2 + \ldots + {}_sl_n \cdot x_n \quad [s = 1, 2, \ldots, n] \ \ldots\ldots(1) $$

where, as before, $\quad {}_sl_1{}^2 + {}_sl_2{}^2 + \ldots + {}_sl_n{}^2 = 1. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots(2)$

If now we substitute in equations (1) the expressions for the x's given in equations (5) and equate the coefficients of the respective y's to the coefficients of the corresponding y's in equations (3) we obtain

$$ \left.\begin{array}{l} {}_sl_1 \cdot {}_1\mu_1 + {}_sl_2 \cdot {}_2\mu_1 + \ldots + {}_sl_n \cdot {}_n\mu_1 \ = {}_sm_1 \\ {}_sl_1 \cdot {}_1\mu_2 + {}_sl_2 \cdot {}_2\mu_2 + \ldots + {}_sl_n \cdot {}_n\mu_2 \ = {}_sm_2 \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ {}_sl_1 \cdot {}_1\mu_N + {}_sl_2 \cdot {}_2\mu_N + \ldots + {}_sl_n \cdot {}_n\mu_N = {}_sm_N \end{array}\right\} [s = 1, 2, \ldots, n] \ldots(8) $$

If we square and add equation (8) and use equations (2) and (6) we obtain equation (4). In order to determine the nN μ's and the n^2 l's in terms of the given m's we therefore have the nN equations (8), the $\frac{1}{2}n(n-1)$ equations (7), the n equations (2), and the n equations (6) of which, as we saw, n are not independent. So we have $nN + \frac{1}{2}n(n+1)$ independent relations between the $nN + n^2$ undetermined μ's and l's. If by means of equations (8) the μ's be first determined in terms of the l's we then have $\frac{1}{2}n(n+1)$ equations from which to determine the n^2 l's, which can therefore be determined with $n^2 - \frac{1}{2}n(n+1) = \frac{1}{2}n(n-1)$ degrees of freedom: the number of degrees of freedom with which n mutually perpendicular directions can be fixed in n-dimensional space.

We conclude that any n correlated variables can always be expressed in terms of n independent variables, which may be selected with $\frac{1}{2}n(n-1)$ degrees of freedom.

III.

Now suppose that the correlations between the n correlated variables $q_1, q_2, \ldots, q_n$ satisfy the conditions[2] for a hierarchy: namely that

$$ \frac{r_{as}}{r_{at}} = \frac{r_{bs}}{r_{bt}} \quad [a, b, s, t = 1, 2, \ldots, n] \ \ldots\ldots\ldots\ldots\ldots\ldots(9) $$

[1] Garnett, *loc. cit.* 96, equation (16).

[2] The following equation (9) that expresses these conditions was given by Mr Cyril Burt in this *Journal*, III. 1909–10, 159; and Professor Spearman and Dr Hart in this *Journal*, V. 65, refer to it as 'Burt's equation.' In two previous papers by the present writer—one in this *Journal* and one in the *Proc. Roy. Soc.*—the conditions in question

where r_{as} denotes the correlation between q_a and q_s, and where a, b, s, t have any four different values from 1 to n inclusive. It has been shown[1] that, when these conditions are satisfied, the n q's may be expressed in terms of $n+1$ independent x's $(x_1, x_2, \ldots, x_n, x_g)$ by means of the equations

$$q_s = r_{sg} \cdot x_g + \sqrt{(1 - r^2_{sg})} \cdot x_s \quad [s = 1, 2, \ldots, n] \quad \ldots\ldots\ldots(10)$$

where r_{sg} is the coefficient of correlation between q_s and x_g, x_g is a *single general factor* occurring in each of the n equations (10), and x_s is a specific factor occurring in the sth of these equations only. There are no group factors.

In the preceding paragraph and in what follows the term 'single general factor' will be used to denote only that one (x_g) of $n+1$ independent x's which, when n correlated q's whose co relations satisfy equation (9) are expressed in terms of these x's, enters according to equations (10) into each of the q's, while the remaining n independent factors are specific, each entering into a different q.[2]

If now we would know how many of the conditions for a hierarchy expressed in equation (9) are independent, we observe that these conditions led to equations (10) from which it follow by means of the cosine law of correlation[3] that

$$r_{st} = r_{sg}\, r_{tg} \quad [s, t = 1, 2, \ldots, n]. \quad \ldots\ldots\ldots\ldots\ldots\ldots(11)$$

There are $\frac{1}{2}n(n-1)$ of these equations. We may solve n of them for $r_{sg}, r_{tg}, r_{ug}, \ldots$, the coefficients of x_g in equations (11). If then we substitute these solutions in equations (11) we have

$$\tfrac{1}{2}n(n-1) - n = \tfrac{1}{2}n(n-3)$$

remaining independent relations between the correlations of the q's. The independent conditions for a hierarchy are therefore $\frac{1}{2}n(n-3)$ in number[4].

were described as 'Mr Burt's conditions' in order to distinguish them from certain other conditions that were described as Professor Spearman's 'correlation between columns' conditions, or briefly as 'Professor Spearman's conditions.'

But equation (9) which expresses the conditions for a hierarchy was, as Mr Burt acknowledged, immediately deducible from an equation (equation (f), *Ztsch. f. Psychol.* XLIV. 85) previously given by Krüger and Spearman in 1906, and was already foreshadowed by Professor Spearman in the *Amer. Journ. of Psychol.* XV. 1904, 274, 275. In these circumstances we shall not continue to describe by Mr Burt's name the conditions expressed by equation (9): they will instead be referred to below as 'the conditions for a hierarchy.'

[1] Garnett, *loc. cit.* 102, equation (35).

[2] Such a definition of a single general factor has already been given (Garnett, this *Journal*, IX. 347) where also the need for such a definition has been pointed out.

[3] Garnett, *loc. cit.* 96, equation (16).

[4] This result was given without proof (Garnett) in this *Journal*, IX. 1919, 347.

IV.

We have said that whenever the coefficients of correlation of n variables $q_1, q_2, \ldots, q_n$ satisfy the conditions for a hierarchy expressed in equation (9), these q's can be expressed by means of equations (10) in terms of $n+1$ independent x's of which one is a single general factor and all the rest are specific factors. We proceed to prove that, as Dr Thomson has found to be the case in his dice-throwing experiments, by making use of a sufficiently large number of independent variables $(x_1, x_2, \ldots, x_N)$ in terms of which to express the same n q's whose correlations form a hierarchy, we can so arrange that the number of x's having finite coefficients in the expressions[1]

$$q_s = {}_sl_1 \cdot x_1 + {}_sl_2 \cdot x_2 + \ldots + {}_sl_N \cdot x_N \quad [s = 1, 2, \ldots, n]$$

for each of the q's and therefore capable of being called general factors (although not of course *single* general factors) shall be very small compared with the number of x's that enter as group factors or specific factors into the expressions for the q's. But we shall go on to show that this result is not inconsistent with the conclusion expressed in the first sentence of this paragraph; for we shall prove that a linear transformation of independent variables will result in the q's being expressed according to equations (10) in terms of one single general factor and n specific factors, without group factors.

Let us then express the q's in terms of a very large number, N, of independent x's by means of the equations

$$q_s = \frac{1}{\sqrt{Nc_s}} (\text{sum of } Nc_s \text{ whole } x\text{'s}) \quad [s = 1, 2, \ldots, n] \ldots\ldots(12)$$

in which c_s is a positive proper fraction that represents the proportion of the whole number N of x's that enters into the expression for q_s. Let us further arrange that the number, Nc_s, of whole x's in the expression given by equation (13) for q_1 is made up of

$NP\dfrac{c_1}{1-c_1}$	specific to q_1.
$+\,NP\dfrac{c_1}{1-c_1}\left\{\displaystyle\sum_{s=2}^{s=n}\frac{c_s}{1-c_s}\right\}$	in which the sth term represents the number of x's common to q_1 and q_s only.
$+\,NP\dfrac{c_1}{1-c_1}\left\{\displaystyle\sum_{s,t=2}^{s,t=n}\frac{c_s}{1-c_s}\cdot\frac{c_t}{1-c_t}\right\}$	in which the typical term represents the number of x's common to q_1, q_s, and q_t only.

[1] It has been proved that each q will be a linear function of the x's (Garnett, *loc. cit.* 95).

$$+ NP \frac{c_1}{1-c_1} \left\{ \sum_{s,t,u=2}^{s,t,u=n} \frac{c_s}{1-c_s} \cdot \frac{c_t}{1-c_t} \cdot \frac{c_u}{1-c_u} \right\}$$ in which the typical term represents the number of x's common to q_1, q_s, q_t, and q_u only.

$+ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$

$$+ NP \frac{c_1}{1-c_1} \left\{ \frac{c_2}{1-c_2} \cdot \frac{c_3}{1-c_3} \cdots \frac{c_n}{1-c_n} \right\}$$ which term represents the number of x's common to all the q's, $q_1, q_2, \ldots, q_n$. ....(13)

where $$P \equiv (1-c_1)(1-c_2) \ldots (1-c_n). \quad \ldots\ldots\ldots\ldots(14)$$

By adding the numbers in the successive lines of this expression (13) for the whole number of x's involved in q_1 it will be seen that they amount to Nc_1, for their sum is

$$NP \frac{c_1}{1-c_1} \left\{ 1 + \sum_2^n \frac{c_s}{1-c_s} + \sum_2^n \frac{c_s c_t}{(1-c_s)(1-c_t)} + \ldots + \frac{c_2 c_3 \ldots c_n}{(1-c_2)(1-c_3)\ldots(1-c_n)} \right\}$$

$$= NP \frac{c_1}{1-c_1} \left(1 + \frac{c_2}{1-c_2}\right)\left(1 + \frac{c_3}{1-c}\right) \ldots \left(1 + \frac{c_n}{1-c_n}\right) = Nc_1.$$

Moreover the number of x's common to q_1 and q_2 is made up of the number common to q_1 and q_2 only together with those which enter into q_1 and q_2 as well as into other q's. The number in question therefore is

$$NP \frac{c_1}{1-c_1} \cdot \frac{c_2}{1-c_2} \left\{ 1 + \sum_3^n \frac{c_s}{1-c_s} + \sum_3^n \frac{c_s c_t}{(1-c_s)(1-c_t)} + \ldots + \frac{c_3 c_4 \ldots c_n}{(1-c_3)(1-c_4)\ldots(1-c_n)} \right\}$$

$$= NP \frac{c_1}{1-c_1} \cdot \frac{c_2}{1-c_2} \left(1 + \frac{c_3}{1-c_3}\right)\left(1 + \frac{c_4}{1-c_4}\right) \ldots \left(1 + \frac{c_n}{1-c_n}\right)$$

$$= Nc_1 c_2.$$

It follows from the cosine law[1] that the coefficient of correlation between two q's, say q_s and q_t, given by equations (12) is

$$r_{st} = \frac{1}{\sqrt{Nc_s}} \cdot \frac{1}{\sqrt{Nc_t}} \cdot Nc_s c_t = \sqrt{c_s c_t}. \quad \ldots\ldots\ldots\ldots(15)$$

We therefore have

$$\frac{r_{as}}{r_{at}} = \sqrt{\frac{c_a c_s}{c_a c_t}} = \sqrt{\frac{c_s}{c_t}} = \frac{r_{bs}}{r_{bt}}$$

so that the q's given by equations (12) the right-hand sides of which are in the form (13) have correlations that satisfy the conditions for a hierarchy. And yet inspection of the expression (13) shows that the

[1] Garnett, *loc cit.* 96, equation (16).

number of general factors (not *single* general factors) is $Nc_1 c_2 \ldots c_n$ and is therefore smaller than the number of group factors common to any two, three, four or more up to $n-1$ of the q's, and therefore very much smaller than the whole number of these group factors; for the number of group factors common to $q_s, q_t, \ldots, q_w$ is $Nc_s c_t \ldots c_w$, and each c is a proper fraction.

We may now give rules for writing down the successive expressions for $q_1, q_2, \ldots, q_n$ according to equations (12) supposing that

$$r_{1g} \geqslant r_{2g} \geqslant r_{3g} \geqslant \ldots \geqslant r_{ng},$$

so that

$$c_1 \geqslant c_2 \geqslant c_3 \geqslant \ldots \geqslant c_n.$$

RULE:—*First write down as the expression for* q_1

$$q_1 = \frac{1}{\sqrt{Nc_1}}(x_1 + x_2 + \ldots + x_{Nc_1})$$

involving only the first Nc_1 *x's; then write down a similar expression for* q_2 *involving a proportion* c_2 *of the* Nc_1 *x's in* q_1 *and the same proportion of the* $N(1-c_1)$ *x's not in* q_1*; then write down the corresponding expression for* q_3 *involving a proportion* c_3*, first of the* Nc_1c_2 *x's common to* q_1 *and* q_2*, then of the* $Nc_1(1-c_2)$ *x's in* q_1 *but not in* q_2*, then of the* $Nc_2(1-c_1)$ *x's not in* c_1 *but in* c_2*, and then of the* $N(1-c_1)(1-c_2)$ *remaining x's; and so on.*

It is perhaps worth observing that the whole number of x's entering into the expressions for $q_1, q_2, \ldots, q_n$ is $N(1-P)$, where

$$P = (1-c_1)(1-c_2)\ldots(1-c_n).$$

The x's in expression (13) are distributed among specific factors, 2-group factors, 3-group factors, and so on in the same proportions as among new variables that would be respectively obtained by adding together the sums of $Nc_1, Nc_2, \ldots, Nc_n$ x's selected at random from N given x's a very large number, T, of times, the sums of the x's taken in each trial being added together so as to form the new variables. For example, the number of specific factors in q_1 according to expression (13) is

$$Nc_1(1-c_2)(1-c_3)\ldots(1-c_n),$$

while the number of x's that will occur in the first of the new variables but not in any other in a large number T of trials is

$$NTc_1(1-c_2)(1-c_3)\ldots(1-c_n);$$

since

$$c_1(1-c_2)(1-c_3)\ldots(1-c_n)$$

is the probability that any one x will occur in the first and will not occur in the second or third or ... or nth new variable.

In order that it may be possible exactly to follow the above italicised Rule for writing down expressions for q's having correlations that satisfy the conditions for a hierarchy, it is necessary and sufficient that Nc_1, Nc_1c_2, $Nc_1c_2c_3$, ..., $Nc_1c_2...c_n$ should all be integers. And, if only N be chosen large enough, it will be possible to find integers whose ratios to Nc_1, Nc_1c_2, ..., $Nc_1c_2...c_n$ approach unity within any desired degree of approximation. By choosing N large enough it will therefore be possible to write down, according to our Rule, expressions for the successive q's that will make their correlations approach within any desired limits correlations that satisfy the conditions for a hierarchy.

We have therefore proved our proposition that it is possible, by employing a very large number of x's, to express n q's whose correlations satisfy the conditions for a hierarchy so that the number of x's that enter as general factors (not *single* general factors) will be very small indeed compared with the number of group factors and specific factors; but the number of x's employed may have to be infinite and the x's consequently infinitesimal. We have seen too that the distribution of the x's among the q's defined by our italicised Rule is the same as that which would be obtained by taking the average of a very large number of trial distributions so that the same proportion c_1, c_2, ..., c_n of the whole number of x's always enters into q_1, q_2, ..., q_n respectively.

It follows that, by employing a very large but not an infinite number N of x's for expressing, as nearly as possible according to our italicised Rule and according to equations (12), a much smaller number n of q's, it will be possible to give to the q's correlations which are nearly *but not quite* equal to correlations that satisfy the conditions for a hierarchy. Moreover the number of x's that enter as general factors will, as before, be very small compared with the number of group factors or specific factors; and may indeed be zero.

For example Dr Thomson[1], by choosing $N = 145$ independent x's by means of which to express $n = 10$ correlated q's has, without employing general factors, obtained expressions for the q's which make their correlations approximately satisfy the conditions for a hierarchy. The distribution of Dr Thomson's 145 independent x's is shown in the following table in which the columns given are rearranged from the table published by Dr Thomson[2] so as to show how nearly the x's of successive q's are chosen according to our italicised Rule (which, as we have just seen, would make the correlations of the q's satisfy the conditions for a hierarchy if only a sufficient number of independent factors had been employed). The first 36 columns of the table show the arrange-

[1] This *Journal*, VIII. 1916, 271 *et seq.*

[2] *Ibid.* 277.

ment of group factors, and the last column shows the number of specific factors in each of the ten q's, $q_a, q_b, \ldots, q_l$. The figures above the first 36 columns mark the order of the corresponding columns in Dr Thomson's table. Dr Thomson tells us that the average difference between the correlations of the q's in this table and a set of correlations that satisfy the conditions for a hierarchy is only 0·016. It may, however, be shown that the chance of obtaining so close an approximation to the satisfaction of the conditions for a hierarchy in a random distribution of group factors

	32	33	34	35	36	1	2	7	10	4	14	11	5	3	8	12	15	13	9	26	28	29	30	23	6	24	17	21	20	25	16	27	31	18	19	22	s
q_a	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	–	–	–	–	–	–	–	–	–	–	–	–	0
q_b	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	–	–	–	–	–	×	–	–	–	–	–	–	–	–	–	–	–	0
q_c	×	×	×	×	×	×	×	×	×	×	×	×	–	–	–	–	–	–	–	×	×	×	×	–	×	×	–	–	–	×	–	–	–	–	–	–	1
q_d	×	×	×	×	×	×	–	–	–	–	–	–	×	×	×	×	×	–	–	×	×	–	–	–	–	×	–	–	–	×	×	×	–	–	–	–	3
q_e	×	×	×	–	–	–	×	–	–	–	–	–	×	–	–	–	–	–	×	–	–	×	–	–	–	×	×	–	–	–	–	–	×	×	–	–	9
q_f	–	–	–	–	–	–	–	×	×	–	–	–	–	×	–	–	–	–	–	–	–	×	–	–	–	–	×	–	×	–	–	×	–	–	×	×	14
q_g	–	–	–	–	–	–	–	–	–	×	–	–	–	–	×	–	–	×	–	×	–	–	–	–	–	–	–	×	–	–	–	–	×	–	×	–	16
q_h	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	×	–	–	–	–	×	–	–	×	×	–	×	×	–	–	–	–	–	–	–	–	20
q_k	–	–	–	–	–	–	–	–	–	–	×	–	–	–	–	–	–	×	×	–	–	–	×	–	–	–	–	×	×	–	×	–	–	–	–	–	22
q_l	–	–	–	–	–	–	–	–	–	–	–	×	–	–	–	–	×	–	–	–	–	–	–	×	–	–	–	–	×	–	–	–	–	×	–	×	24

is less than one in 10^{11} or less than one in one hundred thousand million. But in view of the fact that, as we saw above (p. 248), the average of a large number of random distributions of the x's in given proportions among the q's is a distribution that satisfies the conditions for a hierarchy, we shall not be surprised if any particular chance distribution of a very large number of x's among a much smaller number of q's yields correlations between the q's that are not very far from satisfying these conditions.

V.

Since the n q's expressed by means of equations (12) according to our italicised Rule in terms of a very large number N of independent x's have correlations that satisfy the conditions for a hierarchy, it must also be possible to express these n q's in terms of $n+1$ independent variables of which one is a single general factor while the remainder are specific factors.

Let us define $n+1$ new variables $g, \xi_1, \xi_2, \ldots, \xi_n$ by means of

$$g = \frac{1}{\sqrt{N}}(x_1 + x_2 + \ldots + x_N) \quad \text{(16)}$$

and

$$\xi_s = \sqrt{\frac{1-c_s}{Nc_s}} \,.\, (\text{sum of the } Nc_s \ \ x\text{'s in } q_s) - \sqrt{\frac{c_s}{N(1-c_s)}} \,.\, (\text{sum of the } N(1-c_s) \text{ remaining } x\text{'s}) \, [s = 1, 2, \ldots, n]. \quad \text{(17)}$$

It is easy to see that these $n+1$ new variables are independent of each other; for the cosine law[1] gives as the correlation of g and ξ_s

$$r_{g\xi_s} = \frac{1}{N}\left(\sqrt{c_s(1-c_s)} - \sqrt{c_s(1-c_s)}\right) = 0$$

and it is equally easy to verify that the same cosine law gives as the correlation between any two ξ's, say ξ_s and ξ_t,

$$r_{\xi_s\xi_t} = 0 \quad [s, t = 1, 2, \dots, n].$$

Moreover, employing the expressions for g and ξ_s given by equation (16) and (17), we obtain

$$\begin{aligned}\sqrt{c_s}\,.\,g + \sqrt{1-c_s}\,.\,\xi_s &= \sqrt{\frac{c_s}{N}}\Big\{(\text{sum of all the } x\text{'s}) \\ &\quad + \frac{1-c_s}{c_s}(\text{sum of the } Nc_s \quad x\text{'s in } q_s) \\ &\quad - (\text{sum of the } N(1-c_s) \quad x\text{'s not in } q_s)\Big\} \\ &= \frac{1}{\sqrt{Nc_s}}(\text{sum of the } Nc_s \quad x\text{'s in } q_s) \\ &= q_s \qquad [s = 1, 2, \dots, n]. \qquad \text{............(18)}\end{aligned}$$

Thus we have proved that the n q's which are expressed by means of equations (12) according to our italicised Rule in terms of a very large number N of independent x's may, by means of the linear transformation of variables given in equations (16) and (17), also be expressed by means of equations (18) in terms of $n+1$ independent variables g, ξ_1, ξ_2, $\dots$, ξ_n of which one, g, is a single general factor while all the others (ξ's) are specific factors.

Comparing equations (18) with equations (10) and making use of equation (15) we have

$$r^2_{sg} = c_s = \sqrt{\frac{c_s c_t \,.\, c_s c_u}{c_t\, c_u}} = \frac{r_{st} r_{su}}{r_{tu}} \qquad \text{..................(19)}$$

giving r_{sg} and c_s in terms of the correlations of the q's.

We have thus verified that while n q's, expressed by means of equations (12) in terms of an infinite number of infinitesimal independent variables (x's) according to the Rule given on p. 248, have correlations that satisfy the conditions for a hierarchy, these same n q's may alternatively be expressed in terms of $n+1$ independent variables of which one is a single general factor while the other n are specific factors. We have moreover given a linear transformation of the independent vari-

[1] Garnett, *loc. cit.* 96, equation (16).

ables which enables us to replace the large number N of x's employed in the first form of expression by the $n+1$ independent variables employed in the second form. If we would explain in non-mathematical terms why these two forms of statement are equivalent to one another we may point out that, as equation (16) indicates, the very large number of x's, from among which given numbers occur according to the 'all or none' law of equation (12) in each of n correlated q's, may be regarded as interchangeable constituent elements of the single general factor, g. Thus, in equation (16), g is not altered by any increase of x_s and an equal decrease of x_t: any quantity may be taken from any one x and added to any other one or more x's without altering the general factor. It is true that each of these x's enters into the q's in equation (12) according to the 'all or none' law. But as we have seen the satisfaction of the conditions for a hierarchy within any desired degree of approximation, by means of integral numbers of x's employed according to the Rule on p. 248, requires so large a total number N of x's and proportionately large numbers (Nc_1, Nc_2, ..., Nc_n) of x's in each q (q_1, q_2, ..., q_n) that the significance of this 'all or none' law disappears when those conditions are satisfied; for any given fraction of the whole number of infinitesimal x's may then enter into any one of the q's.

VI.

It is important to observe that the single general factor and each of the specific factors among $n+1$ independent variables employed in equations (10) and (18) for the expression of n given q's, whose correlations satisfy the conditions for a hierarchy, is unique. For suppose that the q's are originally expressed in terms of N independent variables x_1, x_2, ..., x_N where $N \geqslant n$, by means of equations

$$q_s = {}_sm_1 \cdot x_1 + {}_sm_2 \cdot x_2 + \ldots + {}_sm_N \cdot x_N \quad [s = 1, 2, \ldots, n] \ldots(20)$$

where
$${}_sm_1{}^2 + {}_sm_2{}^2 + \ldots + {}_sm^2{}_N = 1. \quad \ldots\ldots\ldots\ldots\ldots\ldots(21)$$

Since by hypothesis the $\frac{1}{2}n(n-1)$ correlations of the q's satisfy the conditions for a hierarchy we may write

$$q_s = r_{sg} \cdot g + \sqrt{1 - r^2{}_{sg}} \cdot \xi_s \quad [s = 1, 2, \ldots, n] \ldots\ldots\ldots\ldots(22)$$

where g, ξ_1, ξ_2, ..., ξ_n are $n+1$ independent variables and where r_{sg} is the correlation between q_s and g. We note that $r_{sg} = \sqrt{\dfrac{r_{st}r_{su}}{r_{tu}}}$ and is therefore uniquely determined, apart from sign, in terms of the given correlations of the q's. In order to obtain expressions for the $n+1$ new independent variables in terms of the original x's we may write

$$g = l_1 x_1 + l_2 x_2 + \dots + l_N x_N \quad \dots\dots\dots\dots\dots\dots(23)$$

where
$$l_1^2 + l_2^2 + \dots + l^2_N = 1 \quad \dots\dots\dots\dots\dots\dots\dots\dots(24)$$

and
$$\xi_s = {}_s\lambda_1 \cdot x_1 + {}_s\lambda_2 \cdot x_2 + \dots + {}_s\lambda_N \cdot x_N \quad [s = 1, 2, \dots, n] \dots\dots(25)$$

where
$${}_s\lambda_1^2 + {}_s\lambda_2^2 + \dots + {}_s\lambda^2_N = 1 \quad [s = 1, 2, \dots, n]. \dots\dots\dots\dots(26)$$

If now we substitute in equations (22) the values of q_s, g and ξ_s given by equations (20), (23) and (25), and if we then equate the coefficients of x_p on both sides of the equations, we have

$${}_s m_p = r_{sg} \cdot l_p + \sqrt{1 - r^2_{sg}} \cdot {}_s\lambda_p \quad [s = 1, 2, \dots, n;\ p = 1, 2, \dots, N], \dots(27)$$

a result that might have been obtained directly from the cosine law by observing that ${}_s m_p$ is the correlation between q_s and x_p. Moreover, since $\xi_1, \xi_2, \dots, \xi_n$ are independent of g and of each other, we have

$$l_1 \cdot {}_s\lambda_1 + l_2 \cdot {}_s\lambda_2 + \dots + l_N \cdot {}_s\lambda_N = 0 \quad [s = 1, 2, \dots, n] \dots\dots(28)$$

and
$${}_s\lambda_1 \cdot {}_t\lambda_1 + {}_s\lambda_2 \cdot {}_t\lambda_2 + \dots + {}_s\lambda_N \cdot {}_t\lambda_N = 0 \quad [s, t = 1, 2, \dots, n] \dots(29)$$

Substituting for the λ's from equations (27) in equations (28) we obtain

$$l_1 \cdot {}_s m_1 + l_2 \cdot {}_s m_2 + \dots + l_N \cdot {}_s m_N = \sqrt{r_{sg}} \quad [s = 1, 2, \dots, n]. \dots(30)$$

Including equation (24) we therefore have $n+1$ equations from which to determine the N l's in terms of the given m's; for equations (29) may be deduced from equations (27) and (30). It follows that all the N l's may be determined in terms of any $n+1$ of them. If therefore $N = n+1$ the l's are uniquely determined in terms of the given m's and so also therefore are the λ's. Employing a geometrical metaphor we may express this result by saying that in a space of $n+1$ dimensions g and the ξ's are uniquely determined.

VII.

Professor Spearman and Dr Hart have shown[1] that the correlations of any set of sufficiently varied mental tests that has yet been carried out approximately satisfy the following 'correlation between columns' conditions: namely that, if the variables $q_1, q_2, \dots, q_n$ measure the n tests in question; and if r_{st} is the coefficient of correlation between q_s

[1] This *Journal*, v. 1912–13, 51.

and q_t; and if the $\frac{1}{2}n(n-1)$ r's are arranged in the form of a correlation table, thus

$$\begin{array}{ccccc} - & r_{12} & r_{13} & \cdots & r_{1n} \\ r_{12} & - & r_{23} & \cdots & r_{2n} \\ r_{13} & r_{23} & - & \cdots & r_{3n} \\ \vdots & \vdots & \vdots & \cdots & \vdots \\ r_{1n} & r_{2n} & r_{3n} & \cdots & - \end{array};$$

then the correlation between every pair of columns is ± 1. An examination of the mathematical consequences of the fulfilment of the 'correlation between columns' conditions shows that, whenever these conditions are completely satisfied by a set of n *sufficiently dissimilar* mental tests, the correlations (r's) will form a hierarchy so that the n q's must be expressible by means of equations (10) or (18) in terms of $n+1$ independent variables of which one is a single general factor while the remainder are specific factors[1]. Thus it follows from Professor Spearman's and Dr Hart's results that the correlations of any n sufficiently dissimilar mental tests approximately satisfy the conditions for a hierarchy; so that the measures (q's) of any n such tests can be approximately expressed by means of equations (10) or (18) in terms of $n+1$ independent factors of which one is a single general factor while the remainder are specific factors. We may therefore say that, in any set of sufficiently dissimilar mental tests that has hitherto been carried out, a single general factor together with specific factors are dominant, while group factors, although they may occur, are of minor significance; or, in other words, that the qualities tested in any such set of tests *tend* to be made up of a single general factor and specific factors only.

It is true that n q's whose correlations satisfy the conditions for a hierarchy and which can therefore be expressed by means of equations (18) in terms of $n+1$ independent variables may, after a linear transformation of independent variables given by equations (16) and (17), be expressed in terms of a very large number, N, of independent x's by means of equations (12) and our italicised Rule; and it follows that n q's whose correlations approximately satisfy the conditions for a hierarchy may be approximately expressed by means of equations (12) and our italicised Rule.

We have therefore two alternative ways of expressing the consequences of the fact that the correlations of any n sufficiently dissimilar mental tests approximately form a hierarchy. These alternatives are:

[1] Garnett, *Proc. Roy. Soc.* XCVI. 105.

(*a*) Each of the qualities tested tends to be made up of a single general factor and specific factors only; and

(*b*) Each of the qualities tested is made up of a very large number of elements which tend to enter into the various tests according to the 'all or none' law of equations (12) and according to the italicised Rule on p. 248.

The alternative mode (*b*) of expressing the approximate satisfaction of the conditions for a hierarchy may be put in other words by saying that the approximate satisfaction of these conditions implies that the qualities tested are made up of a very large number of elements of which some (lower level) elements are entirely specific, while other (higher level) elements come into play in different activities; but (1) so that any two given higher level elements both of which enter into any test *tend* to enter in the same ratio (unity or any other) into that test as into any other test into which they both enter; and (2) so that the number of higher level elements that enter as group factors into any two or more tests *tends* always to be proportional to the product of the whole number of elements in the two or more tests in question.

These two qualifying conditions, numbered 1 and 2 in the preceding sentence, are satisfied in dice-throwing tests such as have been employed by Dr Thomson in the paper[1] to which reference has already been made, and in a further paper[2] of recent date. For in such tests the scores, say x_a and x_b, of any two dice that enter both into the sth test measured by q_s and into the tth test measured by q_t enter into both in the same ratio, namely unity[3]. And we have already seen (on p. 248) that on the average of a very large number T of trials, in each of which the same number (say Nc_s) of dice enters into the sth test, the number of x's that enter into any two or more of the tests $q_s, q_t, \ldots, q_w$ is $Nc_sc_t\ldots c_w$ and is therefore proportional to the product of the numbers ($Nc_s, Nc_t, \ldots, Nc_w$) of elements in the two or more tests in question.

Without the two qualifying conditions enumerated in the last paragraph but one, the tendency of the correlations of any set of sufficiently varied mental tests to satisfy the conditions for a hierarchy would not be expressed by that paragraph. Nor would it be expressed by any such statement as

> "The mind, in carrying out any activity such as a mental test, has two levels at which it can operate. The elements of activity at the lower level are entirely specific; but those at the higher level are such that they may come into play in different activities. Any activity is a sample of these elements."

[1] This *Journal*, VIII. 1916, 271. [2] This *Journal*, IX. 1919, 337.

[3] See for example Garnett, *Proc. Roy. Soc.* XCVI. 98, equations (22).

unless the qualifying conditions are introduced. Dr Thomson has accordingly been careful to qualify the conclusion we have just quoted by means of the following final sentence:

"The elements are assumed to be additive like dice, and each to act on the 'all or none' principle, not being in fact further divisible[1]."

We have quoted Dr Thomson's words in order to point out how vast a difference this final qualifying sentence makes. Without it, or without the equivalent qualifying conditions numbered 1 and 2 above, Dr Thomson's conclusion would be no more than an expression in words of the fact[2] that each of n correlated variables can always be expressed as a linear function of a sufficiently large number of independent variables some of which are specific factors while the remainder may enter into any two or more of the expressions for the correlated variables in question. In other words, without the final qualifying sentence or its equivalent, Dr Thomson's statement would not express the fact that the conditions for a hierarchy tend to be satisfied by the correlations of any set of sufficiently varied mental tests.

The fact that the correlations between any set of sufficiently dissimilar mental tests *tend* to satisfy the conditions for a hierarchy therefore justifies speaking, either with Professor Spearman (a) of a tendency among the qualities tested to exhibit a single general factor as we have defined it and specific factors only, or (b) of a tendency among the qualities tested to be expressible, approximately according to equations (12) and the italicised Rule on p. 248, in terms of a very large number of higher level elements that are in effect, as we have seen, small interchangeable parts of Professor Spearman's general factor. But, while waiting for further experimental evidence, it is surely preferable, because so much more simple, to speak and to think of Professor Spearman's general factor together with specific factors than of the large number of elements of which the general factor and the specific factors may be supposed to be made up according to equations (16) and (17).

If we have been right in suggesting elsewhere[3] that the measure, g, of the single general factor is a measure of the subject's Will or power to concentrate attention or (in other words) to concentrate nervous excitement in a particular system of nervous arcs, the various x's may measure the numbers of a subject's neurones rendered active, according to

[1] This *Journal*, IX. 1919, 344.

[2] Cf. Garnett, *Proc. Roy. Soc.* XCVI. 95.

[3] This *Journal*, IX. 1919, 350.

Lucas's and Adrian's 'all or none' law, in that subject's exercise of his different qualities. The whole number of a subject's neurones rendered active by an effort of his Will would then be proportional to his g.

VIII.

The results obtained in this paper, which is only concerned with variables that are distributed according to the normal law and measured in such units as will give to each the same standard deviation σ, may be summarised as follows:

(1) Any n correlated variables can always be expressed in terms of n independent variables which may be selected with $\frac{1}{2}n(n-1)$ degrees of freedom.

(2) The number of independent 'conditions for a hierarchy' (the satisfaction of which conditions by the correlations of n variables implies that each of these variables is compounded, according to the vector law in equations (10) or (18), of a single general factor and of one of n specific factors) is $\frac{1}{2}n(n-3)$.

(3) Since the correlations between three correlated variables will therefore always satisfy the conditions for a hierarchy, three correlated variables can always be expressed in terms of four independent variables of which one is a single general factor[1] while each of the others is a specific factor.

(4) The single general factor among $n+1$ independent variables employed for the expression of n variables whose correlations satisfy the conditions for a hierarchy is *unique* and is defined as *the* single general factor of any such n variables. But there can always be found an infinite number of general factors of n correlated variables: general factors which must be distinguished from *the* single general factor of n variables whose correlations satisfy the conditions for a hierarchy.

(5) By means of an infinite number N of independent variables $x_1, x_2, \ldots, x_N$ it is possible to express each of n variables $q_1, q_2, \ldots, q_n$ whose correlations satisfy the conditions for a hierarchy in the form $q_s = \frac{1}{\sqrt{Nc_s}}$ (sum of Nc_s whole x's), where $s = 1, 2, \ldots, n$ and where the x's in each q are selected according to the italicised Rule on p. 248. That Rule involves selecting the x's for the expression of each q so that the number of x's common to any two or more q's, say $q_s, q_t, \ldots, q_w$, shall be $Nc_sc_t\ldots c_w$ and therefore proportional to the product of the whole numbers of x's employed for the expression of each of the q's in

[1] Comparisons of tests of three mental qualities only will therefore throw no light upon the existence of a single general factor.

question. The distribution of the x's among the q's defined in the above Rule is the same as would be obtained by taking the average of a very large number of trial distributions so that the same proportions $c_1, c_2, \ldots, c_n$ of the whole number of x's always entered into $q_1, q_2, \ldots, q_n$ respectively. Moreover, the distribution of the x's that results from this Rule is such that the number of x's entering as general factors (not single general factors) is $Nc_1c_2 \ldots c_n$ and therefore, since each c is a positive proper fraction, very small indeed compared with the number of group factors and specific factors. By making N very large instead of infinite n q's whose correlations satisfy, to any desired degree of approximation, the conditions for a hierarchy can be expressed in the same form by following the same Rule as nearly as possible.

(6) There are two equivalent ways of stating the consequences of the fact that the correlations between any set of sufficiently dissimilar mental tests *tend* to satisfy the conditions for a hierarchy. These two alternatives are:

(*a*) The measures $q_1, q_2, \ldots, q_n$ of any set of sufficiently dissimilar mental qualities *tend* to be made up, according to the formula

$$q_s = r_{sg} \cdot g + \sqrt{1 - r^2_{sg}} \cdot \xi_s$$

or the equivalent vector law $q_s = g \cos \theta_s + \xi_s \sin \theta_s$ where $\theta_s = \cos^{-1} r_{sg}$, of $n + 1$ independent variables of which one is a single factor and the remainder are specific factors, where r_{sg} is the correlation between the sth quality q_s and the unique single general factor g; and

(*b*) The measures $q_1, q_2, \ldots, q_n$ of any set of sufficiently dissimilar mental qualities *tend* to be expressible—by following the above Rule but not otherwise—according to the formula

$$q_s = \frac{1}{\sqrt{Nc_s}} \text{ (sum of } Nc_s \text{ whole } x\text{'s)}$$

in terms of an infinite number of infinitesimal independent variables $x_1, x_2, \ldots, x_N$ that may be regarded[1] as the constituent interchangeable elements of the single general factor, where c_s $(= r^2_{sg})$ is the proportion of the whole number of x's that enter into the expression for q_s.

(7) It is submitted that of these two equivalent statements, (*a*) and (*b*), the former is preferable because so much more simple.

(8) A suggestion is made for explaining, in psychological terms, the equivalence of the two alternative forms of statement, (*a*) and (*b*).

[1] Cf. a joint note on the hierarchy of abilities by Garnett and Thomson, this *Journal*, IX. 1919, 368.

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OBSERVATIONS ON THE DE SANCTIS INTELLIGENCE TESTS[1].

By W. B. DRUMMOND.

(*From the Baldovan Institution for Feeble-minded Children.*)

I. Introductory.
II. Description of the tests.
III. The material required.
IV. The nature and claims of the tests.
V. The tests applied to normal children. Conclusions as to the grading of the tests.
VI. The tests applied to mentally defective children.
VII. Resulting criticisms.
VIII. Conclusions.

I. *Introductory.*

In the year 1905 Binet published his first article "On the Measurement of Intelligence" in the *Année Psychologique.* Immediately afterwards de Sanctis[2] published in the same journal a paper on "The Types and Degrees of Mental Deficiency" which was inspired by the same idea, namely that instead of grading defectives by applying to them such loosely defined terms as idiot, imbecile, feeble-minded and so forth, it would be at once more scientific and more practically useful to arrange a series of tests of gradually increasing difficulty which might be used as a standard whereby to measure the actual amount of intelligence present in any given case. The tentative series of tests published by Binet in 1905 was subsequently elaborated by him, and a scale of graded tests was published in 1908, which was further revised and published in its final form in 1911.

[1] The Author was assisted by a grant from the Medical Research Committee.

[2] Sante de Sanctis: (*a*) "Tipi e gradi d' insufficienza mentale," *Annali di Nevrol.* Naples, 1906. (*b*) "Types et Degrés d'insuffisance mentale," *L'Année Psychol.* No. 12, 1906, 70–84. (*c*) "Mental Development and Measurement of the Levels of Intelligence," *J. of Educ. Psychol.* Nov. 1911, 498–508.

Binet's idea that the intellectual level of a defective child can be indicated in terms of 'Mental Age,' the said Mental Age being ascertained by the application of a scale of graduated tests obtained by experimental observations upon normal children of various ages, has proved to be a most fruitful one. De Sanctis, as has been stated above, shared with Binet the idea of formulating a scale of tests of gradually increasing difficulty by means of which the mental capacity of a defective person might be gauged. He did not, however, possess Binet's fruitful idea of Mental Age, and he did not grade his tests in accordance with the ages at which they can be successfully passed by normal children. His scale consists simply of a series of six problems or tests of gradually increasing difficulty, and de Sanctis was quite satisfied to indicate any subject's capacity by stating how many of the six tests he could pass successfully.

The primary object of the de Sanctis tests differs from that of the Binet tests. The difference may be expressed very simply. The Binet scale is intended to measure the amount of intelligence; the de Sanctis tests are intended to measure the degree of mental defect. Binet himself states that the chief value of his scale is that it affords a child who is suspected of being mentally defective an opportunity of rehabilitating himself. A child who is very backward at school may be reported by the teacher as mentally defective. Such a child may yet be able to pass the tests suitable for his age. If so, however dull and backward he may be, he is quite intelligent and should be taught in an ordinary school, but by a different teacher. The Binet scale may be used for testing normal children, and is also used for gauging the mental status of the mentally defective. The de Sanctis tests are stated by their author to be specially useful for dividing the mentally defective into three grades—idiots, imbeciles, and the feeble-minded or morons. They are recommended for the examination of children between the ages of seven and sixteen.

The de Sanctis scale is certainly far behind the Binet-Simon scale in practical utility. Nevertheless it has seemed worth while to apply it to a series of normal, and of mentally defective children, of various ages, with the objects (*a*) of ascertaining whether the tests are practical and properly graduated; (*b*) of discovering whether the claim made by de Sanctis that his scale is capable of differentiating between mentally defective and normal persons is justified; (*c*) of standardising the method of using the tests; and (*d*) of grading the de Sanctis tests in terms of Mental Age so that they might be available as substitute tests for some of the problems in the Binet-Simon scale. Every one who has worked with the Binet-Simon scale knows how useful a series of substitute tests would be, for it is by no means uncommon to find a child who cannot be successfully tested either because some other person has tested him recently, or because he has been coached in some of the tests by some interested adult or even by some other child who has undergone examination[1].

II. *Description of the Tests.*

As the de Sanctis tests are not well known[2], no translation of them, so far as the present writer knows, having been published in Great Britain, it may be well, before proceeding further, to describe the tests by means of a somewhat free translation of the directions given by their author. The tests should be applied at a time when the subject is at his best, *i.e.* in good humour, and not fatigued, or disinclined to submit himself to examination.

Test 1. Say to the subject "Give me a ball." (Show the child five glass balls of different colours. The observer notes the time it takes the child to respond, and when the response is obtained, covers the balls with a screen.)

Test 2. Say, "Which is the ball you gave me?" (Exhibit the balls arranged in a row. Time, and cover with a screen as before.)

Test 3. "Do you see this piece of wood (a cube)? Pick out all that are like it in that group." (Five cubes, three cones, and two oblong blocks mixed together are shown. Time and screen as before.)

Test 4. "Here is a card. Point to all the figures on it shaped like this piece of wood" (a cube). (The card, described below, shows a series of black squares, oblongs, and triangles arranged in rows.)

Test 5. "Here are some blocks of wood like the figures you have been pointing out on the card." (Twelve cubes of different sizes are arranged upon the table.) "Look at them carefully and (1) tell me how many there are." (The child is expected to count.) (2) "Now tell me which of them is biggest"; (3) "and which of them is the furthest away from you." (Note the time and the mistakes and screen as before.)

Test 6. This test consists in asking the child the following questions: (*a*) "Are big things heavier or lighter than small things?" (*b*) "How does it happen that a little thing sometimes weighs more than a big thing?" (This question is given if the child has replied correctly to the first.)

[1] Many additional tests will be found in *The Stanford Revision of the Binet-Simon Scale*, by Lewis M. Terman (Warwick and York, 1917), and *The Measurement of Intelligence*, by Lewis M. Terman (Houghton Mifflin Co. N.D.—? 1916).

[2] O. Decroly and J. Degand, *La mesure de l'intelligence chez les enfants. Seconde contribution critique. La méthode de de Sanctis, L'Année Psychol.* 1907, 230–304.

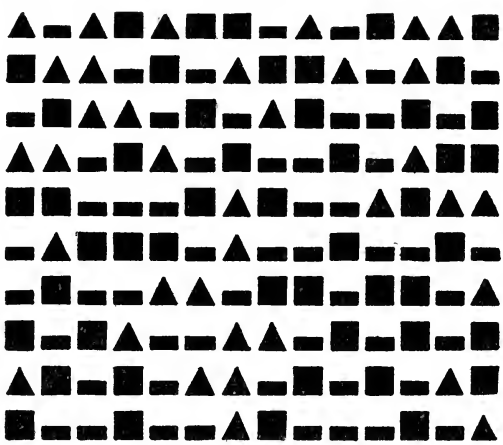
(*c*) "When things are far away do they look larger or smaller than things which are near?" (The object of this question is to prepare the child for the following.) (*d*) "Are they really smaller or do they only appear to be so?" (The object of this question is to ascertain whether the subject is conscious of physiological optical illusions.)

III. *The Material required.*

Test 1. The coloured balls should not be too small. De Sanctis recommends glass balls, but any other material would serve the purpose. A paste-board box will serve the purpose of a screen.

Test 3. The cubes required should measure about $1\frac{1}{2}$ inches. The pyramids should be about two inches high, and have about the same base as the cubes. The oblong blocks should measure $2\frac{1}{2} \times 1 \times \frac{1}{2}$ inches.

Test 4. The card has ten rows of fourteen figures each. The figures are of three kinds, squares, oblongs, and triangles. The figures have a base of about half an inch. The triangle has the altitude of the square. The oblong is half the height of the square.



The de Sanctis Form-Test. (About one-fourth the actual size.)

Test 5. The twelve cubes are graded in size from half an inch to three inches.

When the tests are given the child must be quite at his ease. Each question may be repeated three times. If the child does not respond, the examination must then be stopped. If there is any doubt as to whether the child has reached his limit, the examination should be repeated again after a few days.

IV. *The Nature and Claims of the Tests.*

In this series of tests the progressive difficulty consists, according to de Sanctis, in the fact that they begin by calling upon the lower mental functions and ultimately make a demand upon the higher. He considers this a more rational procedure than that which consists in giving the subject a series of harder and harder tests of memory or attention. In the mentally defective it is the higher mental functions that are lacking. Many defectives have excellent memories and a very good capacity for attention, which is more than can be said of many quite intelligent people.

De Sanctis claims that by his series of tests one obtains information as to the following: (1) Capacity of adaptation to experience which comprises adaptation to work and certain conditions of attention, of perception, and of will. (2) Immediate memory of colours. (3) The capacity to recognise colours and forms, and to recognise the identity of a plane figure with a solid. (4) The tenacity or duration of attention. (5) The capability of enumerating objects and of judging of their quantity, size, and distance. (6) The capacity to reason about objects no longer present to the senses, and on the general concepts derived from them. This involves not only attention and imagination but also the faculty of generalisation and abstraction. (7) The rapidity of perceiving, of reflection, and of acting.

De Sanctis lays considerable stress upon the rapidity of the child's response, but he admits that this is no gauge of the subject's intellectual level, inasmuch as rapidity of response may depend upon the condition of the sense organs and the muscles, and not solely on the capacity for perceiving, and appreciating.

Bearing de Sanctis's claims in mind, let us now examine the tests seriatim and consider which particular 'functions' are requisite for the solution of the various tests.

Test 1. This test obviously requires an elementary degree of attention, the ability to understand a request, and the capacity to obey a single order, and to carry out a coordinated movement of hand and eye.

Test 2. This test involves in addition the possession of immediate memory for colour.

Test 3. This test involves discrimination of solid form, and self control, if the child stops after picking out all the cubes. Many young children, after picking out all the cubes, thereby showing their capacity to discriminate solid forms, proceed to pick out the pyramids, and the

the oblongs, sometimes arranging these in groups. This unasked for action may result partly from the tendency automatically to continue a pleasurable activity, but may also be in part a manifestation of the acquisitive instinct.

Test 4. To begin with, this test involves the ability to recognise a plane form (the square) as corresponding to a solid form (the cube). It is curious that de Sanctis says nothing about the manner in which the cube is to be held. It is obvious that the cube may be shown to the child in such a way as to present a plane surface of a square shape, in which case it is very easy for the child to recognise the square on the chart as the form demanded; but if the cube were presented with an angle directed to the child and three or four faces visible the problem would be more difficult. In my own observations, the child was shown the cube in such a position that a square surface was plainly visible.

The square being recognised (recognition of plane form), the test then involves the ability to follow the lines of the chart, *i.e.* coordination of hand and eye on a higher level than is required in previous tests, and sustained attention of a very considerable degree.

In examining a number of children, it is quite apparent that the ability to find and follow lines which is acquired during lessons in reading is of great assistance to the subjects. The strain on attention is shown by the fact that mistakes are much more frequently made near the end than in the course of the test. Some children make mistakes at the beginning owing to a failure to understand what they are expected to do, then they perform the greater part of the test without mistake, and finally, becoming fatigued, they make several mistakes towards the end. In many cases mistakes result from hurry or carelessness, whereas more cautious or more deliberate, though not more intelligent, children perform the task perfectly. These variations are of considerable interest, and this test alone throws a good deal of light on the capacity of the subject.

Test 5. This really consists of three different tests, of which the first is much the hardest. This first part (5 *a*)—the enumeration of twelve blocks of different sizes—involves (1) a knowledge of the number series and (2) the possession of the much later acquired fundamental idea in arithmetic of the one to one correspondence.

The second part of the test (5 *b*) requires discrimination and judgment of size, and is the easiest part of the test.

The third part (5 *c*) requires discrimination and judgment of distance. Children often fail in this owing to their tendency to respond as quickly as possible leading them to point to one of the cubes before they have quite grasped what they are expected to do.

Test 6. This really consists of two distinct tests which are not of equal difficulty, though both involve the capacity of generalising from experience. Both tests are open to the objection that they invite guessing, though an experienced examiner will seldom have any difficulty in deciding whether a correct answer is the result of guessing or of understanding. For example if a little girl says that little things may be heavier than big things, and by way of example says that "a weight is heavier than sugar," one sees that the child is speaking from her own experience and one realises the interest with which she has watched the process of weighing sugar at the grocer's. This is the least satisfactory of the de Sanctis tests.

V. *The Tests applied to Normal Children.*

In order to ascertain the value of the de Sanctis tests and attain the aims detailed above, the writer began by testing a series of normal children attending free Kindergartens and Board Schools. The children tested varied in age from three to nine years. Between fifteen and thirty children were tested at each age, with the exception of the first, as only seven three-year old children were available. All the children of three and four, and some of those of five years of age were attending Kindergartens, and came from very poor homes. The three-year old children particularly were very backward physically and mentally, compared to children in better circumstances. The same statement applies to most of the older kindergarten children but not quite to the same extent, as some of them had obviously benefited by their kindergarten training. In the Kindergartens visited all the children were examined. In the Board Schools the teachers were asked to send 'average children' for examination. Whether the children were really average is another question. In carrying on this investigation, and an investigation of the Binet tests, two things were quite apparent and are worth noting. The first was that children of the same age taken from different classes were often at a quite different level of mental development. This will be readily understood, when it is considered that in a large Board School there must be two or three different classes in which the children are of practically the same age. Naturally the more advanced children are put in one class, while the less advanced children of the same age are put in another. Consequently an observer who examined the children of a particular age from one class only might be led astray. The second point was that although the teachers were asked to send average normal children, it was often difficult to get them to attend to that point. Indeed many of them, when they heard that the children were to be examined by a doctor, purposely selected exceptional children in order that they might obtain the observer's opinion upon them. In one school quite a number of exceptional children were noted, and it was found that the infant mistress had been selecting children whom she considered defective in number sense, in ability to learn to read, or in some other way.

Table I.

Table showing percentage of normal children passing the various Tests.

Number tested	7	20	29	23	18	19	16
Age	3	4	5	6	7	8	9
Test 1	100	100	100	100	100	100	100
2	?	95	97	100	94	100	100
3	30	60	100	100	100	100	100
4	—	60	65	91	100	100	100
5 *a*	—	10	38	69	77	100	100
5 *b*	*	55	82	91	94	100	100
5 *c*	—	35	61	86	82	100	100
(1) 6 *a*	—	—	—	—	11	63	56
(1) 6 *b*	—	—	—	—	11	36	56
(2) 6 *c*	—	—	40	56	82	84	100
(2) 6 *d*	—	—	27	47	72	80	100

* One child correct out of seven—obviously an exceptional case.

Test 1. All the three-year old children tested were successful, and although no children under three years of age were tested, one may safely say that normal children may be expected to be able to pass this test before they are three years of age. Between their second and third birthdays children make a very remarkable advance in their mental development, but individual variations are so great that it is not possible to grade this test more accurately.

Test 2. One minute is supposed to elapse between tests 1 and 2. In some cases the writer applied test 3 in the interval. This, however, is not to be recommended, as it makes test 2 more difficult than if the child's mind had not been so exercised. Nearly all four-year old children succeed with this test, and so do a large proportion of three-year old children. Bearing in mind that the kindergarten children tested were for the most part somewhat retarded in their development, we may fairly place this as a three-year old test.

Test 3. In carrying out this test it is important to avoid giving the child any assistance. The directions should be followed exactly. At most, one may amplify the words used thus—"Pick out all that are like it and put them there," indicating a place on the table. This test, as already mentioned, very often brings out the phenomenon known as automatism. A child lifts the cubes from the group one by one and places them on one side, but instead of stopping he proceeds to remove and set aside the other blocks also. Sometimes he places them among the cubes, sometimes he sets them in another place. These differences should be noted, but if the right blocks are selected first the test should be marked 'passed.' A few three-year old children, a majority of four-year old children, and practically all five-year old children pass this test. A four-year old child who cannot pass may fairly be considered backward.

Test 4. This test may be carried out, as already mentioned, in various ways which present various degrees of difficulty. De Sanctis evidently expects the child tested to follow the card systematically line by line, and to point out every square without mistakes or omissions. Consequently a child should not be counted 'passed' who does not succeed in completing the test in this way without assistance. Complete success involves a good deal more than ability to distinguish the squares from the other figures, for the child is called upon to follow the lines consecutively and to maintain a considerable degree of attention and mental concentration for a sufficient time to complete the task. The test is a valuable one because there are so many degrees of partial success that an observer may very quickly arrange a group of children in classes, thus:

1. Children who fail completely.
2. Children who point to squares and oblongs but omit triangles.
3. Children who point to squares only, but pick them out unsystematically on any part of the card.
4. Children who can point out the squares in a given line, when the line is indicated by the observer.
5. Children who can pick out and follow the lines for themselves but become fatigued and make mistakes towards the completion of their task.
6. Children who are completely successful.

This test may be varied by making the children count the squares. If the squares are counted separately in each line, the process of counting seems to make the test easier, perhaps by making it more interesting. On the other hand it is more difficult for the child to count the total number of squares on the chart, because many children who can recognise the squares easily have no facility in counting beyond ten or twenty.

Many children tested make one or two mistakes at the outset through want of confidence or from not being quite sure of what they are to do, yet the rest of the test is carried out correctly. Such children may be regarded as having been practically successful. In marking the children I made use of the following signs: + = quite correct; +? = one or two mistakes or omissions; − = failed or more than three mistakes or omissions.

It is not till seven years of age that all children are successful or practically successful with this test, but at six years of age twenty-one out of twenty-three were marked + or +?

The test may be graded as a six-year old test. Possibly this may be true only of children who have had some lessons in reading, and consequently know how to follow a line, and how to proceed from one line to another.

Test 5. The twelve blocks must be arranged on the table in such a way that all are easily seen. They should be about an inch apart and the furthest away should not be the biggest. In my own tests I placed the second largest block furthest from the child, while the largest block was placed to the child's left and the third largest to the right. The largest blocks were placed as far from one another in the group as possible in order to make the test of the child's ability to compare sizes more severe. Occasionally, though rarely, a child who is asked to count the blocks will lift them up one by one and set them aside. In such a case the child should not be interfered with until the blocks have been counted, but the blocks must be placed in their original positions before the further questions are asked.

The most difficult part of this test (5 *a*) is the enumeration of the blocks, and it is not till eight years of age that 100 per cent. of the children are successful. Of seven-year old children 77 per cent. pass the first part of the test, 94 per cent. the second, and 82 per cent. the third part. For six-year old children the percentages are 69, 91, and 86.

The test may be graded as a seven-year old test. Although a large proportion of six-year old children are able to pass it, the proportion is less than for test 4.

Test 6. Before the questions for this test are put to the child the blocks used in test 5 should be placed out of sight.

The second portion of the test (6 *c* and *d*, see p. 262) might be expected to be the more difficult, as the interests of young children, especially young town children, are directed to near objects. However, it is really the easier, and calls for little comment, except that it is necessary for the observer to make sure that the child really understands. De Sanctis does not insist that his *ipsissima verba* must be used in putting his tests, but says that words must be used which the child can understand. Hence, as children are concrete thinkers, it seems allowable to express the question thus: "If a man went to the end of the street would he look smaller or bigger?"

Observe that this is the correct order of the words. One should not say 'bigger or smaller,' because many young children have a tendency to repeat the last word heard, and therefore might give the correct answer accidentally if the word 'smaller' were placed at the end of the sentence.

This part of the test is passed by 82 per cent. of seven-year old children, while 72 per cent. answer correctly the further question "Is he really smaller or does he only appear so?" For six-year old children the percentages are 56 and 47.

The first part of this test (6 *a* and *b*, see p. 261) also consists of two questions. The preliminary question is "Are big things heavier or lighter than small things?" It is evident that de Sanctis expects the child to answer that little things may be heavier than big ones, and counts the child wrong who replies "Big things." But is this quite fair? Other things being equal, big things *are* heavier than little ones, and therefore, when a child answers so, it seems only just to interpolate the further question, "Are big things *always* the heavier?" No doubt this is a leading question, but a large proportion of young children answer 'Yes' quite positively. A few who say 'No' may be only guessing, and are to be eliminated by the succeeding question. De Sanctis does not say what answer he expects to the question "How does it happen that a little thing sometimes weighs more than a big thing?" nor how he would mark a child who gives a correct example but no explanation. As one can scarcely expect a child to do more than give a correct example, it seems preferable, and gives more definiteness to the test, to ask for one, thus "Can you tell me any little thing that is heavier than a big thing?" To this question children give such answers as "A weight is heavier than sugar"; "a stone is heavier than a basket"; "iron is heavier than paper."

For the first part of the test (*i.e.* 6 *b*) only 11 per cent. of seven-year old children were successful. Of eight-year old children 36 per cent. passed, and of nine-year old children 56 per cent.

Conclusions as to the Grading of the Tests. The following grading of the de Sanctis tests should be confirmed or modified by the examination of a larger number of normal children[1].

[1] My paper was written before I saw L. Martin's paper, "A Contribution to the Standardization of the de Sanctis Tests," *Training School Bulletin*, XIII. 1916, 93–110.

Table II.

Test	Age
1	2
2	3
3	4
4	6
5	7
6	9

VI. *The Tests applied to Mentally Defective Children.*

All the defective children examined were resident in Baldovan Institution, near Dundee. The group dealt with consisted of 65 boys—practically all the boys who attend the school attached to the Institution. The girls were left out of account in the meantime, as their examination had not been quite completed. It is believed, however, that the examination of the girls would yield quite similar results. The children were examined individually in a quiet room, and they were, in nearly all cases, quite at their ease during the examination.

The general results obtained are summarised in the following table, which shows the number of children examined, their mental ages as ascertained by the Binet tests, and the number and proportion of the children of each age who succeeded with each of the de Sanctis tests.

Table III.

Table of children tested by the de Sanctis Tests.

(Showing the number of subjects who passed each test *but failed with higher tests.*)

		De Sanctis tests										
Mental age	No. of children	6 (1)	6 (2)	5 *a*	5 *c*	5 *b*	4 *a*	4 *b*	4 *c*	3	2	1
9	3	1	2	—	—	—	—	—	—	—	—	—
8	4	—	2	—	1	1	—	—	—	—	—	—
7	9	—	4	5	—	—	—	—	—	—	—	—
6	9	—	1	2	—	3	—	2	1	—	—	—
5	15	—	—	1	4	3	4	1	1	1	—	—
4	12	—	—	1	2	1	—	2	1	4	—	1
3	9	—	—	—	—	—	—	—	3	3	2	1
2	4	—	—	—	—	—	—	—	—	2	—	2

A comparison of the figures in Tables II and III leads to a very interesting result, for it shows that the grading of the de Sanctis tests for normal children of various ages is correct for mentally defective children *of corresponding mental ages* as ascertained by the Binet-Simon scale. This is shown in the table below.

Table IV.

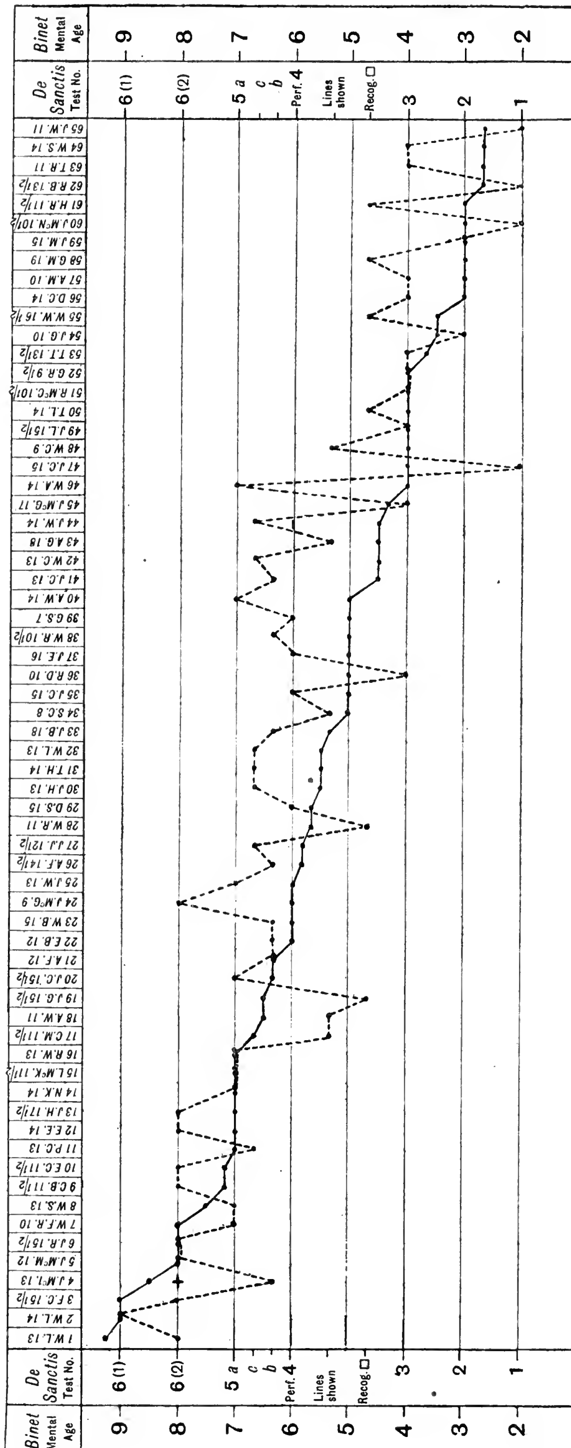
de Sanctis tests	Normal children's age	Defective children of corresponding 'mental ages'				
6	9	1	successful	out of	3	tested
5	7	10	,,	,,	13	,,
4	6	8	,,	,,	9	,,
3	4	26	,,	,,	27	,,
2	3	8	,,	,,	9	,,
1	2	4	,,	,,	4	,,

VII. *Resulting Criticisms.*

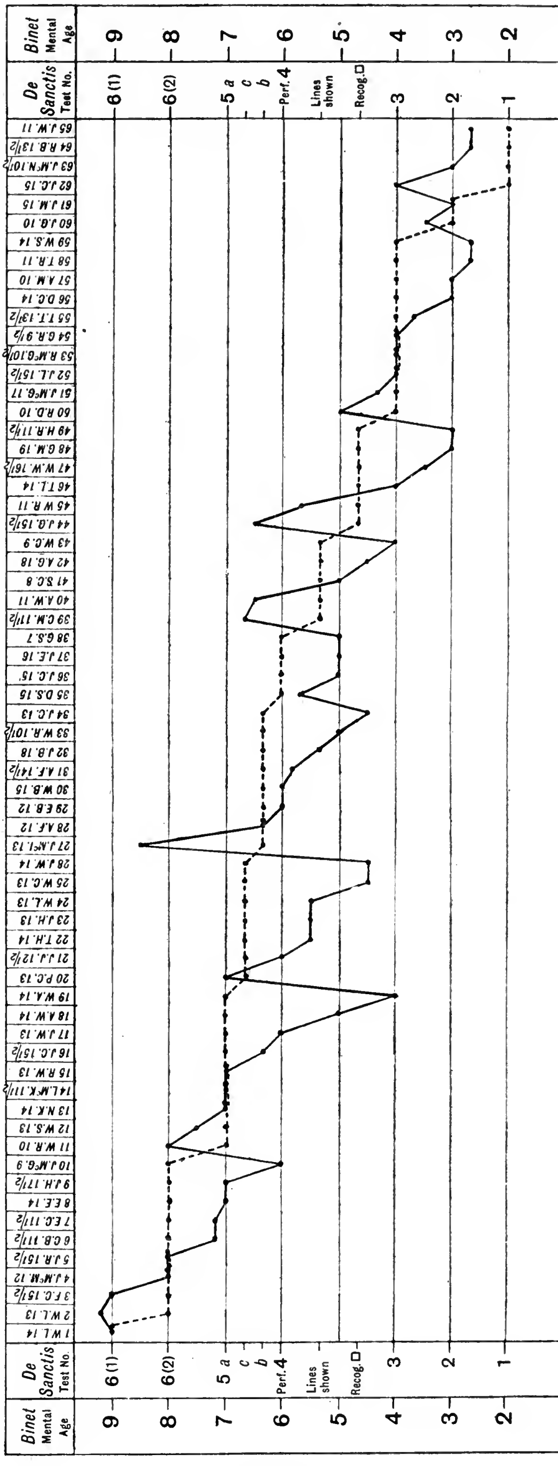
If we now proceed to consider whether these problems really do, as de Sanctis claims, test higher and higher mental functions, it will be found that the contention can hardly be sustained. In the first place several of the tests differ largely in the fact that, like Binet's tests, they make different demands upon the same function. The first four tests, for example, demand progressively a greater and greater degree of attention. In the next place, it is doubtful whether the chief 'functions' tested are really higher in successive problems. The early tests call for the recognition of colour before the recognition of form, but studies of infants and young children show that form is recognised in advance of colour. Again, the tests place the recognition of solid form before the recognition of plane form, which does not appear to be in accordance with the known facts of mental development. It is also a fault that some of the tests contain minor tests of differing degrees of difficulty, whereas each test should be a unit.

Facility in the successful performance of the fourth test is greatly aided by previous instruction in reading; and reading, being dependent upon a special 'centre' in the brain, may be regarded as a special mental 'function.' It does not follow, however, that success with this test proves a child to be in possession of higher mental functions than a child who passes test 3, but does not quite succeed with test 4. The assistance a child gains from lessons in reading is simply facility in finding and following consecutive lines, and this particular facility is, as a matter of fact, acquired by word-blind children.

The first test which really requires the possession of a new 'function' is the fifth, when the subject is called upon to enumerate twelve blocks. It is very probable that number is dependent upon the development of a special brain 'centre.' It is certain that it develops late, and that mentally defective children are frequently, if not usually, more defective in number than in other faculties.



Graph I.


Binet Mental Age
De Sanctis Test No.
6 (1)
6 (2)
5 a
c
b
Perf. 4
Lines shown
Recog.
3
2
1
9
8
7
6
5
4
3
2

Graph II.

Although the de Sanctis scale does not answer to its author's idea of testing successively higher and higher functions, it is worth while asking whether this idea is likely to be a fruitful one. The present writer would answer this question in the negative. The various mental functions are not built up one upon another in such a way that a higher function does not make its appearance until the lower ones upon which it depends have reached their full, or even a high degree, of development. If it were so, no doubt a series of tests designed to show which functions were present and which were still absent would be of great practical utility, and could be designed without much difficulty.

Although these considerations seem to show that de Sanctis, in choosing and arranging his tests, was to some extent working upon a false ideal, it by no means follows that the tests themselves are without practical utility. To the practical utility of the tests we must now turn our attention.

One noteworthy feature of the de Sanctis tests is that a child rarely succeeds in passing any test higher than a test in which he has failed. In this respect the de Sanctis scale differs from the Binet scale. A child's mental age, according to Binet, is the age all the tests appropriate to which he succeeds in passing, *plus* one year for every five tests successfully passed from higher ages. It is not uncommon to find that a child passes tests several years in advance of some with which he failed. Wallin has shown that epileptics are peculiarly liable to exhibit this phenomenon of 'scattering,' their mental age being frequently made up of 'advance credits' spread over a number of years.

The graphs[1] accompanying this paper show the mental ages of the children according to the Binet scale, but do not show how the children reacted to the individual tests. They show also how uniformly the children attained a definite level on the de Sanctis scale. Where—exceptionally—a child failed in one of the lower tests while passing a higher, the fact is indicated on the graph by the minus sign.

When we examine the graph showing the comparative results obtained by the use of the two scales, it appears at first sight that, although

[1] The graphs show the mental ages of sixty-five defective children as ascertained by the Binet tests, and the de Sanctis tests passed by each child. In Graph I the children are arranged in order according to the Binet tests; in Graph II according to the de Sanctis tests. Binet tests —; de Sanctis tests - - - -. The actual ages of the children are shown beside their initials. The de Sanctis tests are indicated in the margin by number. Recog. □ = child can point out squares accurately but not consecutively; 'lines shown' = child points out all the squares if shown the beginning of each line; 'Perf.' = child passes test without any help. 6 (1) and 6 (2) = parts 1 and 2 of Test 6.

there is a very general resemblance between the two curves, the discrepancies are very numerous, and that in many cases the differences are considerable. Closer examination, however, shows that the differences are much exaggerated owing to the fact that there are fewer stations upon the de Sanctis scale. For example between the de Sanctis tests 5 and 6 there is a period of two years. Normal children of seven succeed with test 5, but not until they are nine years old do they succeed with test 6. Accordingly a child whose mental age is eight years according to the Binet-Simon scale might be expected to succeed with de Sanctis test 5 and to fail with test 6. This would lead to his being placed on the seven-year line on the graph (if test 6 were taken as a unit), and his position would appear too low simply from the absence of any station between the seven and nine-year stations on the Binet-Simon scale. To diminish this irregularity I have, for reasons explained above, divided test 6, and children who passed one of the sections of the test are placed upon the eight-year level in the graph.

Where there is a great discrepancy between the results of the two scales, the question arises, which is the more correct? As we possess no independent means of testing the results, I have asked Miss E. Ross, M.A., Headmistress of the School at Baldovan Institution, who very kindly drew the graphs, to express her opinion based upon observation in school. Briefly her opinion may be expressed as follows:

Binet scale:	too high—3 cases,
	too low— 4 cases,
	probably too low— 3 cases.
de Sanctis scale:	too high—9 cases,
	too low— 5 cases.

In some of these cases the discrepancies can easily be explained by reference to the individual children, in others the explanation is to be found in the nature of the tests. Thus the three children judged too highly placed upon the Binet scale are all comparatively old, their ages being thirteen, fourteen, and sixteen years. These children are making little if any progress in school work, but having much more experience of the world than younger children they are better able to score in a series of tests of general capacity than younger children whose progress in school work makes a better impression on the teacher.

Of the three children considered as placed too low in the Binet scale no general explanation can be given. One of these children suffers from idioglossia and his speech is almost unintelligible. One is partially paralysed.

The most striking discrepancy is found in the large number of cases which appear to be too highly placed in the de Sanctis scale. Some of these are children who have passed a higher test and failed in a lower one, the failure not being allowed for in fixing their position on the graph (failures are indicated by the minus sign); others, on the eight-year old level, were placed there with some hesitation as it was not certain that the child had not succeeded with one of the six tests by confident guessing. When there is any uncertainty as to whether a child is guessing in this test, it is probably wise *not* to give the child the benefit of the doubt.

The de Sanctis tests very rarely seem to give a truer estimate of a child's capacity than the Binet tests. They may occasionally do so when the child is of an essentially practical turn of mind, and especially when speech is defective, as faulty articulation is a cause of failure in several of the Binet tests. Mentally defective children are specially liable to be handicapped by imperfections of speech and by their remarkable backwardness in number. In these two particulars they are, in a large proportion of cases, quite considerably behind normal children approximately on the same level in other respects. On the other hand, a few children seem to be placed too low when tested by the de Sanctis scale. In some cases this may result from poor perception of form—form perception being a necessary part of most of the tests.

On the whole the de Sanctis tests may be regarded as a distinctly useful means of making a preliminary classification of defectives, and they might be used when it is desired to group a number of defective children in as short a time as possible. The fourth test is particularly illuminative, and the manner in which a child tackles this one test and the extent of his success will be found to furnish a very significant indication of the subject's general capacity. The de Sanctis tests cannot, however, be regarded as even an imperfect substitute for the Binet tests. Indeed this could not be expected of such a small number of tests, mostly of a very concrete kind. But the tests may at some future time, in conjunction with other tests yet to be discovered and standardised, be utilised to amplify and extend the Binet-Simon scale. The present Binet-Simon scale is admitted by all experienced in its use to furnish the best preliminary survey of the capacity of defectives, but the future is sure to bring an extended and improved scale which will indicate more clearly the level attained by different functions or capacities.

VIII. *Conclusions.*

(*a*) The claim made by de Sanctis that his problems test successively higher mental functions cannot be sustained.

(*b*) Nor can his claim be sustained that his tests can differentiate between the feeble-minded and the normal. Feeble-minded persons with a mental age of nine years may pass all the tests.

(*c*) The tests are quite practical and afford a rapid means of classifying the mentally defective. They are correctly arranged in order of difficulty.

(*d*) Suggestions for the standardisation of the tests are given in the text.

(*e*) The de Sanctis tests may be utilised as substitutes for some of the tests in the Binet scale, but cannot take the place of that scale.

(*Manuscript received* 8 *October*, 1919.)

PUBLICATIONS RECENTLY RECEIVED.

Psychology from the Standpoint of a Behaviorist. By Prof. J. B. WATSON Philadelphia and London: J. B. Lippincott Co. 1919. Pp. xiv + 429.

The definition of psychology as the science of human conduct, must, if consistently adhered to, result in the identification of psychology with 'behaviourism.' This is the standpoint from which the present work,—the first text book on behaviourism by one of its foremost champions,—has been written. Ridding itself of the mental aspect, the new 'psychology' is able to ignore the relation of mind to body: it is a thorough-going 'psychology without a psyche.' "The reader will find," we are told (viii), "no discussion of consciousness and no reference to such terms as sensation, perception, attention, will, image and the like. These terms are in good repute, but I have found that I can get along without them....I frankly do not know what they mean...." It is interesting to observe the construction, the limitations and the consistency of a psychology skilfully developed on these lines.

Emotion (considered, of course, solely from the aspect of its reactions) is distinguished from instinct by two characteristics: (i) "the shock of an emotional stimulus throws the organism for the *moment at least* into a chaotic state" (196), which does not occur in instinctive actions (and surely also in such emotions as tenderness, sympathy, scorn, subjection, admiration and gratitude!); (ii) "in emotions the radius of action lies within the individual's own organism," and it involves principally the visceral muscular and the glandular tissues, "whereas in instinct the radius of action is enlarged to such an extent that the individual as a whole may make adjustments to the objects in his environment" (197) and the striped musculature of the organism is involved. Thus the movements of defence and attack are regarded by Dr Watson as instinctive, the accompanying vascular and visceral changes as emotional reactions. But the wide mental differences broadly distinguishing emotions from instincts are ruled out of account. Conation, pleasure and displeasure, coenaesthesis, etc., have no place in behaviouristic psychology; curiosity, *e.g.*, becomes "investigatory behaviour" (399).

Thinking, of course, disappears likewise. "Thought is the action of language mechanisms" (316). It is silent speech, "implicit language," "a constituent part of every adjustment process. It is not different in essence from tennis-playing, swimming, or any other overt activity except that it is hidden from ordinary observation and is more complex and at the same time more abbreviated....To make thought a bodily process like any other act" goes 'against the grain' because "historically 'thought' has always been connected with religion" (325).

Sensations are fully considered, not of course as mental processes (or products) but merely in terms of reaction. Various paraphrases are necessary to avoid slipping back into the older psychology. But now and again we seem to catch the author tripping. Thus "in a weak spectrum the region offering the highest stimulating value to the dark adapted eye is the blue-green" (96). Here he succeeds in avoiding the 'mental' term 'brightness' by substituting the rather doubtful and obscure 'behaviourist' equivalent 'highest stimulating value'; but he surely succumbs in admitting the terms 'dark' and 'blue green'! Indeed is it not obvious that ultimately all behaviour must be described in terms of the senses by which it is observed, although the study of such sensations is barred by the behaviourist from psychology? Visual illusions are mentioned as such, but only in inverted commas; in behaviourist language they must appear as the under- or over-reaction of the eye to acute or obtuse angles, etc.

We may well ask how such a 'psychology,' which is debarred from considering the forms and functions of consciousness, differs from physiology. We are told that whereas the latter deals with the reactions of individual, isolated parts of the organism, the former is concerned with the reactions of the entire organism, which is now "as it were put back together again and tested in relation to its environment as a whole" (20). For our part, we doubt whether the physiologist will be content with this limitation of his work or agree with the behaviourist as to the "entire theoretical independence of the two fields" (21).

As might be expected of the author, the book is generally excellent so far as matter is concerned, and the style is breezily American. It contains some new but not very important observations by the author on infantile emotions, instincts and reflexes, and some first-rate original drawings of the nervous system, larynx and sense organs made from special dissections. There are no references to the works of the many authors whose names occur throughout the text.

An Introduction to Social Psychology. By Dr WILLIAM MCDOUGALL. Fourteenth Edition. London: Methuen & Co. 1919. Pp. xxiv + 459. 7*s.* 6*d.*

During the eleven years' existence of this widely read book, it has been enriched by the addition of three supplementary chapters. Of these the first, on 'theories of action,' was written for the fifth edition; the second, on 'the sex instinct,' was written for the eighth edition; and the third, hardly less important than its predecessors, on 'the derived emotions,' now appears for the first time. This chapter and a fresh preface constitute the main changes in the present edition.

The new chapter serves a double purpose. It enables the author to define his position in regard to such emotions as confidence, hope, despondency, despair, regret and remorse, which had found no place in the previous editions of his book, but which have been fully discussed of late by Mr A. F. Shand in his *Foundations of Character.* Dr McDougall also subjects Mr Shand's views of these emotions to a vigorous criticism. Mr Shand calls them 'emotions of desire,' but he also calls them 'secondary emotions.' Dr McDougall invents a new term, 'derived' emotions, at first sight a needless substitution for 'secondary' but warranted in view of a different connotation and of his strong opposition to considering them as 'emotions of desire.'

The cause of this divergence of outlook can be traced, we think, mainly to their fundamentally different conceptions of impulse. According to Dr McDougall (and in accordance with general usage), impulse is essentially a form of conative tendency; according to Mr Shand it is an 'undifferentiated emotion.' Hence for Dr McDougall desire is but thwarted, unsatisfied conative tendency (429), whereas for Mr Shand it arises from the evolution of undifferentiated emotion. So, too, Mr Shand applies the term 'impulse' to self-display and self-abasement, reserving the term 'primary emotion' to pride or vanity and to humiliation or shame which he considers to be respectively evolved from them.

Dr McDougall does not apparently recognise that this is why Mr Shand comes to regard desire as an emotional system while he himself regards it as a conative system. It is not surprising that Mr Shand should term confidence, hope, anxiety, despondency and despair 'emotions of desire,' while Dr McDougall should vigorously oppose such terminology. But again it seems as if Dr McDougall's opposition is not always based on adequate apprehension of his opponent's views. For he describes (428) Mr Shand as regarding "these emotions of desire as *comparable with...the primary emotions...*, *each of these qualities of emotion...* being rooted in or dependent upon the activity of a disposition which has *its own conative tendency and proper end.*" But on reference to Mr Shand's work we find that he definitely states (468) that hope, despondency, anxiety, despair, etc. appear to have no impulses or ends of their own, that they *all have "the same end...*the end of the desire to which they belong," and that therefore "they *must be different not only from the primary emotions,* but from all those of which we have hitherto made no mention...all of which have impulses and ends of their own" (the italics are the reviewer's).

Dr McDougall protests (430, 431) against Mr Shand's view that the derived emotions (Mr Shand's emotions of desire) enter into the structure of character. Yet he admits himself (447) that the 'tempers' (hopeful, despondent, anxious, etc.) "are very important as determinants of character"! What he really opposes is the view that they are "forces comparable to the great primary emotional conative tendencies" (431), and he gives reasons for this attitude. He points out that hope is merely a new way in which desire operates when confidence begins to fail, that when hope fails it gives place to anxiety, that when anxiety fails it yields in turn to despondency, and that the latter ends in despair; despair being the turning point where we cease to have a prospective derived emotion, and begin to look back only with the retrospective derived emotion of regret.

Mr Shand includes only the prospective forms among his emotions of desire: Dr McDougall adds the retrospective emotions of regret and remorse; he also recognises sorrow as a special form of regret, namely tender regret, whereas Mr Shand regards sorrow and joy among the primary emotions. We may note, in passing, that in earlier chapters Dr McDougall's former view that sorrow is a primary emotion organized within a sentiment still stands (81, 152); he now holds, apparently, that it is a derived emotion, toned by a primary emotion, and springing from a sentiment.

Dr McDougall makes use of the term 'temper' employed by Mr Shand, but not quite in the same way. He objects to Mr Shand's 'irascible' and 'timorous tempers' on the ground they have reference to the primary emotions and should therefore be called 'dispositions' rather than 'tempers.' On the other hand he extends 'temper' to include all the derived emotions, 'hopeful,' 'anxious,' 'despondent,' etc., tempers, and also 'steadfast,' 'fickle' and other tempers. Both agree in applying the epithets 'violent,' 'gentle,' 'sorrowful' and 'joyous' to temper. Dr McDougall follows Mr Shand in showing how individual variations in temper depend on differences in intensity and in persistence of the conative tendencies. He mentions another important factor,—individual differences in susceptibility to pleasure or pain. On the basis of excess or defect of one or other of these three factors he draws an interesting picture of the way in which the various derived emotions are distinguished from one another.

Problems of Subnormality. By Dr J. E. WALLACE WALLIN. With an Introduction by Dr J. W. WITHERS. Yonkers-on-Hudson: World Book Co. 1917. Pp. xv + 485. $3.00.

The writer of this book has spent eight years in the study of abnormal children in institutions for the feeble-minded, epileptic and insane, and in public-school and University clinics. His main thesis is a protest against the rule-of-thumb use of the Binet-Simon tests for the recognition of mental deficiency. In America these tests are being widely applied by "psychological amateurs," by "young teachers just out of the normal school," untrained in research, and unfamiliar "with psychasthenic or psychopathic anomalies." The result is that statements have been there made that from 2 to 3 per cent. of all public-school children, and from 50 to 97 per cent. of persons in reformatories, penal institutions, etc., belong to the class of mentally deficient; whereas the writer would reduce these figures to from less than half to one per cent., and from 10 to 25 per cent., respectively. He maintains that the greater number of cases, graded as mentally deficient, turn out to be merely backward. But when we seek for his distinction between backwardness and mental deficiency (or feeble-mindedness), we are told that "there are types and degrees of backwardness which are quite as irremediable as feeble-mindedness, although the backward children can be brought nearer to the normal standards; the difference is essentially quantitative. There are no pathognomonic signs by which we can infallibly differentiate the one condition from the other." He believes, for instance, that dulness and backwardness, rather than mental deficiency, are responsible for crime, the former leading to active immorality, the latter only to moral weakness.

A series of forcible protests is entered against the generally adopted standard that any one above the age of 15 who fails to pass the XII- and XIII-year Binet and Simon tests is necessarily mentally defective. In the first place, Dr Wallin gives five instances of highly successful farmers and one of a successful housewife, aged between 37 and 59, all of whom "according to the Binet-Simon XIII-year standard of normality...would be feeble-minded." Secondly, he denies that social and industrial competence is dependent solely on intelligence and mental development, arguing that an individual's ability to earn a living (which is the practical test of mental deficiency to be adopted) must also depend on the community in which the individual finds himself, on the degree of permanence of his mental retardation, on the presence or absence of accompanying epilepsy, on the age at which the tests are applied, etc.

For these and other reasons Dr Wallin strongly combats the view that "the Binet scale is wonderfully accurate" and will "tell us to a nicety just where a child stands in his mental capacity," although he admits its "very high value...provided that it is legitimately used (that is, as an aid in mental diagnosis but not as an automatic mental diagnosticon)." He maintains that "it is out of the question to make any sort of satisfactory mental or educational examination of a child in ten or twelve minutes."

Such protests would have carried greater weight if the book had been written with more care, fuller knowledge and less diffuseness. The historical section is obviously incomplete; pages 123–128 contain a mass of ill-digested studies on socially or educationally abnormal individuals; the chapter on Epilepsy lacks the modern knowledge of the subject which a thorough medical training would have given; and there is far too scant recognition of the importance of psychopathic conditions in relation to the "problems of subnormality."

Nevertheless, the broader questions, dealing with the educational, social and preventive aspects of the problem, are ably discussed, and the book cannot fail to rank as a substantial contribution to the literature of the subject.

Lectures on Industrial Psychology. By BERNARD MUSCIO, M.A. Second Edition (revised). London: Routledge & Sons. 1920. Pp. iv + 300. 6*s*. 6*d*. net.

These lectures were first published in 1917 in Sydney, at the University of which they were delivered. Consequently they have not been as well known in this country as they deserve to be. It was the first British book to appear on industrial psychology, and no better work can be consulted by any one seeking a popular, level-headed and fair account of the aims and difficulties of the subject. The chief change in the present edition consists in a fuller and more satisfactory account of Scientific Management.

Human Personality and its Survival of Bodily Death. By FREDERIC W. H. MYERS. Edited and abridged by S. B. and L. H. M. London: Longmans, Green & Co. 1919. Pp. xiii + 307. 6*s*. 6*d*. net.

This well-known work, which is full of material of psychological interest, was originally published in two volumes of about 700 pages each and cost two guineas. In the present edition, the text has been very considerably condensed, and most of the appendices, which occupied about a half of each volume, have been omitted. A portrait and a biographical sketch of the author are included.

Modern Science and Materialism. By HUGH ELLIOT. London: Longmans, Green & Co. 1919. Pp. 211. 7*s*. 6*d*. net.

The six essays in this book are entitled: "The Universe as a Whole," "Matter and Energy," "Life and Consciousness," "The Fallacy of Vitalism," "Materialism" and "Idealism." Their preparation, as the writer tells us in his preface, "has involved a study of many different branches of Science and Philosophy. As I completed my survey of these branches in turn, I usually summed up the conclusions which I had gathered and published them as articles in the reviews...." The following sentences may be quoted to illustrate the author's views. "All that really exists is the material particles of the substance of the nervous system" (196). "Sensations, originally free, begin to hang together in little groups. Here we get matter" (203). "Materialism is the name for the great scheme of associated sensations which represents knowledge" (204). "Sensation is the only fundamental reality attainable" (209). "Mind and matter are equally real, but they are not made of different stuff. Mind is neural activity; matter is associated sensation....That is to say, they are both products and types of *experience*. Hence, too, they are both equally real" (210).

Schriften zur Anpassungstheorie des Empfindungsvorganges. By Prof. JULIUS PIKLER. 1919. Leipzic: J. A. Barth. Part 1. Hypothesenfreie Theorie der Gegenfarben. Pp. viii + 104. M. 8. Part 2. Theorie der Konsonanz und Dissonanz. Pp. 34. M 2.

Professor Pikler's colour theory forms part of a general 'adjustment' theory for all sensations. The different grades of tone-free colours are referred to a mean normal condition. A stimulus is not to be regarded as an excitation in a passive organism, but as inducing a reactive adjustment in the organism. The author propounds his explanation of simultaneous and successive contrast, spatial induction, etc., and enters into lengthy criticism of Hering's theory. The more difficult explanation of the toned colours is based on the black-white series. Blue and yellow correspond to black and white respectively; red and green to mean grey. Ethical and aesthetic analogies are freely indulged in. "Gelb verhält sich zu Weiss, wie siegreiche Tugend zu kampfloser Unschuld" (!). The theory is highly speculative, and does not seem calculated to initiate research. It is paradoxical to describe it as "independent of hypotheses."

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PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETING.

November 15, 1919.	A Study of Nyctopsis, by LL. WYNN JONES.
	A Psychological Study of God, by STANTON COIT.
January 31, 1920.	Observations on the Suggestibility of School Children, by F. AVELING.
	Some Mental Effects of Alcohol and other Drugs, by MAY SMITH (in collaboration with W. McDOUGALL)

SECTIONAL MEETINGS.

(a) *Educational Section.*

October 15, 1919	Psycho-analysis, Suggestion and Education, by WILLIAM BROWN.
November 26, 1919.	The Psychology of Child Education, by MARIA MONTESSORI (Joint Meeting with Medical Section).
December 18, 1919.	The New Psychology and the New Teaching, by KENNETH RICHMOND.
January 5, 1920.	The Development of Mental Tests, by P. B. BALLARD.

(b) *Industrial Section.*

October 22, 1919.	What Industry expects from Psychology, by P. G. JACKSON.
November 28, 1919.	The Present Attitude of the Workers to Industrial Psychology, by S. S. BRIERLEY.
December 19, 1919.	The Causation of Accidents, by E. L. COLLIS.
January 7, 1920.	A National System of Education, by J. C. M. GARNETT.

(c) *Medical Section.*

October 29, 1919.	Suggestion and Suggestibility, by E. PRIDEAUX.
November 26, 1919.	The Psychology of Child Education, by MARIA MONTESSORI (Joint Meeting with Educational Section).
December 17, 1919.	Some Physical Signs of Unconscious Wishes and Mental Conflict, by W. H. B. STODDART.
January 21, 1920.	Recent Advances in Psycho-analysis, by ERNEST JONES.

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NOTE ON PROFESSOR J. LAIRD'S TREATMENT OF SENSE PRESENTATIONS

BY J. E. TURNER.

PROFESSOR LAIRD'S article[1] is concerned with the origin of the whole of knowledge, which at every stage however advanced maintains its connexion with our sense experience of the material world. His able treatment appears to furnish further evidence that any theoretical isolation of sense data (as such) from the total apprehended content is much more difficult than is generally admitted. This becomes clear, I think, if we recognise that the original mind content—the "vague voluminous sensory mass" (p. 278)—even though transformed or transcended, always persists as a constituent of experience, and further that the development from this of the higher forms of knowledge is essentially continuous. It is therefore impossible ever to demarcate absolutely, within the total apprehended content, those elements, and those alone, which are contributed by sense, for these have become so interwoven with and penetrated by the products of higher activities that their isolation could be effected only by the abolition of all conscious processes except those initiated by the sense organs alone. This again at once raises the question—What exactly is the 'sense organ'? Plainly it is one thing for physiology, and quite another for psychology; for the latter cannot disregard the connexion between its nerve structures and the cerebral centres; these for psychology all function together, so that it becomes impossible to formulate any perfectly general characters of 'sense content'; as Professor Laird expresses it, perceived content "differs for different subjects according to the character of their several minds" (p. 264); and hence arises the difficulty attending its description to which he alludes on p. 266, and which is really due to the impossibility of expressing in general terms characteristics which are essentially individual and exclusively peculiar.

All that appears feasible therefore is the merely provisional ascription of average characters to a theoretically average experience, leaving

[1] This *Journal*, 1919, IX. 261.

room in the case of every individual for very wide limits of departure from this average; in the same way *e.g.* as the definition of a biological species aims at no more than an expression of average characters, from which subspecies, varieties and individuals themselves always differ; and any such interpretation of sense data (meaning by this term "the objects with which we are directly acquainted in sensation and perception" (p. 265)) must be governed by two main principles. In the first place, since developed knowledge always has as its direct object the real world, and since this knowledge has arisen by continuous and insensible gradations from the primal 'sensory mass,' every perceived content therefore must be no other than a portion or aspect of the real world; and secondly, its apprehension must be directly dependent in the main on the activity of sense organs.

I am not sure that Professor Laird admits this first principle to be more than either a mere possibility, or a matter of indifference to psychology,—"appearances may in all cases be part of the objective world"; they are "facts which the psychologist is bound to take at their face value" (pp. 264, 265)[1]. True; but then what is their "face value"? Exactly as the face value of gold currency cannot be severed by the economist from the underlying world of commerce, so the face value of sense data includes their identity with that part of the real material world which is apprehended by means of the sense organs; it is from this real world that they are always delimited by the psychologist in the first instance in order to constitute the sphere (really more or less arbitrary) of his investigations; and to exclude this fact as to their origin from their face value is to constitute them at once into a fictitious category which it then becomes impossible to describe, classify or relate, simply because all the requisite terms derive their significance from the real world which has however, been made inaccessible by its prior exclusion.

That this is true becomes obvious, I think, when we consider Professor Laird's ascription of meaning to sense data as part of their face value—"meaning belongs to the presentation *in precisely the same sense* as the colour and the shape" (p. 267). The italics here are my own, and are intended to bring out the importance of the second basal principle determining our consideration of sense data. For if we admit that sense presentations as such have meaning, still this cannot be *in the same sense* as shape and colour; for while our consciousness of these latter depends

[1] Cf. also *Problems of the Self*, 28, 29, 55.

primarily and directly on the sense organs, the consciousness of meaning, even if it can truly be said to depend on sense organs at all, is always indirect and derivative. The sense organs, *i.e.*, can never in themselves alone arouse any consciousness of meaning, which is always the result of mental processes higher than sense, even though they operate on content apprehended through sense[1]. The primary 'sensory mass' from which all experience originates has never, *as sensory*, any true meaning whatever; meaning can arise only through the free play of recollection and imagination becoming co-ordinated into systematic memory and conception. But, though meaning and the purely sensed content differ thus widely in their modes of origin, still they become so indissolubly combined that all perceived content may be said (but from this standpoint only) to have inherent meaning as part of its face value; perception indeed may be regarded as the conferring of meaning upon sensed content; for until meaning arises, nothing is perceived, though somewhat may be sensed; on the other hand if sensed content be absent, we may still retain meaning, but not in perception.

But if meaning is a legitimate element in all presentations, then its limits in any given instance are purely arbitrary; we cannot *i.e.* lay down in advance any general "limitation to presented meanings" which, as a distinct class or category, are never transcended. Certainly the variety and vividness of sensed content cause a natural or unconscious tendency to exclude from our analysis of presentations as much content as possible other than the vivid sense content itself; but this procedure has no foundation whatever in principle, and is but the expression of an economy of mental energy—we reduce deliberate thinking as much as possible, and thus respond to sense stimuli as automatically as we can. Such meaning as any presentation exhibits always appears therefore to be as inherent therein as its purely sensible elements; but at the same time this can attain a very wide range, and a modicum of purely sensed content may be submerged as it were in a mass of meaning to such an extent that the usual proportions are quite reversed, and it then becomes a difficult problem to determine how much of the total apprehended content may properly be called a presentation. Thus when the faintest of heart beats indicates life and hope—when a slight precipitate determines innocence or guilt—how much (if we admit meaning at all) is presentation, and how much not? Professor Laird again instances the sparks at a firework display as having an unques-

[1] Cf. *Problems of the Self*, 53 *n.*: "Probably there is no object sensed which is not also judged."

tionable presented meaning, in so far as "relation to other parts of the presented field" is apprehended (p. 267). But let us take the case of a signaller receiving a code message by means of faint and distant flashes; how much of the total meaning is here presented? and if not the whole, what general principle governs our limitation? Such a question is obviously fundamental to the theory of the nature of knowledge, and to the rôle therein of perception, conception and thought. It is true that "cognition is the guide of all movement" (p. 275); but our problem is concerned with the analysis of cognition itself, and the determination, so far as is possible in general terms, of the elements contributed to it by the different grades of conscious activity; so that this statement leaves the origin of meaning an open question.

Again, if sense data as such include meaning, it is difficult to understand how they can be other than continuants, at least to some degree[1]. It is true that "whatever is perceived is felt to be fragmentary" (p. 279), but a content though fragmentary may still be continuous or enduring; but if further it contains inherent meaning, it must *necessarily* possess some measure of continuity. For meaning consists essentially in the transcendence of the merely given—of the merely momentary; the mind cannot grasp a meaning which has no persistence or continuance whatever. But in order to constitute a sense presentation, it is obvious that some sensed content must also exist together with the meaning, for otherwise nothing is perceived; and since every presentation (as such) is for the mind an individual whole, it must as a whole participate in the continuance possessed by its inherent meaning. And when Professor Laird comes to consider temporal meaning, he abandons his principle of absolute discontinuance (p. 268); but here, as with meaning, *c'est le premier pas qui coute*; and if presentations be admitted to possess any degree, however slight, of continuance, there is then no difficulty in principle in regarding them as sharing, *mutatis mutandis*, in the real continuity of the material world, of which they themselves are indeed parts.

Such a view appears to simplify very much the whole problem of presentations; for we may supplement the realism which Professor Laird advocates[2] by regarding sense data as always fragments or (perhaps better) aspects of the physical world, determined (for psychology) to be sense data by those conditions under which they are always

[1] "Sense presentations are not continuants...our descriptions of presentations are not meant to convey the implication that these are even potentially continuants" (p. 266).

[2] *Mind*, October, 1919.

and necessarily presented to a consciousness debarred by its finitude from an immediate apprehension of the whole of reality. Nevertheless it is always reality which confronts consciousness, from the primitive undifferentiated content we call 'sensory,' to the perceived world of the developed intelligence in which perception, itself always fragmentary, is supplemented by conception and thought. Such a method of treatment plainly permits, and even anticipates, the existence of illusion, hallucination and error, for it regards knowledge as being, certainly in actuality and possibly even in principle, incomplete and defective.

(*Manuscript received* 4 *January*, 1920.)

REPLY TO MR J. E. TURNER'S NOTE

By JOHN LAIRD.

Mr Turner seems to be undecided upon the extent of our disagreement and so am I. I certainly did not set out to deny the truth of his conclusion. On the other hand I tried to keep very closely to the standpoint of psychology, and I deliberately refrained from debating the whole problem of sense perception. Mr Turner's argument, as it seems to me, moves too rapidly, and overlooks certain most important questions of psychological fact.

When we speak of the 'physical world' we may mean little or much. Sense data, I suppose, are physical. So are the 'things' of common sense; so are scientific objects like atoms and electrons. It is clear, however, that scientific objects are not perceived at all, and that it is very doubtful whether the 'things' of common sense are perceived directly. The problem of the nature of the physical world, and of the truth of our acquaintance with it, is metaphysical, I think, not psychological. What psychology has to do is to describe precisely what *appears* to A or B or C when they see, or hear, or taste; and this description, I maintain, should not prejudge any metaphysical conclusion whatsoever. We touch, and smell, and look. So do children, and so do other animals. Surely, then, it is essential to find out, with the most scrupulous accuracy, what precisely is presented to normal and abnormal, developed and rudimentary minds. Psychology, indeed, differs from physics in its treatment of these questions precisely on account of its carefulness in this particular; and there is nothing arbitrary in psychological procedure. To describe the differences between sight-space and touch-space; or between the sensitivity of the finger-tips and of the small of the back; or, again, the effect of attention upon sensory appearances, is to institute a scientific enquiry into an important matter of fact.

So I think it is a great mistake not to take presentations seriously enough, and to speak roughly and generally of the physical world when we ought to speak minutely and precisely of the facts directly perceived; and I am afraid I cannot agree with some of Mr Turner's further remarks.

I should like to affirm, unrepentantly and with all the emphasis of Mr Turner's italics, that certain meanings belong to sense presentations *in precisely the same sense* as colour or shape. I am at a loss to understand how Mr Turner (in view of his earlier remarks) can possibly *know* that colour depends directly on the sense-organs and that meaning does not; and I should doubt whether all meaning is acquired in individual experience. The need for brevity, I am afraid, must be my excuse for dogmatism, but I should very much like to know whether Mr Turner would be prepared to sustain his position after reconsidering Stout's argument in his *Manual*, pp. 342 *sqq.*, and especially p. 349. Even, however, if all meaning were acquired, that, on my view, would not affect the issue. The history of my acquirements may be what you will, but I certainly perceive meaningful presentations *now*, and this meaning must be included in any accurate account of the facts of direct perception.

Again, I do not believe that the limits of perceptible meaning are wholly arbitrary. Meaning varies, to be sure. Montezuma perceived comparatively little in the way of meaning when he first saw the horses that the Spaniards brought. Later on, a horse was as meaningful an animal to him as to most of us. And the 'fringe' of meaning is always obscure. On the other hand, there are clear cases of perceived meaning, and equally clear cases of meaning that is not perceived at all. The sphericity of a golf-ball, for example, cannot be seen with the eyes all at once, but it is a perceived visual meaning all the same. For there is 'complication' in this matter (granting the possibility of mistakes). On the other hand, inferences concerning what the golf-ball would do *if* I hit it or *if* Mitchell hit it may be part of the meaning of the golf-ball, but are certainly not perceived meanings.

Mr Turner's further argument that sense data must *necessarily* possess some measure of continuity if they contain inherent meaning seems to me quite confused. There is all the difference in the world between meaning and what is meant. The black marks e-t-e-r-n-i-t-y, for example, signify everlastingness, but the marks themselves are perishing. My view is that our interrupted glimpses of the everlasting hills contain indications of uninterrupted, abiding existence; for these glimpses, I think, are *sign-facts*, and are directly perceived as such. But this abiding existence is certainly not presented to us in sense. It is unusual, indeed, for anything meant (or signified) to be presented as well as the sign or meaning which signifies it. To choose Mr Turner's illustration, a shilling may indicate a whole set of commercial conditions, but, in the general case, the laws of economics are not before my mind when I look at a shilling, although the shilling, for me, is not merely a silvery disc, but has some vague twelve-penny sort of meaning even on the face of it.

And it is irrelevant, I think, to point out, that sense data have a certain evanescent continuance. Anything directly perceived, to be sure, endures as long as the time taken in perceiving it, but my problem concerned the manner of our acquaintance with uninterrupted continuants through interrupted presentations, and I maintained that continuants are not perceived and that what we do perceive are perishing presentations which signify continuants. I did not deny that these perishing presentations are really parts of the continuants themselves, and I believe that to be true in fact, although the problem goes beyond psychology. None the less, we are bound to distinguish between our presentations and the continuants. If I look at a candlestick, for example, I call it a steady solid thing on the strength of my perception. My presentations, however, are not steady at all. They fluctuate most appreciably, however earnestly I gaze at the candlestick.

All these points I tried to bring together in my article in this *Journal*.

(*Manuscript received* 13 *January*, 1920.)

A PERFORMANCE TEST UNDER INDUSTRIAL CONDITIONS

By S. WYATT and H. C. WESTON.

(*A Report to the Industrial Fatigue Research Board.*)

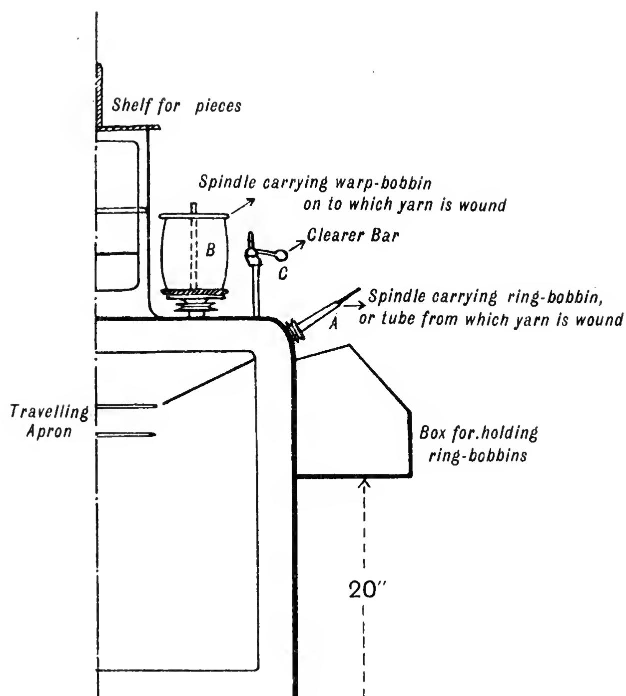
1. Introduction.

The question of a suitable test of fatigue is occupying the minds of many people at the present time. It is generally agreed that the tests already devised and the methods employed are defective in many respects, and cannot be satisfactorily applied to industrial conditions. Those of a psychological nature are usually so different in method and material from the fatiguing industrial processes, that the abrupt change in conditions caused by the application of such a test either during or at the end of a working day may enable the worker to perform the test with renewed interest and energy, and consequently give rise to entirely misleading results. In any case, there must be a considerable number of preliminary trials in order to overcome the effects of unfamiliarity and practice. The existing physiological tests appear to be equally unsuitable and unreliable; those which seem to give the best results being more suitable for the laboratory than for the factory.

2. Description of the Test.

In the course of an investigation into the Cotton Industry carried out under the auspices of the Industrial Fatigue Research Board, it was thought that the essential features of the process under observation might be utilised to form the material for a fatigue test. The process under investigation was that of bobbin-winding in a mill situated in south-east Lancashire. The operation of bobbin-winding, as conducted in this mill, consists in transferring 'ring-yarn' of various lengths from the paper tubes, upon which it has been spun, known as 'ring-bobbins,' on to 'warp-bobbins' large enough to hold an average length of about 20,000 yards. Each operative has charge of one side of a 'winding frame' which contains 100 pairs of spindles carrying the ring-bobbins and the warp-bobbins respectively; and her duties consist chiefly of 'piecing' (*i.e.* connecting up the ends of yarn from the ring to the warp-bobbin) by means of an ingenious instrument (strapped round the palm of the left hand) which ties the ends of the yarn and cuts off the ends close to the knot when the winder presses a small thumb lever.

A section of a winding-frame is shown in the following diagram:



The winder gathers the end of the yarn on *A* round the fingers of her right hand, stops the rotation of the bobbin *B* with her left hand, rotates the bobbin in the direction opposite to the rotation of the spindle in order to find the other end of the yarn, and partly lifts the bobbin from the spindle. She then carries the right hand to the bobbin and picks up the yarn end, thus having both ends of the yarn in her right hand. With a circular sweeping movement of the right hand she brings the yarn ends over the knotting instrument, and presses and raises the knotter lever with the left thumb. Lastly she places the yarn in the clearer bar *C* and retards the speed of the bobbin before finally releasing it. These operations constitute the typical movements involved in bobbin-winding, and are repeated several thousand times in the course of a week. It was thought that, if suitably regulated and controlled, they would form the material for a test which would practically represent a cross-section of the actual industrial process and involve the use of those parts of the organism which were normally liable to become fatigued.

A winding frame was accordingly set aside for the purpose of this test, and the partially filled warp-bobbins and the ring-bobbins were placed ready on their respective spindles, with the ends of the yarn from the latter in a visible and uniform position in each case. The winder had then to perform the operations described above. A preliminary test performed by nine winders daily for a week was first given, but it was afterwards decided to apply the test to four winders and to use 50 spindles instead of 100 as originally chosen. The tests were given from 9.30 to 10.0 in the morning and from 3.30 to 4.0 in the afternoon. Thus at the time of the morning test the winders would probably be working at a high level of efficiency. The test was given at 3.30 in the afternoon because tea was served about 4 o'clock and it was thought that the stimulating effect of tea would temporarily neutralise the effect of any fatigue which might exist.

In order to carry out the test, each winder came from her own winding-frame to the test frame and pieced the 50 pairs of ends as quickly as possible. The time required to perform the task was taken by means of a stop-watch recording tenths of a second, and the times taken to knot the first and second twenty-five ends were separately noted in order to obtain an indication of the relative effects of practice and fatigue during the test itself. In addition, the temperature and humidity in the vicinity of the frame at the time of the test were also recorded, together with any special circumstances likely to affect the performance of the winder.

In order to discover the presence of any unusual subjective states which might influence the results of these tests, the winders were supplied with diaries containing questions framed with this object in view. This procedure was found to give very satisfactory results, and there is no reason to doubt the reliability of the replies given to the questions set. Without exception, the winders showed a genuine desire to answer the questions satisfactorily, and the information thus obtained was undoubtedly of considerable value in connexion with the application of the test and the interpretation of the results. These diaries were supplied to the winders weekly and, in order to prevent the possibility of any misapprehension arising as to the object for which the information was required, they contained the following preface.

Please read this before filling up the Diary.

The information you are asked to give in this diary is required for the purpose of enabling the facts disclosed by the test you are performing to be correctly interpreted. There is no desire whatever to pry into your private life unnecessarily, and you are, therefore, not asked to give, for example, any details of how your evenings are spent, but merely to state briefly whether you stayed at home, reading, or otherwise employed, or whether you went for a walk, etc.

You are, of course, under no compulsion to fill up the diary, either wholly, or in part, but as it is hoped that the investigation now being carried out will ultimately benefit you, your assistance and co-operation is invited and will be greatly appreciated.

All the information you give will be treated by the investigator as strictly confidential.

Please write your name on the cover of the diary and hand the latter to the investigator when completed.

The questions contained in the diaries were as follows, a typical set of answers being also given.

Monday, July 14th, 1919.

1.	Time of rising?	6.30 a.m.
2.	Breakfast?	Bread, butter, and cake.
3.	State how you feel this morning.	Well.
4.	Did you take food to the Mill?	No.
	12.15 to 1.15—dinner time.	
5.	Did you have dinner at home or at the Mill?	At the Mill.
6.	State how you feel this afternoon.	Well.
7.	Do you think you pieced the 50 ends as quickly as you did this morning?	Yes.
8.	Did you have tea at the Mill?	Yes.
9.	How did you feel when you left the Mill?	Fairly tired.
10.	How did you spend your evening?	Sewing.
11.	Time of going to bed?	10 p.m.

These questions were suitably modified in the case of Saturdays and Sundays. The importance of these diaries lies, of course, in the fact that any unusual circumstances, such as ill-health or worry, are disclosed, and may account for any unusual changes observed in the reaction-time of the individual.

During the first week of the test the working hours were:

Monday to Friday:	6.0 a.m. to	8.0 a.m.	1st spell
	8.0 ,, ,,	8.30 ,,	Breakfast
	8.30 ,, ,,	12.30 p.m.	2nd spell
	12.30 p.m. ,,	1.30 ,,	Dinner
	1.30 ,, ,,	5.30 ,,	3rd spell

Owing, however, to shortage of work, only 9 hours per day were worked during this week, the operatives going home about 4.30 p.m.

From Monday, June 23rd, till Saturday, June 28th, a strike prevailed in the cotton industry, but on Monday, June 30th, the operatives resumed work according to the following distribution of hours:

Monday to Friday:	7.45 a.m. to	12.15 p.m.	1st spell
	12.15 p.m. to	1.15 ,,	Dinner
	1.15 ,, ,,	5.30 ,,	2nd spell
Saturday:	7.45 a.m. to	12.0 noon.	

Thus, as regards the actual number of hours worked per day, the new conditions were only slightly different from those which prevailed during the first week of the test.

3. RESULTS OBTAINED.

(a) *Daily and weekly variations.*

The times taken by each winder to piece the 50 ends are given on the following page in Table I.

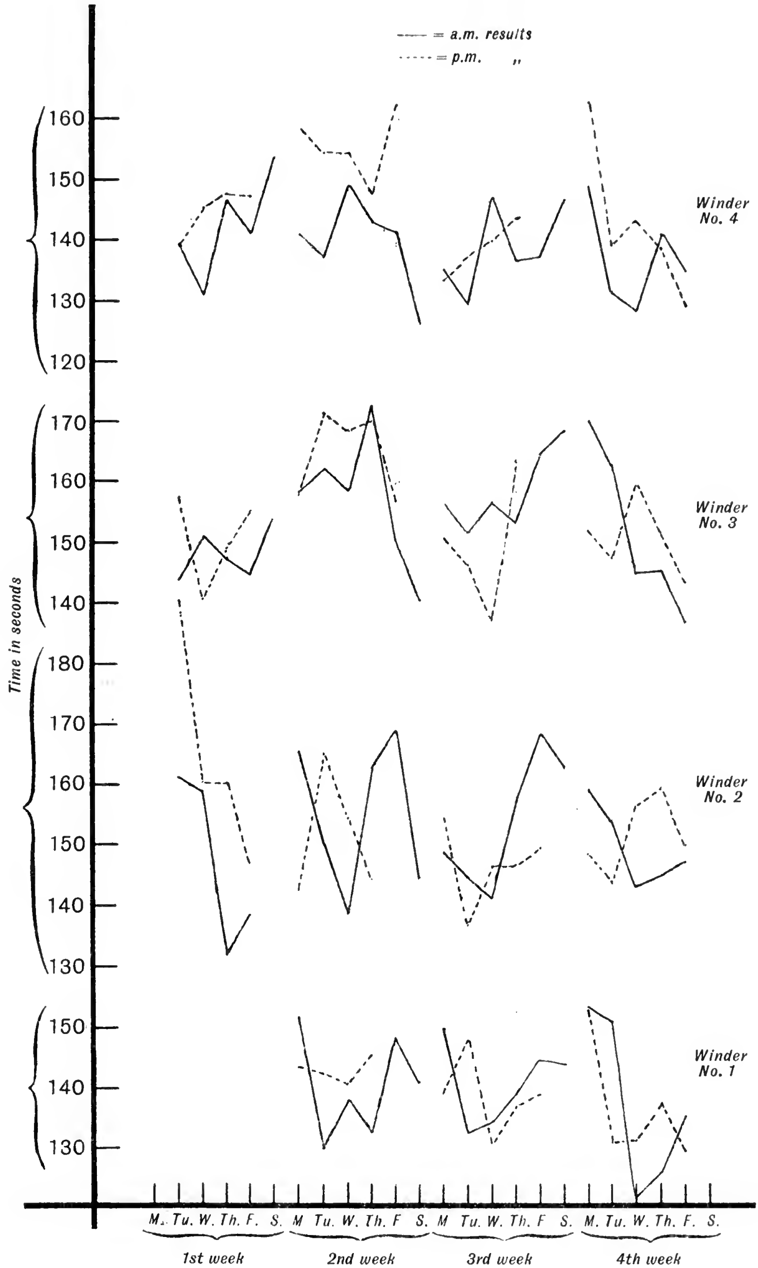
Averages based upon the results of only two winders are enclosed in brackets since they cannot be justifiably compared with the other averages.

The general tendencies of the above results are shown more clearly in Fig. 1.

The most striking feature of the curves of Fig. 1 is the absence of uniformity. The morning and afternoon curves cross each other in the most irregular manner, and in many cases lend no support to the theoretical expectation that the afternoon times would be longer than those of the morning, if fatigue were produced by the winding operations and the test were able to indicate the extent of this fatigue. A certain amount of similarity is noticeable in the morning curves of the individual

Table I.

	Winder	Monday a.m.	Monday p.m.	Tuesday a.m.	Tuesday p.m.	Wednesday a.m.	Wednesday p.m.	Thursday a.m.	Thursday p.m.	Friday a.m.	Friday p.m.	Sat. a.m.	Weekly Average a.m.	Weekly Average p.m.	Weekly Average daily
1st week	No. 1	—	—	134·8	141·5	132·8	off	off	off	off	off	off	(133·8)	(141·5)	(137·6)
	,, 2	—	—	161·5	193·2	159·0	160·5	132·0	160·3	138·4	147·1	off	147·7	165·3	156·5
	,, 3	—	—	144·0	157·5	151·0	140·7	147·9	149·1	145·1	155·2	154·0	148·4	150·6	149·5
	,, 4	—	—	139·4	139·6	131·3	145·4	146·2	147·9	141·2	147·1	153·8	142·4	145·0	143·7
	Avg.	—	—	144·9	157·9	143·5	148·9	142·0	152·4	141·6	149·8	(153·9)	144·5	152·7	148·2
2nd week	No. 1	151·9	143·5	130·0	142·1	138·0	138·6	132·4	145·5	148·8	off	141·0	140·3	142·4	141·1
	,, 2	165·9	142·6	150·5	165·3	138·8	154·2	162·9	144·1	169·4	off	144·4	155·3	151·5	153·3
	,, 3	158·1	157·9	162·1	171·6	158·4	168·4	172·5	170·0	150·1	156·5	140·5	156·9	164·9	160·9
	,, 4	140·8	158·4	137·2	154·1	147·8	154·1	142·6	147·5	141·2	162·0	126·1	139·3	155·2	147·2
	Avg.	154·2	150·6	145·0	158·3	145·8	153·8	152·6	151·8	152·4	(159·3)	138·0	148·0	154·2	151·1
3rd week	No. 1	150·0	139·2	132·6	148·0	134·5	130·7	139·0	136·9	144·5	139·1	144·0	140·7	138·8	139·7
	,, 2	148·6	155·0	144·7	137·0	141·3	146·5	157·1	146·5	168·2	149·4	163·3	153·9	146·9	150·4
	,, 3	156·1	150·6	151·9	146·2	156·6	137·7	153·3	163·6	164·5	off	168·1	158·4	149·5	153·9
	,, 4	135·0	133·6	129·7	136·9	146·3	139·9	136·3	143·4	137·2	off	146·4	138·5	138·5	138·5
	Avg.	147·4	144·6	139·7	142·0	144·7	138·7	146·4	147·6	153·6	(144·5)	155·1	147·9	143·3	145·6
4th week	No. 1	153·5	152·5	151·2	131·0	121·9	131·3	126·0	137·8	135·3	129·5	—	137·6	136·4	137·0
	,, 2	159·3	148·3	154·0	144·0	143·1	156·2	144·7	159·5	147·2	150·0	—	149·7	151·6	150·6
	,, 3	170·0	152·0	162·5	147·9	145·0	159·1	145·4	151·7	137·0	143·5	—	152·0	150·8	151·4
	,, 4	148·9	162·4	131·5	139·0	128·1	143·0	140·9	138·6	134·8	129·0	—	136·8	142·4	139·6
	Avg.	157·9	153·8	149·8	140·5	134·5	147·4	139·0	146·9	138·6	138·0	—	144·0	145·3	144·6
Average		153·2	149·7	144·8	149·7	142·1	147·2	145·0	149·6	146·5	(147·9)	(149·0)	146·1	148·9	147·5
Daily Avg.		151·4		147·2		144·6		147·3		(147·2)		(149·0)			



—— = a.m. results
- - - - = p.m. ,,
Time in seconds
160
150
140
130
120
170
160
150
140
180
170
160
150
140
130
150
140
130
Winder No. 4
Winder No. 3
Winder No. 2
Winder No. 1
M. Tu. W. Th. F. S. M Tu. W. Th. F S. M Tu. W. Th. F S. M. Tu. W. Th. F. S.
1st week
2nd week
3rd week
4th week

Fig. 1.

winders, particularly in the case of winder No. 2, but the greatest amount of uniformity in the results of all the winders is to be observed in the fourth week.

The afternoon curves are extraordinarily irregular, and defy all attempts to discover any other common feature. *It would accordingly appear that individual variability increases during the course of the day's work, but becomes less as the subjects become more accustomed to the test conditions.*

The irregularity in the afternoon results may be due to individual differences in susceptibility to fatigue and to incentives which become effective towards the end of the afternoon period, but a more extensive investigation is necessary to determine the existence and nature of such factors. The greater irregularity of the results during the first three weeks of the test is probably due to variation in adaptability to the test conditions.

There is a tendency, especially during the last week of the test, for the Wednesday morning results to be lower than those obtained on the other days of the week. This is probably due to the fact that Wednesday morning marks the close of the winders' week as far as the payment of wages is concerned, and work done after this period is reckoned in the output for the following week. There is consequently a desire on Wednesday morning to work at high pressure in order that the current week's wages may not be adversely affected. It is quite possible that this weekly spurt is unconsciously transferred to the knotting test, or there may be a conscious desire to perform the knotting test as quickly as possible in order to return to the ordinary routine work. It will be seen, however, that the afternoon results on Wednesday of the third week are lower in the case of three winders, than the morning results on the same day. This is probably due to the fact that this Wednesday morning was exceptionally dull and depressing while the afternoon was bright and fine. The afternoon results on the following Wednesday show a marked rise and are higher than the morning results for that day. The only noticeable feature of an unusual nature which could conceivably give rise to such results was the fact that the winders were waiting for yarn for an hour prior to the performance of the test. The waiting period would probably cause a loss of swing and feelings of discontent and dissatisfaction since it represents decreased output and smaller wages.

Occasionally the time taken by a winder to perform the test would be prolonged because of the presence of an unavoidable and inevitable factor. Thus winder No. 2 was delayed 10 seconds on the afternoon of

Thursday in the fourth week because the yarn became entangled in her knotter. Similarly winder No. 4 had some difficulty in finding the end of the yarn on the bobbin during the second half of the test on the morning of the last Monday. Although the apparent duration of the delay was noted and the necessary correction applied to the results, it is quite possible that the entire performance of that particular test would be influenced by such disturbing factors. Thus many of the individual deviations and discrepancies are due to variations in the objective conditions, but a more complete discussion of the factors which may be responsible for the existence of these irregularities will be deferred to a later part of this paper.

The results give some interesting indications of the effects of practice. The weekly average of all the tests during the first week is 148·2 seconds. In the second week this value increases to 151·1 seconds, but in the third and fourth weeks it falls to 145·6 and 144·6 seconds respectively. Thus from the second to the fourth week there is a decrease of 4 % in the time taken to perform the test. The lower value of the results of the first week compared with those of the second is partly due to the omission of Monday from the first week's results. The Monday results are usually higher than those obtained on other days and consequently the weekly average for the first week will be favourably affected by the exclusion of this day. It will also be remembered that the mill was closed for a week between the first and second weeks of the test and when work was resumed the hours of work had been changed. Loss of swing during the holiday together with changed conditions of work would tend to increase the test time during the second week of the test.

An interesting individual example of the effect of practice is to be found in the first week's results of winder No. 2. In this particular case the frame on which the test was performed was a little higher than the one on which this winder usually worked and this small variation from the usual conditions was responsible for the well-marked practice effect during the first week.

In general, the results point to the interesting and significant fact that the least variations from the usual conditions of work will interfere with its normal performance, and that even in the case of a test which so closely approximates to working conditions as the one described in this paper the effects of practice are still evident after the test has been in progress three or four weeks.

As the curves become more uniform and similar during the progress of the test, practice appears to have a levelling effect upon the results.

Table II.

		Monday				Tuesday				Wednesday				Thursday				Friday				Saturday	
		a.m.		p.m.		a.m.		p.m.		a.m.		p.m.		a.m.		p.m.		a.m.		p.m.		a.m.	
	Winder	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25
1st week	No. 1	—	—	—	—	69·0	65·8	70·0	71·5	65·0	67·8	off	off	off	off	off	off	off	off	off	off	off	off
	,, 2	—	—	—	—	82·0	79·5	108·0	85·2	78·0	71·0	83·0	77·5	69·0	63·0	83·0	77·3	65·5	72·9	73·0	74·1	off	off
	,, 3	—	—	—	—	74·0	70·0	84·0	73·5	70·0	81·0	67·5	73·2	75·0	72·9	75·5	73·6	75·8	69·3	84·9	70·3	82·5	71·5
	,, 4	—	—	—	—	73·0	66·4	70·0	69·6	70·0	61·3	73·0	72·4	69·2	77·0	70·1	77·8	68·9	72·3	74·5	72·6	73·6	80·2
	Total	—	—	—	—	298·0	281·7	332·0	299·8	285·0	281·1	223·5	223·1	213·2	212·9	228·6	228·7	210·2	214·5	232·4	217·0	156·1	151·7
	Avg.	—	—	—	—	74·5	70·4	83·0	75·0	70·8	70·3	74·5	74·4	71·1	71·0	76·2	76·2	70·1	71·5	77·5	72·3	78·1	75·9
2nd week	No. 1	74·2	77·7	69·0	74·5	69·0	61·0	73·0	69·1	75·9	62·1	70·4	68·2	67·5	64·9	76·1	69·4	65·0	73·8	off	off	69·2	71·8
	,, 2	85·4	80·9	75·4	67·2	73·0	77·5	80·0	85·3	70·1	68·7	79·4	74·8	80·5	82·9	69·6	74·5	81·0	78·4	off	off	75·0	69·4
	,, 3	76·1	72·0	85·7	72·2	87·5	74·6	87·6	74·0	83·4	75·0	86·7	81·7	89·6	82·9	85·0	85·0	89·0	61·0	73·5	83·0	69·7	70·8
	,, 4	69·1	71·7	81·0	77·4	68·9	68·3	72·4	71·7	75·7	72·1	77·1	77·0	70·8	71·8	70·5	77·0	80·0	61·2	81·6	77·4	61·5	64·6
	Total	304·8	302·3	311·1	291·3	298·4	281·4	313·0	300·1	305·1	277·9	313·6	301·7	308·4	302·5	301·2	305·9	315·0	274·4	155·1	160·4	275·4	276·6
	Avg.	76·2	75·6	77·8	72·8	74·6	70·4	78·3	75·0	76·3	69·5	78·4	75·4	77·1	75·6	75·3	76·5	78·8	68·6	77·6	80·2	68·9	69·2
3rd week	No. 1	71·5	78·5	68·5	70·7	60·4	68·6	73·6	74·4	64·6	69·9	65·4	65·3	67·5	71·5	65·0	71·9	73·5	71·0	68·7	70·4	72·0	72·0
	,, 2	78·9	69·7	78·0	77·0	67·0	77·7	72·0	65·0	67·4	73·9	65·2	81·3	86·0	71·1	74·5	72·0	83·5	84·7	68·5	80·9	78·2	83·1
	,, 3	85·5	70·6	73·6	77·0	75·6	76·3	70·0	76·2	77·2	79·4	71·2	66·5	80·4	72·9	79·0	84·6	87·6	76·9	off	off	79·0	89·1
	,, 4	68·4	66·6	64·8	68·8	67·5	62·2	71·8	65·1	68·2	78·1	73·4	66·5	70·5	65·8	73·0	70·4	65·1	72·1	off	off	72·0	74·4
	Total	304·3	285·4	284·9	293·5	270·5	284·8	287·4	280·7	277·4	301·3	275·2	279·6	304·4	281·3	291·5	298·9	309·7	304·7	137·2	151·3	301·2	318·6
	Avg.	76·1	71·4	71·2	73·4	67·6	71·2	71·9	70·2	69·4	75·3	68·8	69·9	76·1	70·3	72·9	74·7	77·4	76·2	68·6	75·7	75·3	79·7
4th week	No. 1	70·2	83·3	81·0	71·5	73·0	78·2	63·2	67·8	61·2	60·7	66·0	65·3	61·0	65·0	68·5	69·3	62·3	73·0	62·9	66·6	—	—
	,, 2	82·0	71·3	66·1	82·2	71·1	82·9	65·2	78·8	68·0	75·1	72·0	84·2	64·0	80·7	76·0	83·5	77·0	70·2	80·0	70·0	—	—
	,, 3	83·2	86·8	74·3	77·7	84·5	78·0	75·0	72·9	65·0	80·0	77·8	81·3	74·5	70·9	79·5	72·2	67·4	69·6	72·8	70·7	—	—
	,, 4	75·9	73·0	73·6	78·8	63·9	67·6	64·5	74·5	63·0	65·1	72·0	71·0	70·5	69·5	70·2	68·4	64·6	70·2	62·0	67·0	—	—
	Total	311·3	314·4	295·0	310·2	292·5	306·7	267·9	294·0	257·2	280·9	287·8	301·8	270·0	286·1	294·2	293·4	271·3	283·0	279·7	274·3	—	—
	Avg.	77·8	78·6	73·8	77·6	73·1	76·7	67·0	73·5	64·3	70·2	72·0	75·5	67·5	71·5	73·6	73·4	67·8	70·8	69·4	68·6	—	—

Unfortunately the test was discontinued at the end of a month, but the results suggest that even at this stage the effects of practice had not entirely disappeared. These facts did not become apparent until several weeks had been spent in the treatment of the results, and it was impossible to re-open the test because of the approach of winter which caused the lighting and heating conditions to be very different from those which prevailed during the test. Since several months must pass before the test can be repeated under similar conditions, it was decided to publish the results in their present incomplete form.

(b) *Variations within each test period.*

Since the times required to knot the first and second 25 ends in each test were noted, it is possible to determine the variations in speed within the test itself. The results dealing with this aspect of the test are given in Table II.

The general trend of these results is seen more clearly in Fig. 2, where they are presented in graphical form.

It will be seen that, with the exception of the third week, the curves representing the times required to knot the first and second 25 ends are approximately parallel, but in the fourth week the position of the curves is reversed. This reversal of position begins at the end of the second week, continues during the following week, and is not complete until the beginning of the fourth week.

The parallelism and reversal of the positions are accentuated when the weekly averages are considered. Such results are represented in Fig. 3.

These variations in the results are probably the resultant of the variable effects of interest, practice, and fatigue. Although the test involved the usual operations connected with winding, and was applied under the familiar industrial conditions, the fact that the winder knew that she was being tested would tend to give rise to a certain amount of nervousness during the first few days, the effects of which would be most marked in the early stages of each test. It is highly probable that the increased time required to knot the first 25 ends during the first and second weeks is partly due to this cause.

The test also involved a rhythm which was slightly different from that which prevailed during the ordinary winding operations. In the test the winder had to knot 50 consecutive ends, and consequently was unable to enjoy the short interval which usually elapses as she passes from one spindle to another often some distance away in the ordinary

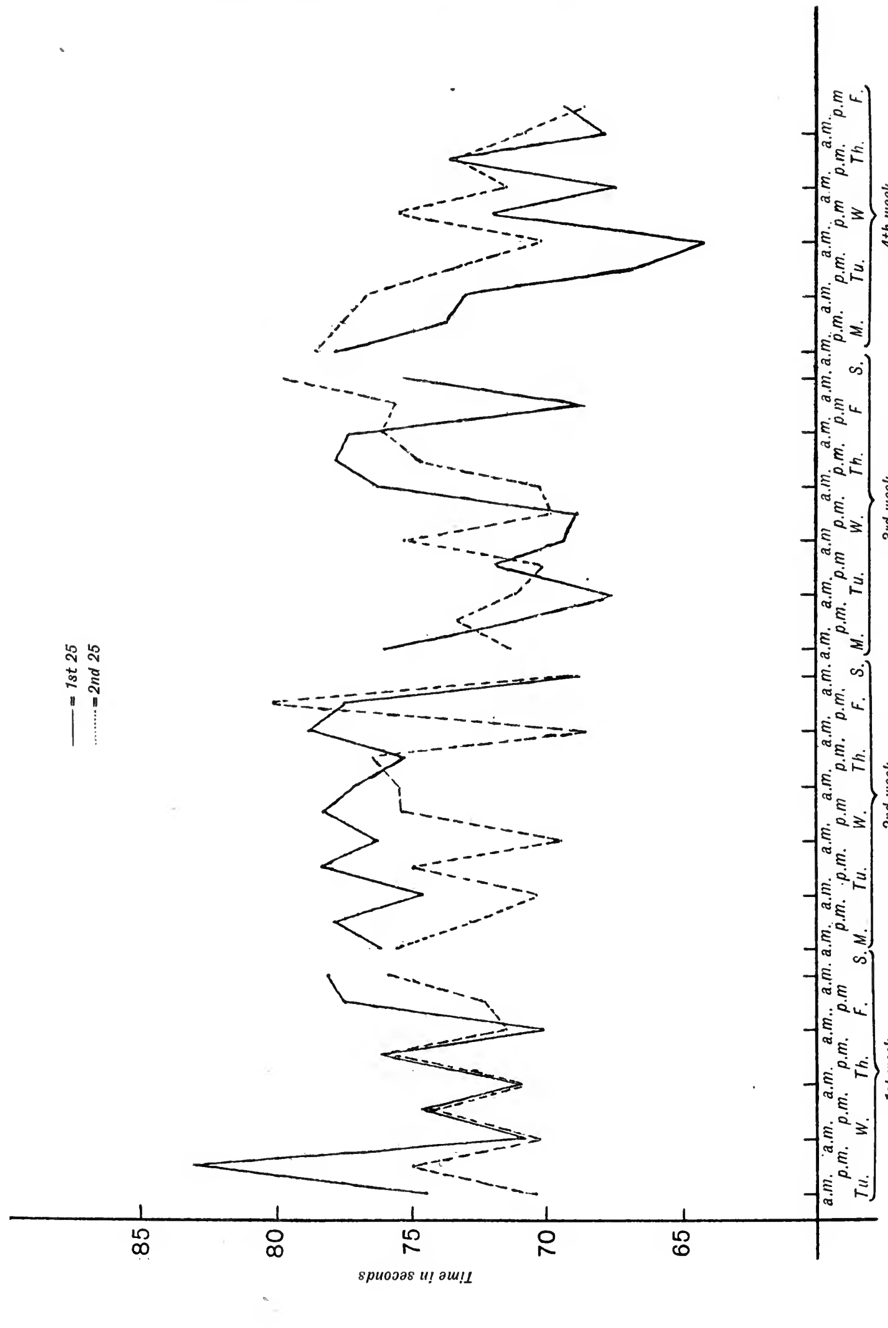

——— = 1st 25
.... = 2nd 25
Time in seconds
85
80
75
70
65
1st week
2nd week
3rd week
4th week

Fig. 2.

course of her work. The rhythm required by the test was quicker and more regular than that required by the ordinary winding process, and several trials would be necessary before the winder became thoroughly adapted to the test conditions.

It seems probable, therefore, that the relative position of the two curves during the first two weeks of the test is due to the causes mentioned, and emphasises the importance of the extended application of a test even though it appears to embody only those processes which the subjects have been performing several thousand times daily for many years.

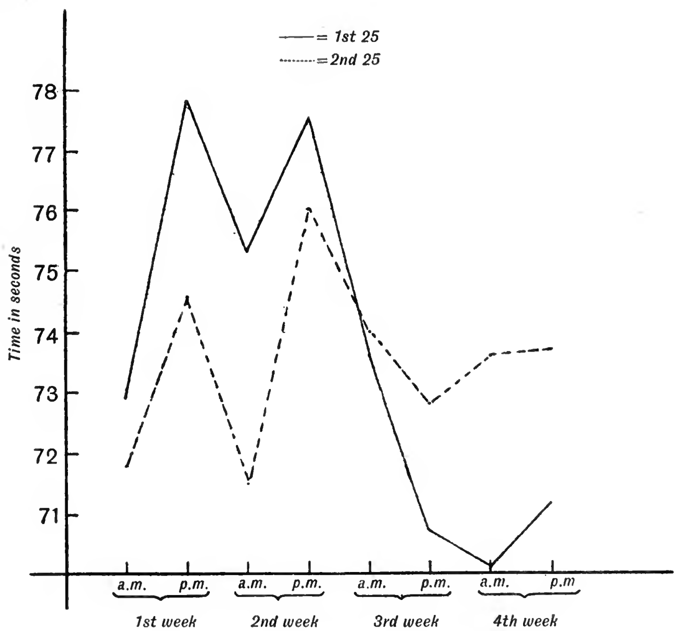


Fig. 3.

The results obtained during the fourth week probably represent the effect of a relatively complete adaptation to the test conditions. The times required to knot the second 25 ends are appreciably longer than the corresponding times for the first 25, a fact which suggests that the test itself gives rise to a certain amount of fatigue. This contention is supported by the remarks made by the winders at the close of the test, to the effect that they felt fatigue of the muscles involved. Thus the nature and duration of the test described is responsible for the existence of a certain amount of local fatigue, a fact which further reduces its value as a suitable test of fatigue.

(*c*) *Temperature and humidity.*

The temperature in degrees Fahrenheit and the relative humidity of the air in the vicinity of the winding frame at the time of the test are given in the following table:

Table III.

		Monday		Tuesday		Wednesday		Thursday		Friday		Saturday	
Week		a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.
1st	Temp.	—	—	68	68	66	68	65	65	63	64·5	63	—
	Rel. hum.	—	—	68	68	68	64	68	73	72	67	72	—
2nd	Temp.	63	69	64	62	59	61	60	62	61	62	62	—
	Rel. hum.	63	60	67	72	76	76	71	77	77	72	72	—
3rd	Temp.	60·5	61	61	65	64	66	65	68	66	68	65	—
	Rel. hum.	74	76	76	73	82	78	73	73	68	73	68	—
4th	Temp.	60	63	64	68	66	68·5	67·5	62	68	69	—	—
	Rel. hum.	71	68	63	68	68	73	73	73	73	68	—	—

It will be seen that the amount of variation throughout the test is comparatively small and fails to make any noticeable effects upon the results obtained. The evaluation of correlation coefficients between knotting times on the one hand, and temperature and relative humidity on the other show that no definite correlations exist. Whatever effects the small variations in temperature and humidity may have upon the efficiency of the winder, they are hidden under the influence of other factors.

4. Conclusions.

Conclusions based upon the results of four operatives over a period of one month can only be of a tentative nature, and may be modified when the investigation includes a larger number of subjects over a longer period. With this reservation, however, it appears that:

1. The time required to knot 50 ends under the conditions described is the resultant of many factors, and the effects of fatigue may be considerably reduced or even entirely hidden by the effects of these other influences. Even when the particular reaction itself has become a long-established habit, as was the case in this investigation, the ever-varying nature of the other psychological, physiological, and physical factors under test conditions tends to modify the results; for fatigue can only be clearly indicated when it has developed to such an extent as to become the dominant factor in the complex situation.

2. Even though the operations involved in the test are similar to those which the winders are in the habit of performing in the course of the ordinary winding operations, about three weeks must elapse before the winders become adapted to the test conditions. The effects of practice are distinctly noticeable throughout the test period, and the least variation from the usual conditions of labour has a disturbing influence upon the results.

3. The results appear to become more uniform and similar as the test progresses, but the afternoon results seem to be more irregular than those of the morning. Thus individual differences decrease as a result of practice, but the day's work has a variable effect upon different individuals.

4. As a means of indicating the amount of fatigue produced in any individual by the industrial conditions under consideration, the test is useless.

5. APPENDIX.

For the benefit and guidance of any investigator who may desire to conduct experiments along similar lines, it may be useful to enumerate and explain the effects of some of the disturbing factors encountered in this investigation. These may be conveniently classified as (i) objective, (ii) subjective.

(i) *Objective.*

(*a*) *Illumination.* The operation of knotting will obviously be affected by differences in illumination since the process involves minute visual discrimination. The mornings of Wednesday, Thursday, and Friday, in the third week of the test, were dull and misty, but the afternoons were bright and fine. It is highly probable that the longer times required to perform the test on the mornings of these days (cf. curve of averages) are due to this fact. Thus variations in illumination at the time of the test may more than neutralise the indications of fatigue.

(*b*) *Humidity.* Since the cooling power of the air depends partly upon the absolute humidity, it is quite possible that differences in the amount of moisture contained by the air at the time of the morning and afternoon tests, or on different days, may affect the results, since bodily comfort and feeling depend partly upon the rate of evaporation of surface moisture.

(*c*) *Temperature.* Efficiency is undoubtedly connected with the temperature of the surrounding air, but the most suitable temperature for winding has never been accurately determined. During the period under consideration moderate temperatures prevailed and the greatest difference between the morning and afternoon temperatures was only 6° F.

(*d*) *Air movement.* Currents of air in the vicinity of the operatives caused either by unequal temperatures in different parts of the room or by machinery movements may exercise a stimulating effect upon the winders, and since these currents are variable the results may be affected accordingly.

(ii) *Subjective.*

(*a*) *Mood.* Variations in the mood of the operative may cause similar variations in efficiency. Remarks made by the winders together with observations of their behaviour disclosed the existence of many different moods which could not be entirely attributed to the presence or absence of varying amounts of fatigue. Sometimes an operator would appear to be depressed and indifferent while at others she seemed bright and happy. The existence of a mood at the time of the test may easily affect the actual capacity for work, and accentuate or diminish indications of fatigue.

(*b*) *Emotions.* Since an emotion is usually more intense and transient than a mood, it is more liable to affect the results of any single test which may be applied at the time of its occurrence. Unless the operatives are able and willing to indicate the nature of their emotional state at the time of the test, it is almost impossible to interpret the results correctly. A winder may be unable to repress the feelings connected with the thoughts of a dance to be held that night; her results will probably indicate the existence of a considerable amount of fatigue, but in reality her reaction times are increased because of the distracting thoughts.

(*c*) *Emotive perseveration tendencies.* These may be variable in their nature and arise from a multitude of causes. A grievance caused by the unjust treatment of a superior, or reflexions upon some domestic trouble may frequently and periodically occupy the focus of consciousness and disturb the operations involved in the test.

(*d*) *Temporary indisposition.* During the course of the investigation it was observed that a winder was slightly indisposed at the time of the morning or afternoon test and was either unable to perform the test or gave such abnormal results that they had to be discarded. Unless the indisposition is noticeable the results may be retained as normal and consequently give rise to incorrect interpretations.

(*e*) *Nervousness.* On one occasion an additional investigator was present during the test, and winder No. 2 was perceptibly nervous. Even when the operatives appear to have become adapted to the presence of the usual investigator, a temporary wave of nervousness may pass over them and affect the results accordingly.

(*f*) *Incentives.* The disturbing effect of periodically or irregularly occurring incentives is very noticeable. The desire to complete a certain number of bobbins within a definite period of time is an instance of such an incentive; and produces a temporary increase in efficiency. The existence of variable stimuli of this nature frequently hides the usual manifestations of fatigue.

Any one or more of these factors may account for the discrepancies observed in the results, but the list enumerated above is by no means exhaustive. It would appear that any test which involves volition and constant attention is liable to be affected by mental and emotional disturbances, and is not likely to be a satisfactory test of fatigue.

(*Manuscript received* 24 *February*, 1920.)

TWO EXAMPLES OF CHILD-MUSIC

By WILLIAM PLATT.

It is a matter of common knowledge that young children, especially when in a dreamy mood, will croon or murmur little tunes or scraps of melody which are obviously original and spontaneous. Young children will often sing, instead of say, some of their shorter sentences, making up the tune as they utter it. I have known this happen as early as three years old and as late as twelve. Young children, when two are together, will also sing their little snatches in harmony, instinctively realising what has a good (or passably good) effect. They will also, and this is one of the most remarkable of their achievements, sing in the form of Round or Canon, one of the earliest forms of part-music, inventing as they proceed. Those who wish to follow this subject further will find in my book[1] full details and many examples, including snatches invented by children from 17 months old to four years old, and canons invented by children of three and four years old.

Since that book was published I have heard many other child-tunes, but none of sufficient difference to demand especial mention till this year, when I noted two very fine examples. The first was a bold little piece of part-singing made up by two children (strangers to me) as they walked over Hampstead Heath. The girl looked about 5 or 6 years old, the boy about 7 or 8. Hers was the chief theme, in the lower part; his was the upper counter-subject.

The instinct that guided his counter-subject was an exceedingly good one. And here let me reiterate my contention that the child's spontaneous creation of music is the finest guide to the art-impulse of his nature. He may attempt to draw, but he lacks control of his hand.

[1] *Child-Music.* London: Renaissance Music Co., Cromwell House, Holborn.

Similarly his aptest self-expression in speech is still inadequate; for this medium also needs much exercise before it becomes fluent enough for real expressive purpose. But in musical expression he is not only far more on an equality with adults, but he is often their superior. How many of the readers of this article would venture to stand up with a friend and improvise a vocal duet? How many, attempting it, would do it as prettily and happily as the example given above? They could only rival the child if they could become as instinctive and as self-unconscious as he. Could they do that, they would probably surprise themselves; for music is an instinct deep down in all of us, and simple spontaneous little musical phrases form the easiest of all art-utterances, whereas the complex orchestral effects form probably the most difficult of all.

The second of the examples to which I have referred above is quite the most remarkable that I have ever noted; it is, in fact, the most interesting specimen of a child-tune ever published. It is a snatch that I heard crooned (with a gentle, humming tone) by a babe only four-and-a-half months old, as he lay contentedly at his mother's breast. It was many times repeated in a dreamy monotonous tone; it was absolutely in tune, despite the somewhat difficult drop of a seventh. The babe who uttered it showed very early the greatest interest in music, and both his parents and all his four grand-parents are musical.

The harmonic basis of this phrase is somewhat similar to that of the duet above noted. Both may be said to be based on the authentic cadence, in contrast to the many examples in which I have shown a distinct tendency in children to favour the plagal cadence; so that it is abundantly evident that children show their temperamental differences in their music.

(*Manuscript received* 28 *January*, 1920.)

A VOICE REACTION KEY.

By ERNEST W. BRAENDLE.

(From the Cambridge Psychological Laboratory.)

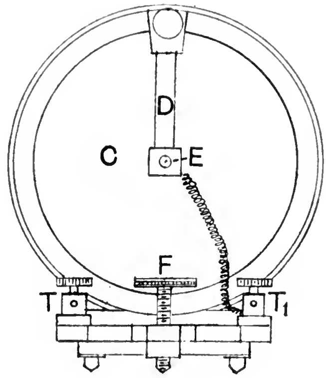
In order to satisfy the requirements of a voice reaction test, it is necessary that either the starting or stopping (or in some cases both) of the Hipp chronoscope be effected by the voice itself. Several devices have been employed for this purpose, but they have generally proved so cumbrous or unreliable, that the voice key has fallen into disrepute.

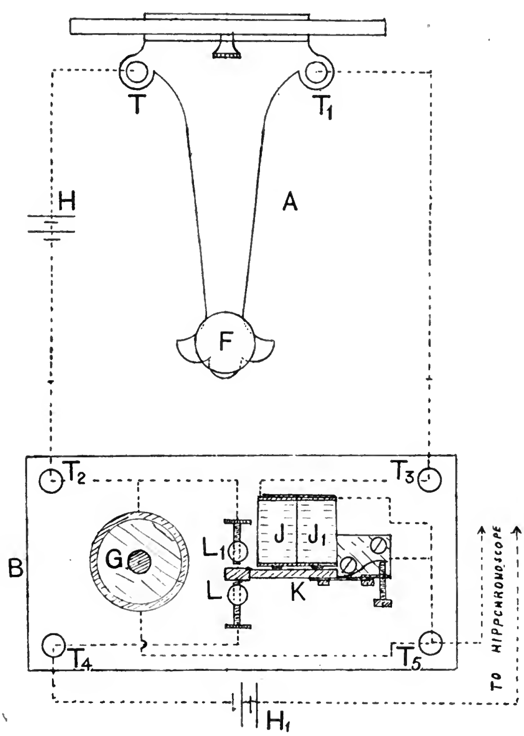
The first model of the apparatus here described was made and tested at the Cambridge Psychological Laboratory, in July, 1919, but has since been improved on, in regard to certain minor details. The apparatus may be said to consist of two parts—(1) the diaphragm circuit breaker A, and (2) the relay B. By suitably mounting the diaphragm I have been able to actuate the relay with a voice of normal loudness up to a distance of three feet.

(1) The diaphragm circuit breaker A consists of a thin metal diaphragm C, in the centre of which, and insulated from it, is a small platinum stud, connected by a piece of flexible wire to the terminal T_1, which is insulated from the base. Suspended from the top of the framework by means of a very thin flat steel strip D is another platinum stud E, which makes contact with the stud in the centre of the diaphragm. A thumb-screw F is used for tilting the diaphragm backwards or forwards, in order to allow the stud E to swing, so that when at rest it is just touching the stud in the centre of the diaphragm. A terminal T is fixed to, and connected with, the framework.

(2) The relay B is of the usual bell type, the button-switch G being used solely for the purpose of resetting the relay.

Connecting the apparatus up as shown in the sketch, it may be seen that, by pressing the button-switch G, the current from the battery H is allowed to flow through the electro-magnets J and J_1, thus drawing the armature K away from the contact-pillar L, where it rests normally, over to the contact-pillar L_1, thereby breaking the circuit of the battery H_1 through the Hipp chronoscope and at the same time short-circuiting the button-switch G, which may then be released. If, now, one of the


D
C
E
A
F
T
T_1


T
T_1
H
A
F
T_2
T_3
B
G
L_1
L
J
J_1
K
T_4
T_5
TO HIPPCHRONOSCOPE
H_1

various types of apparatus for exposing a syllable be placed in parallel with the terminals T_4 and T_5 and a 'break-make' arrangement be employed, the circuit of battery H_1 will be closed and the Hipp chronoscope hands will remain stationary. Exposure of the syllable will cause the circuit of H_1 to be broken and the hands of the Hipp chronoscope to rotate. If now on the exposure of the syllable a word is spoken on to the diaphragm C, the vibration of the diaphragm thus produced will cause the platinum stud E to lose contact with the stud in the centre of the diaphragm, thereby for an instant breaking the circuit of battery H and allowing the armature K to fly back, away from the contact-pillar L_1, thus permanently breaking the circuit of H and preventing the armature K from returning. The armature K is now consequently in contact with the contact-pillar L and, the circuit of battery H_1 being 'made,' the hands of the Hipp chronoscope will cease to rotate, thus the time which has elapsed between the exposure of the syllable and the action of the voice on the diaphragm C may be noted.

A second diaphragm circuit breaker and a relay, having the contact-pillar L on the other side of the armature K, which is now extended in that direction, may be used in place of the syllable exposer to start the Hipp chronoscope.

I have also designed an instrument for experimenters who are unable to employ a Hipp chronoscope, the latter being replaced by a stop-watch, measuring to hundredths of seconds. I have entrusted the manufacture of these instruments to Messrs Hawksley of Oxford Street, London, from whom further particulars may be obtained.

(*Manuscript received* 5 *March*, 1920.)

THE DISTRIBUTION AND RELIABILITY OF PSYCHOLOGICAL AND EDUCATIONAL MEASUREMENTS

By WILLIAM McCLELLAND.

A DIFFICULTY which arises in connexion with the statistical treatment of psychological and educational measurements is that we have, as a rule, a double case of sampling. Not only do we take merely a sample of the individuals, but we take merely a sample of the performances of each. The same problem, of course, arises in Physics and Biometrics, but in the former science the one, and in the latter science the other, source of error is negligible compared with its companion error. In Psychology, however, both are important, and it is the object of the present paper to draw attention to some consequences which have a bearing on the arrangement of a set of measurements and on the treatment of the results obtained.

Let the individuals and the measurements on each individual be normally distributed, with standard deviations σ and σ_m respectively, and in the first instance let us assume that σ_m is the same for all the individuals.

If n be the number of individuals measured, and m the number of measurements made on each; and if Σ be the *apparent* standard deviation of the individuals, each individual being represented by the mean of his m measurements, then it can be shown that

$$\Sigma^2 = \sigma^2 + \sigma_m{}^2/m \quad \text{..............................(1).}$$

It follows that the probable error of the mean value found by making m measurements on each of n individuals is

$$\frac{\cdot 6745}{\sqrt{n}} \sqrt{(\sigma^2 + \sigma_m{}^2/m)} \quad \text{...........................(2).}$$

By giving m and n various values we can find the particular formulae for special cases, as for example making one measurement on each of

n individuals and so on. The total number of measurements is mn, and if this is fixed (say by the time at our disposal) then it is seen that mathematically greater accuracy is attained by increasing the number of subjects as much as possible, making only one experiment on each, if our object is to obtain a value for the racial or class mean[1].

If σ and σ_m can be obtained approximately from experiments already made, formula (1) further enables us to decide the extent to which it is necessary, in the interests of accuracy, to multiply the measurements on each subject. Such increase in the number of measurements does not affect the first term on the right-hand side of (1) but only the second, viz. $\sigma_m{}^2/m$, and if we wish merely to reduce Σ, there is clearly no point in reducing this second term when it is already small compared with the first.

In practice σ is unknown and has to be deduced from Σ and σ_m by writing equation (1) in the form

$$\sigma^2 = \Sigma^2 - \sigma_m{}^2/m \qquad \text{(3)}.$$

This formula, of course, should not be used to find σ if n or m is small.

Actual experiments with continuous subtraction, where the score was reckoned by the number of pairs of digits subtracted in 10 minutes, gave $\Sigma = 51{\cdot}4$ and $\sigma_m = 11{\cdot}9$.

Hence $\sigma = 50{\cdot}0$.

This shows that with continuous subtraction, neglect of the fallibility of the measurements on the individuals causes an error of about 3 % in the estimate of the standard deviation of the 'true' values for the individuals[2]. With operations where the results are less consistent, and consequently σ_m is much higher, the error caused would naturally be greater, and the question arises whether this factor should not be taken into account in such investigations as that recently carried out by Burt in London[3].

Knowing σ and σ_m we can calculate by formula (2) the probable errors for different numbers of subjects and tests, and we get the following table which shows the relative advantages of increasing the number of subjects and the number of measurements on each subject.

[1] A different lower limit to the number of measurements on each individual may, of course, be rendered necessary by some special end in view or by psychological considerations.

[2] Of course, as is done above, we should use the measured value of Σ without reduction in finding the probable error of our estimate of the racial mean.

[3] Burt, *The Distribution and Relations of Educational Abilities.*

Table I.

Probable Errors.

Number of subjects	Number of tests					
	1	2	3	4	5	∞
10	10·9	10·8	10·7	10·7	10·7	10·7
15	8·9	8·8	8·8	8·8	8·8	8·8
20	7·7	7·6	7·6	7·6	7·6	7·5
30	6·3	6·2	6·2	6·2	6·2	6·1
40	5·5	5·4	5·4	5·4	5·4	5·3
50	4·9	4·8	4·8	4·8	4·8	4·8
100	3·5	3·4	3·4	3·4	3·4	3·4
1000	1·1	1·1	1·1	1·1	1·1	1·1

It thus appears that with an operation like continuous subtraction there is little use in making more than one measurement on each individual, unless we have some other end in view than finding the position of the mean. With the present results, for instance, the addition of one subject is of as much value as making twenty additional tests on each of the original subjects. In this connection it is interesting to compare Spearman's method of dealing with correlation[1] where he holds that by making more careful experiments on a smaller number of subjects we shall get as good results. This is clearly not the case in the continuous subtraction experiment. Obviously it depends on the relation of the variability of the individuals among themselves, on the one hand, to the variability of the measurements on any one individual, on the other. If, for example, the standard deviation of the measurements were the same as that of the individuals the probable errors would be as follows:

Table II.

Probable Errors.

Number of subjects	Number of tests					
	1	2	3	4	5	∞
10	15·1	13·1	12·3	11·9	11·7	10·7
20	11·7	9·2	8·7	8·4	8·3	7·5
100	5·2	4·1	3·9	3·8	3·7	3·4
1000	1·5	1·3	1·2	1·2	1·2	1·1

In such a case it is obviously desirable to make more than one measurement on each subject. Thus, by making five tests on each of

[1] *Amer. J. of Psychol.* xv. 201–292. *Ztsch. f. Psychol.* 1906, xliv.

ten subjects we get as precise a result as if we had single tests on twenty subjects.

The above results point to the following conclusions:

(1) If an accurate value of the racial or class standard deviation is required, and if the variability of the measurements on each individual is high, it would appear that the 'raw' standard deviation should be corrected by subtracting the value of σ_m^2/m from its square. In all cases, however, it should be remembered that corrections based on the theory of probability are themselves subject to error, are only 'probable' corrections both as regards amount and direction, and if large are certainly untrustworthy, being usable in the latter case only as a touchstone for deciding the rejection of results, not as corrections; while corrections comparable in amount with the probable error of the quantity corrected are unnecessary.

(2) In planning a set of measurements we should if possible obtain a rough measurement of the values of σ and σ_m, and from these decide the number of times it is necessary to repeat the measurements. We should thus ensure that on the one hand we should get the most accurate value from a given number of subjects, and on the other we should be saved the trouble of making more measurements on each subject than are necessary.

In ordinary experimental work in Psychology and Education it is found that σ_m varies from individual to individual, but the mathematical treatment of this case is impossible in view of the lack of knowledge as to how variability is correlated with performance and as to how variability is distributed. It is also possible that the treatment will be different for different kinds of work. Brown, for example, finds that there is a pronounced positive correlation between ability and variability in the case of speed of addition, and a negative correlation in the case of accuracy[1]. In all cases, however, the variability of σ_m will be small relatively to Σ and σ_m, and it is probable that the above results will hold sufficiently closely if σ_m is replaced by the mean value for all the individuals.

[1] W. Brown, *Essentials of Mental Measurement*, 127.

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THE GENERAL FACTOR FALLACY IN PSYCHOLOGY.

By GODFREY H. THOMSON,

Armstrong College, Newcastle-upon-Tyne.

I. A natural relationship common to all sets of correlation coefficients is what Prof. Spearman has called hierarchical order.

II. The illusion that the hierarchical order found in psychological research is perfect is due to a mathematical error.

III. The first 'condition' which Mr Garnett would impose on the bonds which cause correlation is as unnecessary as is a General Factor.

IV. So also is Mr Garnett's second 'condition.'

V. Mr Garnett's 'linear transformation' is only completely successful when hierarchical order is perfect, and is in any case only an elaborate way of saying that hierarchical order may be due to a General Factor.

VI. Conclusions.

It is with regret that I find it necessary, in view of a recent article by Mr J. C. Maxwell Garnett[1], to return to the question of the phenomenon of hierarchical order among correlation coefficients and to insist, with all the emphasis of which my pen is capable, that the deduction of the presence of a General Factor which Professor Spearman has based upon this phenomenon is utterly and entirely invalid.

I.

Hierarchical order is the natural order to be found among any set of correlation coefficients, *however the correlations may be caused*, provided only that the bonds which cause the correlations are left, as we say, to chance: that is, provided the phenomenon is of that complex nature which, in default of analysis, we call *random*. Hierarchical order will arise among correlation coefficients unless we take pains to suppress it. It does not point to the presence of a General Factor, it cannot be made the touchstone for any particular form of hypothesis, for it occurs even if we only make the negative assumption that *we do not know* how the

[1] This *Journal*, 1920, x. 242.

correlations are caused, that is, if we assume only that the connexions are random.

That this is so, follows quite generally from the investigations of Professor Pearson and Professor Filon on the correlation of correlation coefficients, as I have shown elsewhere[1]. If correlation coefficients are arranged in the square table usual in psychological research—an arrangement which Mr Garnett calls a correlation table, though that term has always had a quite other meaning—then the visible expression of the Pearson and Filon formulae is the occurrence of ridges in the pattern of numbers, and of peaks at the nodes where two ridges cross. This means, as is shown in my paper just referred to, that the coefficients can be arranged in hierarchical order. That paper was indeed more general than I then felt justified in claiming: and such expressions of caution as I inserted from an instinct of scientific reserve were in fact unnecessary, as further consideration has shown me.

It is not even requisite that the correlations should be produced by overlapping, that is by the fact that some elements occur in both the correlated variables. They may be caused in more subtle and less tangible ways, of which, as illustration, may be instanced the correlations between hands at whist: and connexions even less easy to visualise can be conceived. All these possibilities are included.

II.

Prof. Spearman's argument is in outline as follows: (1) A considerable degree of hierarchical order is observable among correlation coefficients. (2) After allowance has been made for errors, by means of a certain criterion which we may call 'the Hart and Spearman criterion,' this hierarchical order is found to be perfect, or practically perfect, in every case. (3) Therefore the correlations are caused solely by a General Factor.

The difficulty of scotching this fallacious argument is due largely to the presence of the second step, for many experimenters have been led to believe that the hierarchical order in their results was indeed perfect. But this illusion arises entirely from an error in the Hart and Spearman criterion, which, as I have shown in another paper[2], creates the regularity which it purports to detect. The use of this criterion has vitiated at their very foundations a number of researches many of which are, from an experimental aspect, of great value: it is for example freely employed

[1] *Proc. Roy. Soc.* A, 1919, xcv. 400.
[2] *Biometrika*, 1919, xii. 355.

in Dr Webb's memoir "Character and Intelligence[1]," and is the first formula in Mr Garnett's paper "On Certain Independent Factors in Mental Measurement[2]."

This illusion being cleared away, and the hierarchical order recognised as only imperfect, the fallacy of basing a hypothesis on a phenomenon, which occurs whether the hypothesis be true or not, is evident. We are forbidden by the whole of our scientific training to postulate entities where they are not necessary.

If I make the discovery that the angles of a quadrilateral are equal in sum to four right angles, I may not conclude that it is a square. This is, by analogy, what Prof. Spearman did when he noticed hierarchical order, and deduced a General Factor. True, the angles of a square are equal to four right angles, and the thing may be a square: and similarly a General Factor may exist. But the marks of the genus do not define the species.

Nor may I conclude that I may call it a square because that is simpler: which is, by analogy, what Mr Garnett does. To be exact, he does rather more than that. He admits that it may not be a square, but holds fast to the faith that it is at least rectangular, a conclusion which is as unwarranted as the narrower one. He claims, that is, that while a General Factor may not be present, yet the elements which are present must be bound by a certain Rule[3], and must fulfil two conditions[4]. *These hampering restrictions are just as unnecessary as the General Factor.*

III.

This assertion is already proved by the generality of the work based on the Pearson and Filon formulae. But to make the matter more clear, it may be worth while to give *ad hoc* consideration to these conditions.

Mr Garnett considers that if a hierarchy is to be produced without a general factor, then "any two given higher level elements both of which enter into any test must *tend* to enter in the same ratio (unity or any other) into that test as into any other test into which they both enter." And he protests that I have artificially supplied this condition by making the elements *all-or-none* in their action.

Now in the first place I must claim that the *all-or-none* principle is, in view of recent work in neurological research, not altogether an unlikely one. But I hasten to add that when I was "careful to qualify the

[1] This *Journal, Monograph Supplement*, 1915.

[2] *Proc. Roy. Soc.* A, 1919, xcvi. 91.

[3] *Op. cit.* p. 248.

[4] Numbered (1) and (2), upper half of p. 255.

conclusion" by means of the sentence asserting the *all-or-none* principle, I did so, not because the conclusion was untrue without such qualification, but because the experiments upon which I had based it in that paper were with *all-or-none* elements, namely dice. If I had omitted the qualification, the conclusion would have been wider than I was justified in drawing from those particular experiments, but it would in point of fact have still been true.

For let us abandon for a while in the higher elements the *all-or-none* principle and leave the extent to which each element acts, in each activity into which it enters, *entirely to chance.* In spite of our doing this, the ratio in which two given elements enter any test will still *tend* to be the same as that in which they enter another test of which they form part. I shall refrain from burdening the pages of this *Journal* with the mathematical symbols which would be required to prove this generally. Such a proof would be repellent to the non-mathematical reader, and is unnecessary to the mathematical reader, who can supply it for himself by analogy with the following simple illustration. Let us assume that an element may enter into a test *either* with one-half *or* with the whole of its energy. Consider two elements entering into a certain test. The possible ways in which they may do so are four, namely:

(1) $\frac{1}{2} : \frac{1}{2}$;
(2) $\frac{1}{2} : 1$;
(3) $1 : \frac{1}{2}$;
(4) $1 : 1$.

That is, in two cases, those numbered (1) and (4), their ratio is unity. In the remaining cases their ratio is $\frac{1}{2}$ and 2 respectively.

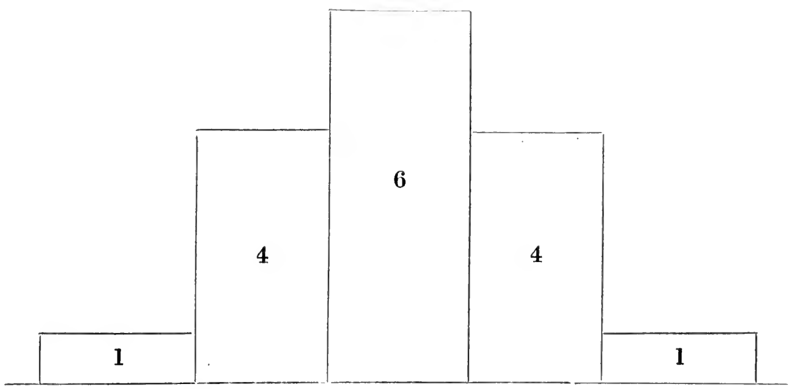
Now consider their entry into a second test or variable, which will take place in the same four ways. Any one of the four ways in which they enter the first test can be associated with any one of the four ways in which they enter the second, making sixteen cases in all. These sixteen cases are shown in the following short table, in which the marginal

		Ratio in first test			
		$\frac{1}{2}$	1	1	2
Ratio in second test	$\frac{1}{2}$	1	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{4}$
	1	2	1	1	$\frac{1}{2}$
	1	2	1	1	$\frac{1}{2}$
	2	4	2	2	1

figures indicate the ratios in which the elements enter the first and second tests or variables respectively, while the inner figures show the relationship of these ratios to each other, the second divided by the first. The two ratios are identical in six of the sixteen cases. In detail, the fraction

$$\frac{\text{second ratio}}{\text{first ratio}}$$

takes on the values $\frac{1}{4}$, $\frac{1}{2}$, 1, 2, 4 in the proportions 1, 4, 6, 4, 1 out of sixteen. The last set of numbers will be recognised as the coefficients of a binomial. Expressed graphically, they give the histogram



in which the tendency towards the central value is evident. In the general case this would become a normal curve. The difficulty about the spacing along the x-axis which may occur to some reader may be minimised for him by the reminder that we might just as well have taken the reciprocal ratio.

IV.

Turning to Mr Garnett's second condition, we find that it too is a 'condition' which fulfils itself and need not be made. It is that the elements must combine "so that the number of higher level elements that enter as group factors into any two...tests *tends* always to be proportional to the product of the whole numbers of elements in the two...tests."

Now this is the fundamental theorem in probability; and that they will so tend without further ado is obvious. For what is the chance that a given element will enter a test? It is the whole number of elements in that test, n_1, divided by the entire number of elements in existence, N; it is

$$\frac{n_1}{N}.$$

In a similar way, the chance that it will enter into the second test is

$$\frac{n_2}{N},$$

where n_2 is the whole number of elements in the second test. And the chance of an element being in both is the product

$$\frac{n_1}{N} \times \frac{n_2}{N}.$$

The conditions which Mr Garnett requires to be satisfied are therefore exactly those which mere chance will satisfy. It is as unnecessary to require them as it is to postulate a General Factor.

The additions which Mr Garnett makes to my explanation of hierarchical order are therefore unnecessary and indeed erroneous; as erroneous as saying that a quadrilateral must be rectangular if its angles are to have a sum of four right angles. And indeed his whole placing of the two 'alternatives' (by means of which hierarchical order may be explained[1]) as alternatives of equal standing is erroneous. The relationship between them is not one of alternation but of subalternation. The second is greater than the first, and includes it, as the genus quadrilateral includes the species square.

V.

The paper is, in fact, apart from the errors which I have pointed out, merely an elaborate way of stating that the theory of a General Factor, although not proved by hierarchical order, is yet left as a possible (though unproven) hypothesis. Mr Garnett's last paragraph (before his summary) is headed: "Dr Thomson's results are consistent with the existence of Prof. Spearman's single general factor in every set of sufficiently dissimilar mental qualities." Of course they are. The only objection I have to Mr Garnett's statement of this obvious fact is, that it may very well lead the reader to imagine that I do not realise it. I would refer the reader to the first two sentences of my first paper[2] on the subject: to the sentence on the first page[3] of a later article, in which I say that "there are many theories *in addition to* that of Prof. Spearman which will explain hierarchical order": to a sentence in a third and more general paper[4], in which I say "there is no doubt but that such a general factor, combined with such an absence of group factors, would produce hierarchical order": and to many other places in those articles. A figure whose angles total four right angles may of course very well be a square,

[1] *Op. cit.* bottom of p. 254 and top of 255.
[2] This *Journal*, 1916, VIII. 271.
[3] This *Journal*, 1919, IX. 337.
[4] *Proc. Roy. Soc.* A, 1919, XCV. 401.

but its squareness is not thereby proved, nor is the existence of general ability.

Mr Garnett's way of stating the whole problem is algebraic in form, but no new truths are thereby revealed. He discovers that the *general* equations which express my general explanation of hierarchical order can, by "a linear transformation of independent variables" be written in that *special* form which expresses the theory of a general factor: he discovers that a quadrilateral, if no further conditions are laid down, may be a square.

In passing it may be noted that, in carrying out this 'linear transformation,' Mr Garnett is as elsewhere obsessed by the idea of absolutely perfect hierarchical order. It is only in this case that the transformation is completely successful, and only in this case that group factors entirely disappear and the factors left are either specific, or "small interchangeable parts of a general factor." But such a transformation can in any case, it may at once be agreed, show that such hierarchical order as is present in a practical case may be due to a general factor, though it could not get rid of group factors except in the perfect case.

Of similar origin is Mr Garnett's demand for an infinite number of factors if the general factor is disallowed. One might in reply point to the still larger number of specific factors required in the alternative, an infinity of higher order. But in practice by no means an infinite number of group factors is required. In that one of my dice experiments to which alone Mr Garnett refers in detail, there were employed 145 factors, of which 36 were group factors: and in another purely chance hierarchy described in this *Journal*[1], there were 87 specific and 13 group elements. In a third, described in another place[2], there are 57 specific and 13 group elements, and the hierarchical order, though poorer, is still very considerable, *and is claimed as absolute perfection by the Hart and Spearman criterion*[3]. Finally, if Mr Garnett cares to try a numerical example (and his errors and misconceptions are almost entirely due to his neglect of this salutary practice) he will find that 13 group factors alone, with no specific factors at all, will give quite a reasonable hierarchy[4]. Infinity, therefore, turns out to be in practice about a couple of dozen, implying a degree of complexity which we may surely without hesitation postulate of the human mind or brain. A conservative estimate of the elementary qualities of sensation alone places them at about 50 to 80[5].

[1] 1919, IX. 341–3.

[2] *Proc. Roy. Soc.* A, 1919, XCV. 401–3.

[3] *Biometrika*, 1919, XII. 361–3.

[4] Cf. This *Journal*, 1919, IX. 341, line 21.

[5] W. McDougall, *Physiological Psychology*, London, 1905, 75.

The whole paper raises the question of what writers mean by a General Factor; the question of the degree of reality and individuality which is attributed to factors both general and specific. For consider another illustration. Let us suppose that five subscription lists were opened in a town, and that the sums obtained were £190, £170, £183, £115 and £124. Then, if I do not see the subscription lists, it is open to me to believe *either* (1) that a lot of people have contributed to each list, no doubt some to more than one list, *or* (2) that a beneficent gentleman, acting the part of a general factor, has given £100 to each. But, even if I write out mythical subscription lists on the second theory (even, that is, if I perform a linear transformation), the actual lists are in existence and may well show that no flesh and blood donor of £100 existed.

And in the same way, if we attribute any degree of reality to the elements which enter into any mental test—if, for example, we assume each to correspond to a neurone, or to a synapse, or to a nervous arc—then a 'linear transformation,' though it may give mathematical equations which describe the quantitative situation, yet does so by substituting for (say) the real neurones a set of mythical neurones, of which each is composed of a little bit of this, a little bit of that, and a little bit of the other real neurone.

VI.

I have ventured to assert, more emphatically than in former papers, the utter invalidity of deducing a general factor from hierarchical order unless absolutely perfect. Hierarchical order, of a high degree of perfection, is the natural relationship which *all* correlation coefficients bear to one another. The Hart and Spearman criterion for the degree of perfection is erroneous and creates the extreme perfection it purports to detect, and which, were it really present, would, it has always been admitted, imply a general factor.

Mr Garnett's attempt to restrict my general statement by qualifications breaks down, for the conditions he wishes to impose are those which chance itself supplies.

The general factor theory is of course, as I have always said, possibly true: but it is unproven, as squareness is unproven of a quadrilateral merely by reason of the fact that its angles are, in the sum, equal to four right angles. To prefer the general factor theory because of its simplicity is as likely to lead to error as would the equally simple practice of making all quadrilaterals square in geometry.

(*Manuscript received* 22 *March*, 1920.)

FLUCTUATIONS IN MENTAL EFFICIENCY.

By B. MUSCIO.

(*A Report to the Industrial Fatigue Research Board.*)

I. Aims.

As the character of much of the work in industry is such that it may be supposed to induce central nervous, rather than peripheral muscular, fatigue, it would appear that in seeking reliable tests of industrial fatigue those tests should be selected which are likely to reveal changes in central conditions. Accordingly the tests which form the basis of this paper have been 'mental' tests, and they have been employed to investigate the fatigue caused primarily by continuous demand upon the attention.

II. The Experiments[1].

(*a*) *First experiment.*

In the first experiment, three tests—a calculation test, a group number checking test, and a complex cancellation test (all described

[1] I wish to thank Dr Winifred Cullis for making all arrangements with her students for the carrying out of these experiments, and the students themselves for taking part in them; also Dr C. S. Myers for very kindly reading through the MS. of the paper and making valuable suggestions.

below)—were given eight times a day to twenty young women engaged in continuous academic work—attendance at lectures, laboratory work and scientific reading. The first test on each day began at 10 a.m. and the last at 5 p.m., the others being distributed over the intervening time at intervals of one hour. On each occasion the tests lasted only a few minutes, and they were carried on for five days. The data showed one result conclusively: that *experiments of this kind are absolutely valueless for the investigation of fatigue because of the complications caused by practice.* It is impossible to determine whether fatigue causes a deterioration in the performance of any test so long as practice is causing an appreciable improvement in its performance; and in the present case the subjects were still improving at the end of the fifth day. In such a situation the opposing influences of practice and fatigue cannot be satisfactorily evaluated.

The first problem therefore was to obtain a satisfactory experimental method by which the effects of fatigue could be separated from the effects of practice. Now, it is *in theory* always possible to eliminate practice by prolonging an experiment until practice effects become inappreciable; but such procedure is hardly ever feasible because of the demands it makes upon subjects. In giving tests to industrial operatives this difficulty is greatly increased; for they themselves have little scientific interest in the investigations, and the industrial manager will not readily consent to their giving much time to the working of tests. Is there, then, any other method by which practice can be eliminated?

Such a method is the 'method of equal groups.' This consists in first obtaining groups of subjects equal in number and in the capacity for a given test, and in then applying the test to these groups under any desired conditions. The simplest case is that in which there are only two such equal groups. These are obtained by first giving the test to all the subjects who are to be tested and by dividing them into two equal groups on the results. One group is then given the test, say, before work, and the other, say, after work. If the groups are known to be equal in capacity for the test, any difference in the results then obtained from them may be considered a probable fatigue effect. At any rate, it could not be due to practice, as the two groups would be at an identical stage of practice in the hypothetical condition stated. The method of equal groups was therefore made the basis of the experiments now to be described.

(b) Second experiment.

(1) *The Subjects.* These were thirty-four third year students (average age = 22 years, *m. v.* = 2·9) at the London (Royal Free Hospital) School of Medicine for Women. Sixteen of these students had already been subjects in the first experiment.

(2) *The Tests.* Three tests, which will be referred to as T_1, T_2, and T_3, were used. These were the same tests that were used in the first experiment, and consequently the sixteen students who had participated in that experiment were in a fairly advanced practice stage with regard to them. To the remaining eighteen students the tests were new.

T_1 consisted of 40 arithmetical examples of the following type: $6 \times 5 \div 3 = 11, 15, 30, 10, 20$. The correct answer was included among the numbers on the right-hand side of the equation, and had to be underlined.

T_2 consisted of 8 vertical columns of 6-place numbers. The subjects were required to underline each of these numbers containing a 1 and 3 and 7. Each vertical column contained 25 6-place numbers, and 10 of these contained all three specified digits.

T_3 consisted of 20 horizontal rows of 30 digits each. Subjects were required to put a vertical stroke through every 3, a horizontal stroke through every 6, and a circle round every 5. There were 10 digits to be marked in each row.

The tests were always worked in the order T_1, T_2, T_3.

Eight different test papers (blanks) were used for each test. Once the subjects had worked through eight test periods, they had to use again test papers containing exactly the same figures as had occurred in the earlier papers.

(3) *Lighting, Ventilation and Temperature.* The lighting and ventilation of the room in which the tests were carried out were good and seemed very constant. The range of variation of the room temperature during the experiments was not more than 2·5° C.

(4) *Procedure in Giving the Tests.* (*a*) *Test periods.* A test period lasted four minutes: $\frac{3}{4}'$ was allowed for T_1, $1\frac{1}{4}'$ for T_2, and $2'$ for T_3.

(*b*) *Giving the Tests.* At the word 'go,' the subjects, who were seated at tables, pencils in hand ready to begin, started T_1; at the end of $\frac{3}{4}'$, the word 'next' was used as a signal to proceed to T_2; at the end of another $1\frac{1}{4}'$ the word 'next' was again used as a signal to proceed to T_3; and after a further $2'$, at the word 'stop,' the subjects immediately ceased work and left the testing room. The tests were arranged so that they could not be seen until a page was turned, which was only done when the word 'go' was given.

Before working the tests at any period, the subjects stated on the cover of the test papers: (1) how they had spent the preceding hour; (2) their feeling of relative fitness (in four degrees); (3) any obvious factor, such as toothache, which might interfere with the results.

(*c*) *Instructions given to the Subjects.* These were general and special.

The *general* instructions were the two following:

α. They were to do their very best at every test period, no matter how tired they might feel.

β. They must observe absolute secrecy with regard to the tests until the whole experiment was over.

The *special* instructions concerned the methods to be followed in working the tests. Thus, the specified digits in T_3 were to be marked as the subjects reached them in proceeding *along* the rows: the subjects were not to mark all the fives in one row first, for instance, then all the threes in that row, and then the sixes. Mistakes were not to be corrected. Finally, the work had to be done as rapidly and accurately as possible.

(5) *Scoring the Tests.* As the results presented below are those for T_3 (almost entirely), it is not necessary to refer to the scoring of the other two tests. In T_3 each digit (presumably) inspected up to the last marked was counted as one unit and included in the results for quantity. The justification for this was that each horizontal row contained the same number of digits to be marked.

(6) *The Equal Groups.* The subjects were divided into two sets (A and B) and each of these was divided into two equal groups (A_1, A_2 and B_1, B_2). The A set consisted of the sixteen subjects who had taken part in the earlier experiment, while the new subjects constituted the B set. This division into sets was made largely because of the two different practice stages of the subjects in the tests. The A set was divided into two equal groups (A_1 and A_2) *on the total results for* T_3 *obtained on the fourth day of the first experiment*—the fifth (final) day would have been chosen but that one of the 16 students was absent on that day. Each A subject was thus given a position in a sub-group on the results of 8 separate test-periods at a fairly advanced stage of practice. The ranking of these subjects has therefore considerable reliability.

The B set was divided into two equal groups (B_1 and B_2) *on the results for* T_3 *of four separate test periods* (which had been preceded by two 'adjustment' periods given to familiarise the subjects with the tests).

The reason for using only one of the three tests in obtaining the equal groups was that no satisfactory combination of them could be effected. Owing to individual differences, two groups which were equal in capacity for any one of the tests were not equal in capacity for any other. It was therefore necessary to limit the investigations to one of the tests, and T_3 was chosen. Consequently, it is the results obtained from the equal groups for this test that are important, for it was only with regard to the capacity for this test that the groups were equal. It should be remembered, however, that the other tests were always worked at each test period before T_3.

The equality of A_1 with A_2, and of B_1 with B_2, is indicated by the figures in the following Table (I) which represent the *quantities* of work in test units[1] done by the various subjects in T_3. In obtaining the equal groups no account was taken of errors: that is, the equality of the groups was based merely upon quantity, not quality, of test output. The justification for this is that in the first experiment the errors in this test were extremely few (they varied between 0·53 and 0·68 of 1 per cent.[2] of the quantity of work).

TABLE I.

Equality of equal groups for T_3.

A_1		A_2		B_1		B_2	
Reference No. of Subject	Average quantity in T_3 (8 test periods)	Reference No. of Subject	Average quantity in T_3 (8 test periods)	Reference No. of Subject	Average quantity in T_3 (4 test periods)	Reference No. of Subject	Average quantity in T_3 (4 test periods)
1	564	9	548	17	377·5	26	402
2	523	10	542	18	368	27	344·5
3	515	11	520	19	342	28	333
4	502	12	503	20	329	29	327·5
5	481	13	470	21	327	30	322·5
6	465	14	460	22	318	31	322
7	450	15	436	23	307	32	315·75
8	389	16	402	24	305	33	305
				25	292·5	34	293·5
8 S's=3889		8 S's=3881		9 S's=2966		9 S's=2965·75	

(7) *Chronology of the Tests.* The chronology of this experiment was as follows. It lasted for five days (Nov. 3rd to 7th[3] incl.). On Nov. 3rd, the

[1] A test unit in this test=one digit in the blank. As each horizontal row contained 30 digits, a subject who had worked through 15 rows, for instance, would have done 450 test units of work. See this section, paragraphs (2) and (5).

[2] The errors here are the total errors for all the subjects at the respective hours, and similarly the quantity of work is the total work of all the subjects at the same hours.

[3] 1919.

B set was given the six preliminary tests referred to above (6); and the A set one readjustment test (to re-familiarise the subjects with the tests).

On the 4th, A_1 and B_1 were given the test at 11 a.m., and A_2 and B_2 the (same) test at 5 p.m. On the 5th A_2 and B_2 were given the test at 11 a.m. and A_1 and B_1 at 5 p.m. (Reversing the groups in this way was designed to prevent the occurrence of any difference between morning and afternoon results due to an unrevealed inequality between the 'equal' groups.) On the 6th and 7th, the A set continued this procedure: on the 6th, A_2 took the test in the morning and A_1 in the afternoon; and on the 7th, A_1 in the morning and A_2 in the afternoon. On these two days the B set followed a quite different procedure. One of the B groups rested ('did nothing') while the other continued its usual academic work on the first of these days, and on the second the group that had rested on the first day worked, and that which had worked rested; and during these days, both groups of the B set were given the tests 8 times a day (the times were the same as for the A set in the first experiment). The chronology of the test periods is set out in the following Table (II).

TABLE II.

Chronology of Test Periods.

Date	10 a.m.	11 a.m.	12 (noon)	1 p.m.	2 p.m.	3 p.m.	4 p.m.	5 p.m.	Remarks
Nov. 3rd	—	—	Bx	Bx	By	By	Ax By	By	Ax = Readjustment test for A set Bx = Adjustment tests for B set By = Tests for obtaining equal groups for B set
4th "	—	A_1 B_1	—	—	—	—	—	A_2 B_2	
5th "	—	A_2 B_2	—	—	—	—	—	A_1 B_1	
6th "	B_1B_2	A_2 B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	A_1 B_1B_2	(B_2 working, B_1 resting)
7th "	B_1B_2	A_1 B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	A_2 B_1B_2	(B_1 working, B_2 resting)

The 'doing nothing' consisted of various things, such as sitting about, chatting, taking 'gentle walks,' reading light literature, playing slight parlour games. 'Work' was definitely put aside, and no activity was entered upon seriously, but merely as a way of putting in time.

III. RESULTS.

The results are shown in the following table and figures. Table III shows the quantities of work done by the equal groups at 11 a.m. and 5 p.m. The figures given are the group averages in test units. In the last vertical column, the afternoon results are shown as percentages of those obtained in the morning. In Table IV are given the T_3 results for the working and resting groups (B_1 and B_2 on Nov. 6th and 7th). Here, again, the figures are the group averages at the various hours. Graphical representation of these results (both Tables) is shown in Figs. 1 and 2.

TABLE III.

The 11 *a.m and* 5 *p.m. Results.*

Date	11 a.m. (T_3) group average and standard deviation (*s. d.*)	5 p.m. (T_3) group average and standard deviation (*s. d.*)	Difference between a.m. and p.m. results (in test units)	P.E. of difference	5 p.m. results as percentage of 11 a.m. results (T_3)
Nov. 4th	A_1=481·1 *s. d.* A_1= *69·4*	A_2=499·5 *s.d.* A_2= *53·2*	18·4	7·4	in A groups=103·8
	B_1=377·7 *s. d.* B_1= *36·3*	B_2=391·0 *s. d.* B_2= *29·6*	13·3	3·5	in B groups=103·5
5th	A_2=538·5 *s. d.* A_2= *46·3*	A_1=503·0 *s. d.* A_1= *63·0*	35·5	6·6	in A groups= 93·4
,,	B_2=411·3 *s. d.* B_2= *20·6*	B_1=387·3 *s. d.* B_1= *40·3*	24·0	3·4	in B groups= 94·2
6th	A_2=556·6 *s. d.* = *61·2*	A_1=524·8 *s. d.* = *61·6*	31·8	7·0	in A groups= 94·3
7th	A_1=540·5 *s. d.* = *58·1*	A_2=533·5 *s. d.* = *38·6*	7·0	5·9	in A groups= 98·7

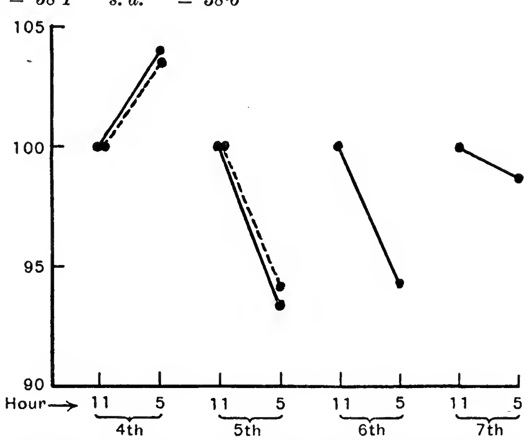


Fig. 1. The dots joined by broken lines are the B groups, those joined by solid lines the A groups. Ordinates indicate group averages for quantity. These are shown in percentages, as given in the right-hand column of Table III.

Table IV.

Results for Working and Resting Groups.

The figures show the group averages and standard deviations (*italics*) in test units.

Date	Group	Hour of Day: 10 a.m.	11 a.m.	12 (noon)	1 p.m.	2 p.m.	3 p.m.	4 p.m.	5 p.m.	
Nov. 6th	B_1	431·7	430·9	435·7	450·1	451·2	437·6	433·9	437·4	B_1 Resting
		32·52	*45·64*	*49·48*	*56·77*	*42·56*	*25·48*	*43·37*	*36·71*	
,,	B_2	425·4	422·5	432·5	436·4	441·3	422·7	419·5	428·8	B_2 Working
		31·37	*31·91*	*33·29*	*29·61*	*38·79*	*43·80*	*37·06*	*34·65*	
Nov. 7th	B_2	462·4	478·9	483·1	485·2	471·2	467·3	471·9	500·1	B_2 Resting
		36·40	*40·67*	*38·60*	*41·90*	*43·78*	*31·10*	*16·37*	*19·42*	
,,	B_1	454·0	464·7	465·9	472·5	469·7	461·5	464·3	472·2	B_1 Working
		46·06	*25·75*	*50·45*	*43·91*	*57·19*	*44·57*	*24·98*	*41·93*	

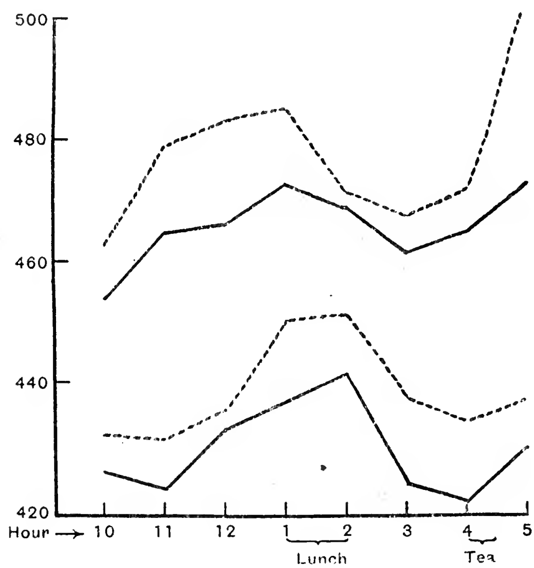


Fig. 2. The broken lines are the resting groups, the solid lines the working groups. Ordinates indicate group averages (Table IV). The two upper curves refer to Nov. 7th, the two lower curves to Nov. 6th. (Practice accounts for the fact that the curves for the 7th are above those for the 6th.)

IV. Discussion of Results.

(1) *The* 11 *a.m. and* 5 *p.m. results*. These show a smaller output in the afternoon than in the morning on three out of the four experimental days, though on one of the three (Nov. 7th) the difference is very small. On the remaining day (Nov. 4th) the smaller output belongs to the morning. In most cases the differences are statistically significant. On Nov. 5th the difference for the *A* groups is more than five times, and for the *B* groups more than seven times, its 'probable error.' In two cases (the *A* groups on the 4th and 7th) the differences are less than three times their P.E.'s; but the defect from this standard is not great on the 4th, and the circumstances of the 7th were somewhat exceptional, as will be pointed out presently.

These differences between the morning and afternoon output seem definitely *not* to have been due to certain factors. Thus, (1) they were not due to *practice* (on Nov. 4th), as the afternoon groups (on that day) were in exactly the same practice stage as the morning groups. (2) Since the subjects were unusually trustworthy and were instructed to do their utmost on every occasion, the differences were not due to *differences in volitional effort*. (3) *Inequality between the morning and afternoon groups in capacity for the test* is ruled out as an explanation by the method adopted in obtaining the groups. Further, (4) as the records showed that on *every* day *the feeling of weariness* was greater in the afternoon than in the morning, the differences (on Nov. 4th, at least, and therefore possibly on the later days) were not due to differences in the intensity of this feeling. (5) As to *environmental conditions*, the variations in these seemed to be very small; and it has been shown that within certain wide limits such conditions as temperature and humidity do not appreciably affect work when subjects are definitely trying to do their best[1].

Were the differences due to fatigue? If we accept the usual interpretation of output figures and suppose that they were, we have to explain the results on Nov. 4th, when the larger output was in the afternoon. Do these results indicate that some factor interfered with the effect of fatigue on this day, and that tests of the nature of T_3 are therefore of no value for fatigue investigation?

The similarity of the results obtained from the *A* and *B* groups on Nov. 4th is very great[2], although the *A* groups were then at a practice

[1] (1) Thorndike, McCall and Chapman, *Ventilation in Relation to Mental Work*, 1916; (2) L. I. Stecher, *The Effect of Humidity on General Efficiency*, 1916.

[2] See Fig. 1.

stage far in advance of the B groups. If some factor interfered with the effects of fatigue on that day, it evidently affected both pairs of groups in the same way (the members of both pairs were doing identical academic work). Investigation elicited the fact that on the morning of the 4th, during the hour immediately preceding the 11 a.m. tests (*i.e.*, from 10 a.m. to 11 a.m.), the subjects attended a lecture which they considered the hardest work of the week[1]. It is therefore possible that on this day the subjects were especially fatigued at 11 a.m. and had partly recovered at 5 p.m. If so, this day's results strengthen the suggestion that T_3, in these conditions, is an indicator of fatigue. If not, the difference in output must here, and perhaps on the other days also, be attributed to unknown factors.

Although practice could not have affected the results of Nov. 4th, it *may* have reduced the difference in output of the morning and afternoon groups on Nov. 7th. These groups had each had three test periods (which would act as further practice periods) during the three days immediately preceding Nov. 7th. Each group had had one afternoon and one morning period; but one had had its third period in the morning, the other in the afternoon. On the 7th, the former group took the test at 5 p.m. If fatigue were operative at all, the morning test periods may have had a greater practice effect than the afternoon periods, and hence the afternoon group on Nov. 7th may have been slightly favoured. Had there not been this difference in the times of the test periods, the difference between the output of the morning and afternoon groups on the 7th might well have been greater than it was: the morning group might have done more than it did, and the afternoon group less.

While these results suggest that, for certain types of work, a test similar to T_3 would show fatigue, they do not allow us to deduce this with certainty, because we have no certain knowledge as to the degree of fatigue present at 11 a.m. and 5 p.m. on the four experimental days. The results thus emphasize a constant difficulty, and demonstrate that *the worth of any suggested fatigue test can only be determined if the degree of fatigue present when it was applied is known independently of the test results.*

(2) *Results of the Working and Resting Groups.* The effects of work are shown in the relatively low output of the working group at every hour of the day. The actual differences in output between the working and resting groups at each hour (in test units) together with their probable errors are given in Table V.

[1] There was a difficult lecture from 9 a.m. to 10 a.m. also on this day.

TABLE V.

Actual differences in test units between the working and resting groups at the various hours on Nov. 6th and 7th, together with the probable errors of these differences. (Actual differences are in favour of the resting group at every hour on each day.)

Date		Hour 10 a.m.	11 a.m.	12 (noon)	1 p.m.	2 p.m.	3 p.m.	4 p.m.	5 p.m.	Remarks
Nov. 6th	Actual Diff.	6·3	8·4	3·2	13·7	9·9	14·9	14·4	8·6	*Mean Diff.* = 9·9
"	P.E. of Diff.	2·78	4·15	4·47	4·76	4·31	3·81	4·26	3·75	*Mean P.E.* = 4·04
Nov. 7th	Actual Diff.	8·4	14·2	17·2	12·7	1·5	5·8	7·6	27·9	*Mean Diff.* = 12
"	P.E. of Diff.	4·36	3·53	4·77	4·52	5·38	4·03	2·23	3·43	*Mean P.E.* = 4·03

The size of the actual difference relative to that of its probable error would render it significant at most hours on each day, even if the results at any one hour were regarded without reference to the results at other hours. But it will be seen that the *mean* actual difference is approximately two-and-a-half-times its probable error on Nov. 6th and three times its probable error on Nov. 7th. Further, when it is recalled that the rôles of the groups were reversed on these two days, and that the superior group while resting was the inferior group while working, the evidence is unquestionable that at least a certain kind of mental work has a deteriorating effect upon the capacity to do mental work similar to that involved in test T_3 (compared with the efficiency of the 'resting' condition).

But perhaps the most important feature in these results is the similarity in the general shape of the curves for the two groups on each day. The rises and falls correspond precisely: there are falls in the curves for the resting groups just as there are in those for the working groups. Are these rises and falls significant? If the curve for either group (on either day) is analysed without reference to the curve for the other group, its variations from its own mean value are so small as to be of little significance: that is, had we only the curve for one of the groups, we could infer little if anything from its rises and falls, since these are on the whole within the range of chance variations. Taking the two curves (for either day) together, however, we get an entirely different situation. The similarity between them, apparent to observation, may be expressed precisely by their correlation coefficient. This is very high. On Nov. 6th, the r of the two sets of results[1] is $+\cdot 88$ ($p.\ e. = \cdot 052$), and on Nov. 7th,

[1] r = the Pearson coefficient.

it is + ·90 ($p.\ e.$ = ·045). There is thus no doubt about the significance of the rises and falls in the curves, and the only question is how they are to be interpreted. The rises may be passed over briefly, as practice would account for them; we may assume that they represent simply a condition in which practice effects were not inhibited. The important features are the falls. Such falls are usually explained by fatigue; but *as they occur in the curves for the resting as well as in those for the working groups*, this explanation does not seem possible here. They indicate that some factor simultaneously neutralised, or more than neutralised, the practice effects in the two groups, and then gradually tended to lose its inhibitive power. This factor does not seem to have been the feeling of weariness, since the curves for this and output in the test do not correspond; apparently it was not a diminution of volitional effort; and it was not temperature nor humidity[1].

Were the falls caused by the taking of food? This seems a probable explanation in view of the altered metabolism which the taking of food would involve. If correct, some special explanation must be found for the Nov. 6th results (Fig. 2) which show the largest output for the day in the first test period after lunch. Possibly the character of the lunch on Nov. 6th was different from what it was on Nov. 7th[2]. At any rate, in the earlier experiment a relatively low output immediately after lunch was the rule, so that there probably is some special explanation of the result on Nov. 6th.

One difficulty in explaining the falls by the taking of food is that the output (in all the experiments) tended *to remain low* during the whole afternoon—notwithstanding some slow recovery. Could the whole of this effect have been produced by the lunch? To the suggestion that fatigue helped to keep the output low in the later afternoon hours, it may be replied that the low afternoon output was on the whole characteristic of the resting group as well as of the working group. Consequently, if fatigue were the factor operative here, it was *not* fatigue *caused by 'work.'* There is at least one other difficulty about the taking of food as an explanation. On Nov. 6th there was a slight fall in the curves from 10 a.m. to 11 a.m. (Fig. 2). Here the suggestion that either the taking of food or fatigue was the cause of the fall seems improbable: for breakfast had been taken probably three hours before the 11 a.m. test on this day (and on Nov. 7th too, when there is a definite *rise* from 10 a.m. to 11 a.m.).

While, therefore, the falls under discussion may have been largely

[1] There was a difficult lecture from 9 a.m. to 10 a.m. also on this day.

[2] This has been suggested by Prof. E. L. Collis, M.D.

due to the taking of food, this cannot be maintained with any assurance. It seems best at present to say that the causes of these falls are not known.

These results thus seem to prove, not merely that a relatively low output need not indicate fatigue, but that it may not indicate any of those factors now accepted as sporadic determinants of a performance, such as a relatively small volitional effort or an intense feeling of weariness. The effects of fatigue appear to be superimposed on those of variations in a 'work mechanism,' the efficiency of which varies in a characteristic manner whether we work or rest.

This result supplements Hollingworth's findings[1] and leads to some modification of his conclusions. On the basis of his results, obtained from thoroughly practised subjects, he concludes that such variations as occur in the efficiency of the 'work mechanism' are not due to diurnal or to organic rhythms (since they occur whether a day's work begins at 7.30 a.m. or 10 a.m.) but to work. The results here presented indicate that these variations occur *even in the absence of work*. It would be important to investigate (with special subjects) their precise nature for different human capacities (Hollingworth found that the work mechanism for motor capacity showed different changes from that for mental capacity) and also, if possible, their determinants.

Whatever be its cause, there is strong evidence for the presence of a diurnal or organic rhythm in the foregoing results.

If we neglect the 10 o'clock results on Nov. 6th and the 5 o'clock results on Nov. 7th, we get curves, covering 6 hours for both days, almost identical in shape. They do not refer to the same six hours for the two days: the curves for the 6th refer to the time from 11 a.m. to 5 p.m., those for the 7th to the time from 10 a.m. to 4 p.m. This strongly suggests the operation of *some* rhythm—earlier by an hour on the 7th than on the 6th.

Results similar to these were also obtained in the first experiment, though here there was no control group of resting subjects: that is, in the first experiment, as well as in the second, *working* subjects yielded differently-shaped curves (for the same test) on different days. It is a fair inference from the results shown in Fig. 2 that the curves of *these working* subjects are typical of the curves which would have been obtained on the same days from *resting* subjects. It would therefore be of interest to make tests over the whole twenty-four hours, or at least over a much larger number of hours than was covered in these experiments.

[1] "Variations in Efficiency during the Working Day," *Psychol. Rev.* 1914, xxi. 473–491.

Some importance attaches to the results for T_1 and T_2 on Nov. 6th and 7th. As the working and resting groups were not equal in capacity for either of these tests, the curves for the resting group are not constantly higher than those for the working group (as they are for T_3, with regard to the capacity for which the groups were equal). The relative heights of these curves at any hour should, therefore, not be considered of any particular significance. The significant feature is general similarity in

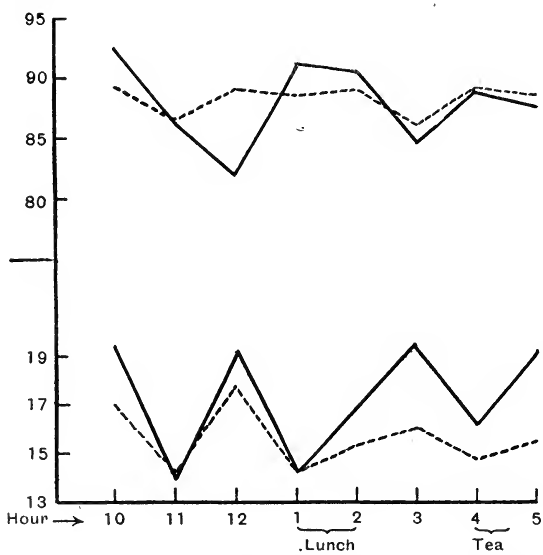


Fig. 3. The broken lines are the resting groups, the solid lines the working groups. Ordinates indicate group averages. The upper curves are for T_2 on Nov. 6th, the lower curves for T_1 on the same day.

form over the whole day. The curves are shown in Fig. 3. It is at once obvious that they strengthen the idea that characteristic variations in efficiency[1] occur during a day in addition to variations produced by fatigue *due to 'work.'* And this idea gains still further probability from the following facts.

In order to test the reliability of the group average curves obtained in the first experiment, the total group of twenty subjects (all working) was divided (after the experiment) into two chance groups of ten each, and the average results of these chance groups were compared. The

[1] These variations would be different for different capacities: at least, this is suggested by such differences as those between the T_1 curves on Nov. 6th (Fig. 3) and the T_3 curves for the same day (Fig. 2).

chance groups were obtained by arranging the subjects in alphabetical order, and taking the first ten as one chance group and the second ten as the other. Samples of the curves thus obtained are shown in Fig. 4; *X* is one chance group and *Z* the other.

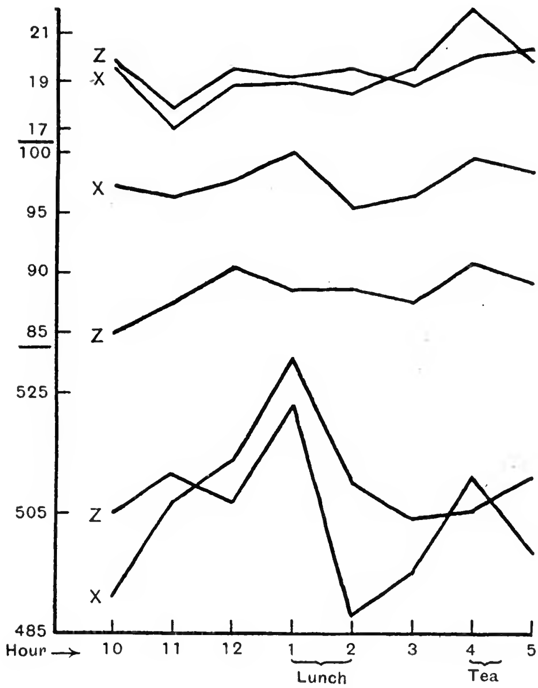


Fig. 4. *X* and *Z* = the chance groups in the earlier experiment. The upper pair of curves are for T_1 (June 27th), the middle pair are for T_2 (June 26th), and the lower pair are for T_3 (June 27th). Ordinates indicate group averages.

These curves are very similar for any given test on any given day. The observed similarity may be given precise numerical expression by means of correlation coefficients between the chance groups, and these are always positive and generally high. Thus, the *r* for the chance groups for T_1 on June 27th (Fig. 4) is $+ \cdot 76$ (*p. e.* $= \cdot 10$), for T_2 on June 26th (Fig. 4) it is $+ \cdot 70$ (*p. e.* $= \cdot 12$), for T_3 on June 27th (Fig. 4) it is $+ \cdot 53$ (*p. e.* $= \cdot 13$). Now, the members of these chance groups were all engaged upon work during the day, and further upon the same work. The similarity of their average curves is therefore not surprising. But—what is somewhat surprising—the curves for the *resting and* *working groups* for the tests in which they were *not equal* are, on Nov. 6th and 7th, also very similar. Thus, the curves for T_1 on Nov. 6th (Fig. 3), almost as similar as those for T_3 on Nov. 6th and 7th (Fig. 2), are *more* similar than any pair of curves obtained from the chance groups in the earlier experiment. The r for these T_1 results is $+ \cdot 81$ (*p. e.* $= \cdot 083$), whereas the highest r for the chance groups was $+ \cdot 76$ (*p. e.* $= \cdot 10$). And *in general* the curves of Fig. 3—not to mention those of Fig. 2—*representing working and resting conditions* are quite as similar as the pairs of curves in Fig. 4, which represent the test output of two chance groups *both working*.

A further question of some interest is whether the effects of work gradually became greater as the day advanced—whether fatigue was progressive. Although the appearance of the curves (Fig. 2) does not make the occurrence of such an increase in the effects of work obvious, analysis of the actual figures tends on the whole to support it. On Nov. 7th, it is true, the mean afternoon difference between the two groups is actually less than the mean morning difference; but if the results for Nov. 6th and 7th are taken together, then the mean afternoon difference for the two days is greater than the mean morning difference, though by a small amount only. More important, perhaps, is the fact that if the results for the two days are taken together, the mean difference between the groups for the last two hours of both morning and afternoon is greater than the mean difference for the first two hours: that is, the effects of work seem to be greater in the late stages than in the early stages of a work period. This is in accord with the current view; but it is worth noting that the results for the afternoon of one of the two days (Nov. 6th) are not in accord with this view, and also, as was stated above, that the mean afternoon difference between the groups on Nov. 7th is actually less than the mean morning difference.

On the whole, therefore, these results afford no very convincing evidence that the effects of fatigue, as judged by diminished capacity for work, are progressive throughout a day's work.

Finally, there is a practical consideration. If fatigue was the cause of the differences between the 11 a.m. and 5 p.m. results (Fig. 1), it must have been operative in reducing the output of the *working* group on the afternoons of Nov. 6th and 7th (Fig. 2). But since the curves for the resting groups are, on these days, practically identical in shape with those of the working groups, we may reasonably infer that had *control* (resting) subjects been used in connexion with the 11 a.m. and 5 p.m. results, they would have shown similar differences to the working subjects; that such control subjects would have done *more* than the working subjects both at 11 a.m. and 5 p.m., but that, as with the working subjects, their 5 p.m. output would generally have been *smaller* than their 11 a.m. output. Either, then, such differences are not caused by fatigue, or, if they are, we cannot argue from them as to the relative strenuousness of a subject's work. That is to say, it must be futile to compare the test output of subjects at one time with their test output at another time[1] if we wish to obtain any indication of the hardness of their work, as there will be, within limits, a similar difference in output whether they work or rest. What seems to be necessary is to compare the test output of *working* subjects at any given time with that of *resting* subjects *at the same time.*

V. Conclusions.

1. Possibly as a consequence of the production of fatigue, continuous mental activity, such as is involved in academic study, definitely lowers the capacity for certain mental tests (type T_3, at least).

2. It is impossible to estimate accurately the value, as fatigue tests, of the tests used in these experiments, because positive knowledge of the degree of fatigue present when the tests were applied was unobtainable. There is no doubt, however, that with reliable subjects such tests may yield highly important information (see 3).

3. Within the meanings of 'work' and 'rest' relevant to the conditions of these experiments, similar variations in the capacity for the performance of a task may occur whether a person works or rests; and these variations are probably different for different capacities. This conclusion is especially pertinent to the interpretation of output and accident curves.

4. Practice must be eliminated before any performance test can be used to diagnose fatigue. For this purpose, the method of equal groups is probably the best, and can readily be used in the office or factory.

5. It is essential to make use of control subjects in fatigue experiments. The contrary procedure, which is universal, may lead to fundamentally misleading conclusions—for instance, in the interpretation of the falls in output curves.

VI. Appendix.

I am indebted to Miss S. C. M. Sowton for the following results. Miss Sowton first worked through T_3 145 times (preceded on each occasion by T_2), the test periods being distributed over seven days. By the end of this time practice effects, though not absent, were relatively

[1] On the same day.

small. The test was then worked for a further three days (21 times a day at intervals of about half-an-hour, and always preceded by T_2) during which no work whatever was done in the intervals between the tests. The composite curve for these three days is here given. Miss Sowton worked through the whole test blank at each period; and efficiency is therefore measured by the relative smallness of the time taken for the test. In the figure, the ordinates are the times required for working T_3, the abscissæ the hours at which the test was worked (the intermediate points on the curve represent the results of tests at intermediate times).

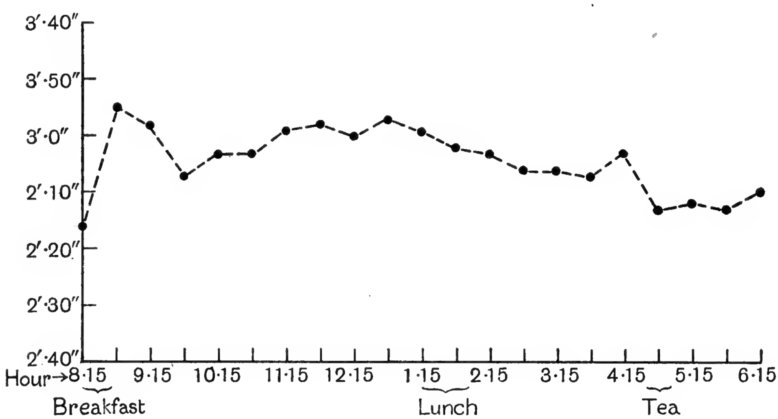


Fig. 5.

It will be seen that efficiency for the test gradually becomes lower during the day. Notwithstanding that the only work on these days was that involved in the doing of the tests,—which amounted to not more than 7 minutes per half-hour,—the test results from 4.15 p.m. onwards are definitely poorer than any other test results during the day with the exception of the first. This tends to confirm the position stated in the last paragraph of Section IV above.

(*Manuscript received* 18 *March*, 1920.)

PUBLICATIONS RECENTLY RECEIVED

Aesthetics: A Critical Theory of Art. By Professor HENRY G. HARTMAN. Columbus, Ohio: H. G. Adams and Co. 1919. pp. 250.

In this book an attempt is made to determine the elements of interest in painting, music and poetry. The author maintains that to attack questions of aesthetics from a purely general standpoint is misleading, and in strictness the book should be called "a critical description of certain of 'the arts.'" Thus all of the first part of the treatise is really introductory to the detailed study of painting, poetry and music which occupies the last three chapters. It is throughout urged that questions concerning the 'substance' of an art—a conception which is nowhere very clearly defined—its technique, conventions and material, deserve equal consideration with problems of individual expression and enjoyment. Consequently psychological aesthetics receives from the author much criticism, of which some is good, and most is interesting. The book is written with the most complete confidence imaginable; but it is both readable, and, for the greater part, worth reading.

Lehrbuch der experimentellen Psychologie. By Professor J. FRÖBES, S. J. Freiburg im Bresgau: Herdersche Verlagshandlung. 1917. Vol. I. pp. xxvii + 605. 9*s.*: bound 10*s.* 9*d.*

The author, now professor at a theological college, learnt his experimental psychology in G. E. Müller's laboratory at Göttingen. The first volume of his text-book imitates his master's work in thoroughness of method and conscientiousness to detail, but it is lacking in originality and system. It consists of a patch-work summary of the work of recent investigators, whose names are mentioned on every page, but generally without reference to their published papers, and without adequate recognition of British and American contributions.

Report of the Care and Control of the Mentally Defective and Feeble-Minded in Ontario. By the Hon. F. E. HODGINS. Toronto: A. T. Wilgress. 1919. pp. 236.

The writer, a Justice of Appeal, was appointed by Royal Commission to consider and inquire into the existing methods of dealing with imbecile, feeble-minded, or mentally defective persons in the Province of Ontario, and to make recommendations in regard thereto. The report contains not only a survey of the present conditions in Ontario and a scheme of proposals, but also an interesting account of the work carried on in the United States and in this country, and reprints of a number of papers by T. W. Salmon, Adolf Meyer and others on mental deficiency in an appendix.

Two New Papers on Hypnotism. By PATRICK P. DALTON, of University College, Galway. Privately printed, N.D. pp. 12. 1*s.* 7½*d.* post free.

Of the two papers contained in this pamphlet, the first is an address to the Galway Medical Faculty on the value of hypnotism as a therapeutic agent. The author rightly distinguishes two methods of employing hypnosis, (*a*) for direct suggestion, (*b*) for the exploration of repressed experience. The second paper is so different in character that it should not have been issued with the first. It is entitled "Investigations into the state of the brain in Hypnotism, and the nature of Clairvoyance." It contains no proof of clairvoyance, beyond the author's statement of its occurrence. Such sentences as "informs me that all the area of the brain in front of the ascending frontal convolution is superficially awake in fairly deep hypnosis," "Touch centres very active (can feel pores on my skin)," and his subjects' description of the "second self" as a "blue cloud" or "a cloud of dark smoke" which "he can send anywhere he pleases when in a hypnotic sleep" will suffice to indicate the general nature of this paper.

Man, Past and Present. By A. H. KEANE. Revised and largely re-written by A. HINGSTON QUIGGIN and A. C. HADDON. Cambridge: University Press. 1920. pp. xi + 582. 36*s.* net.

Mrs Quiggin and Dr Haddon deserve the thanks of all interested in anthropology for bringing this well-known work up to date. Certain sections have had to be entirely rewritten and in many places pages have been suppressed to make room for important information. Full references are given in footnotes to original papers. Sixteen plates of racial types are placed at the end of the book. The volume will rank as a standard text-book on physical and social anthropology. One on the psychological aspect of the subject has still to be written.

Die Ursprünge der Metapher. By HEINZ WERNER. Leipzig: W. Englemann. 1919. pp. 238.

This monograph on the origins of the metaphor forms the third volume of the series *Arbeiten zur Entwicklungspsychologie* edited by Professor Felix Krueger of the University of Leipzig. As the author points out in the preface it is an enlarged section of a work of his, as yet unpublished, on the origins of lyrical poetry.

In the introductory chapter the author formulates the conception of genuine metaphor as the paraphrase of an experience, sentiment or object in terms of another with the characteristic feature of a consciousness on the part of the speaker of the duality of the versions, and of the awareness that the paraphrase is not a real equivalent of or effective substitute for the original.

Genetically the genuine metaphor is preceded, according to the author, by certain forms of "pseudo-metaphors," arising among primitives either from their inability to abstract, or the limitations of their power of expression, both of which lead to paraphrases of things, acts and experiences. Neither, however, is a form of genuine metaphor as neither achieves the duality and conscious distinction between original and paraphrase but both on the contrary are intended to be actually equivalent and even identical. With the anthropomorphic stage of culture and the appearance of magic a complete readaptation of the psychic, especially emotional, life sets in, which for the first time provides the psychological basis for an attitude capable of the dualism requisite for the development of metaphorical speech. This dualism reaches its fullest growth in the cultures which stand under the rule of some tabu. Indeed the whole tabu-adaptation, with its fears, avoidances and repressions and the consequent tendency to lies, fictions, allusion and illusions, is essentially the soil in which metaphor flourishes. So far from being, as might naïvely be supposed, an aid to clearness, explanation and amplification, the metaphor appears as a means of obscuring and hiding the real meaning of the speaker, allowing it to be understood only by the initiated or by the victim against whom he directs his words. As the ideal form of such partial concealment and partial revelation of the meaning, the author pursues the metaphor with a great wealth of illustration and of psychological distinctions through all its various stages from the tabu of names with the complete avoidance of their use, to the alteration of names by additions, by the metathesis of syllables, by alliteration and rhyme, the complete substitution of names and the development of whole tabu-languages; to the phrase metaphors; to the use of metaphors in threats and the transition from scorn to irony and from irony to flattery. In order to prove this, his main thesis, that metaphor has sprung originally from the usages of tabu, the author shows that in primitive poetry the metaphor is characteristic of tabu-cultures and, in his fifth chapter, that the occurrence of metaphorical forms of speech is coincident in its ethnological distribution with the civilisations based on tabu. In chapters 6 and 7 he discusses the various forms of complex metaphors with illustrations from primitive poetry and speech, and various extraneous aids to metaphorical concealment, either linguistic as the use of the question or the play on words, or the more ideal means as allusion, innuendo and pseudo-logic (*Scheinlogik*). His last chapter gives a summary of the genesis of the metaphor and the various paths of its development to the purely poetic and artistic use with which we are familiar in the poetry of civilised races.

The work is one of those monographs which students of the origins of literature, poetry and art will heartily welcome as offering an account of the psychological changes which must be postulated as lying at the back of those forms of aesthetic activity. That some very complex shiftings of motives—the *Motivwandlungen* of the author—must have taken place before anything in the nature of art or poetry in our sense of those terms can have come into existence, has been suspected for some time. Here we find an account, not only amply supported by evidence, but convincing in itself, of these very subtle and very elusive changes and substitutions. It is all the more attractive as an argument of a more general kind, as its material is the apparently so harmless and poetically so respectable practice of metaphorical paraphrase.

Psychoanalysis: a brief account of the Freudian theory. By Barbara Low. London: George Allen and Unwin. 1920. pp. 191.

As Dr Ernest Jones states in his preface to this volume, "That the deductions made from psychoanalysis are both novel and not easily acceptable, Miss Low makes plain in her book, and she has not adopted the easier way of concealing these attributes of them. She has chosen the loftier aim of attempting to present all aspects of the psychoanalytical theory fairly and straightforwardly, and yet to bring them within reach of those who have made no previous study of the subject. I can answer for it that she has performed the first part of this task successfully, and can only hope that her readers will find that she has performed the second part with equal success."

The difficulty of achieving this success lies not so much in the arduousness of psychoanalysis as in the fact that it has to be studied from a point of view which is entirely new and quite different from that of every other science. Miss Low therefore wisely begins her book with a chapter on "The Scope and Significance of Psychoanalysis," which clearly explains the psychoanalytic point of view. Then comes a review of "Mental Life" with its unconscious and conscious mechanisms, followed by a discussion of "Repressions," the pleasure-principle and the reality-principle, the egocentric and the social impulses, sublimation and the sexual theory. The remaining chapters deal with dream interpretation, treatment by psychoanalysis and prospective social and educational results.

To those who are unfamiliar with the fact that Freud is not merely the pioneer of psychoanalysis but is also the discoverer of nearly every mental fact and mechanism laid bare by the method, it may become rather tedious to come across Freud's name on practically every page; and many readers may wince at the German tendency to the excessive use of capital letters. Here and there, too, one might find fault with the English; for example, "fantasying." There is no verb 'to fantasy' in English, though there is in German. But these are minor defects. Miss Low's is the best elementary book yet published on psychoanalysis.

An Examination of William James's Philosophy. By J. E. Turner. Oxford: B. H. Blackwell. 1919. pp. vii + 76. 4*s.* 6*d.* net.

The writer of this brief critical essay confines himself to the consideration of the philosophical theories of Pragmatism and Radical Empiricism which James put forward in the last ten years of his life. He hardly does more than point out the paradoxes, which are and of course were meant to be obvious, although he evidently believes that he has discovered them.

The Field of Philosophy. By Dr Joseph A. Leighton. 2nd edition, revised and enlarged. Columbus, Ohio: R. G. Adams and Co. 1919. pp. xii + 485. $2 net.

A small, but comprehensive, text-book for the beginner, written in a clear and straightforward manner.

Treatment of the Neuroses. By Dr Ernest Jones. London: Baillière, Tindall and Cox. 1920. pp. viii + 233. 10*s.* 6*d.* net.

The title of this book does not indicate its full contents. Dr Jones has here given us a valuable account not only of the principal methods of treating the neuroses, but also of the pathology and symptoms of these disorders. The greater part of the book consists of a long chapter devoted to the consideration of hysteria in which "to avoid needless repetition the principles of psychopathology and of mental therapeutics have been dealt with at some length under one heading..., so that the chapters on the other neuroses are correspondingly short." A short chapter on the psychoses and various habits and aberrations concludes the volume.

Throughout, as might be anticipated, the point of view is thoroughly Freudian. The sexual basis of the neuroses is hence brought prominently to the fore. For example, hysteria is attributed to suppressed sexual wishes, neurasthenia (in its narrower, more literal sense) to excessive sexual self-abuse accompanied by intense moral conflict, hypochondria to "excessive erotogenicity of certain areas of the body," alcoholism to repressed homosexuality, anxiety neurosis to "undue sexual excitation...and inadequate relief," anxiety hysteria (e.g. most phobias) to the symbolization of some sexual situation arising in the early development of the psycho-sexual life, obsessional neurosis to buried self-reproach dating from childhood and referring "to certain sexual performances or tendencies," war neurosis generally to "repressed narcissism."

But the truth or error of such pathogeny hardly affects the value of the book. It will for long rank as a standard work upon the various forms, physiological as well as psychological, of treatment of the neuroses. Dr Jones has achieved his aim with surprising impartiality, considering the strong views which he obviously holds on the prepotency of Freudian psychoanalysis. The psychologist may object to his statement that "there is plainly no psychological difference between" suggestion and persuasion, which he bases on the non-psychological ground that "it is obviously a matter of convention or of external and irrelevant circumstances whether a given suggestion is to be considered reasonable or not" (p. 69). Some hypnotists may complain that their special methods of employing hypnosis, not for suggestion but for exploration and subsequent "conscious assimilation of dissociated elements," has not received adequate notice at Dr Jones's hands. Believers in 're-education' methods may resent his conclusion that "probably the greater part of" their "success...is produced by the action of suggestion" (p. 101).

Such criticisms, however, only serve to indicate the extreme interest of the book. It is written in the author's usual clear style and for the most part in non-technical language, so that it serves not only as a compendium of various modes of treatment but also as an excellent introduction for the general public to the psychoanalytic standpoint, written as it is by an ardent Freudian who has had considerable personal experience of most of the therapeutic methods in vogue.

The Foundations of Music. By Dr H. J. Watt. Cambridge: University Press. 1920. pp. xiv + 240. 18*s.* net.

Dr Watt's work is to be welcomed for the guidance which it affords to those who realise that the science of psychology is coming with big strides not to uproot, but to study the rules of harmony. The author is always careful not to detract from what may be learned from the great composers and theorists. In his preface he points out that the foundations of music are not complete until a statistical study of the great composers has been conducted.

After a preliminary survey covering the ground of his earlier book, Dr Watt discusses consonance and dissonance of tones and their fusion. Next, the consonance and dissonance of successive tones is dealt with, and interesting examples are quoted from Schubert's songs which support his statement that smallness of interval is a factor favouring continuity or melodic progression. Beethoven's works might have also been cited, for in them countless passages of successive scale notes are to be found: particularly may be noted the upward scale in the Scherzo of the B♭ Pianoforte Trio, and the downward scale in the Scherzo of the *Eroica* Symphony.

In his discussion of consecutive fifths, it is pleasant to notice that with this as with other harmonic questions Dr Watt deals broadly, even humanely. His psychological sense has saved him from regarding this sequence as necessarily bad. He writes: "The pleasantness of fifths in a certain setting by no means discredits their prohibition under most circumstances. This could be gainsaid only by the pedant who lives on rules and does not apprehend the structures he studies in their primary aspect—aesthetically—at all. But the sole standard of art is the beauty inherent in the created object." In his analysis of this sequence, after pointing out that the prohibition appeared early in the history of the art and much earlier than the formulation of the connexions of inversions, Dr Watt appends the following as the chief theories of prohibition: habit and tradition; excessive sweetness; want of variety; ambiguity of key or tonality; want of relationship; the nature of the interval itself; want of balance. Of these the author appears to favour the last but one.

From a table of progressions prepared by Dr Watt, he says that the following conclusion may be drawn: "If due consideration is given to the prominence of the pair of voices that bear the interval in question, it appears that the immediate repetition of an interval in the same voices is the more offensive the greater the consonance and dissonance of that interval. The point of minimal unpleasantness or of maximal pleasantness (as the case may be) in the series from greatest consonance to greatest dissonance lies amongst the thirds and sixths. These intervals may, therefore, be held to be fusionally neutral." This is interesting when we recall the frequent use of progressions in thirds and sixths to be found in Brahms's music.

Subsequent chapters deal with hidden octaves and fifths, common chords, and melodic motion in relation to degrees of consonance. Independently of Gaudentius, Dr Watt arrives at apparently similar ideas of the properties of 'paraphony.' "In symphony the tones of an interval tend to become indistinguishable through too much unitariness or fusion; in diaphony they sound through or against one another; in paraphony there is balance, so that melodies formed of such intervals will flow evenly side by side, the one not inhibiting the apprehension of the other."

Dr Watt may deservedly lay claim to being a pioneer. Differences of opinion may be held here and there from his views or deductions, but appreciation must be rendered to his thoughtful studies and to the labour he has devoted to them.

Introductory Psychology for Teachers. By Professor EDWARD K. STRONG, jun. Baltimore: Warwick and York. 1920. pp. xii + 233. $1.60.

This book consists of forty-four "lessons" intended to interest school teachers in psychology considered as the science of behaviour. It is an unconventional work, in which a great deal of material, some of it good and much of it bad, is put together in rather a happy-go-lucky manner. As a basis for lectures it might be found helpful.

Graphology and the Psychology of Handwriting. By Professor JANE E. DOWNEY. Baltimore: Warwick and York. 1919. pp. 142. $1.60.

This is a short but serious study of the possibility of using handwriting tests for purposes of mental diagnosis. The author finds that variability in size, slant. alignment and other graphic characteristics may probably afford some indication of typical ways of controlling motor impulses. She candidly states that: "Chiefly, our results are of value in that they outline a programme for further investigation," and it must be admitted that she makes out a very good case for such additional research.

Human Psychology. By Professor HOWARD C. WARREN. New York: Houghton Mifflin Company. 1919. pp. xx + 460.

Prof. Warren adopts the view that "psychology is the science which deals with the mutual interrelation between an organism and its environment," and discusses an enormous variety of types of interrelation from the simplest nerve muscle response to the complications of 'conscious purpose.' In spite of the fact that it is somewhat overloaded with detail, the volume is interesting, and will doubtless prove of use to many students who are beginning the subject.

The Class-Room Republic. By E. A. CRADDOCH. London: A. and C. Black. 1920. pp. iv + 80. 2*s.* 6*d.* net.

The writer is a form master in a London polytechnic secondary school. He renounces all his offices save that of instructor, and asks his class to form a committee which shall be empowered to punish and to reward, shall have full control of discipline both inside and outside the class room, and shall be responsible not to him, but to the class itself. "For two years," says the writer, "I have not punished a single boy in any way in any of the forms in which I have established, for the purposes of my daily lessons, a class republic. I leave everything except my actual teaching in the hands of the class, with perfect confidence that all will go well. Think of the joys of teaching when all the sordid work of the executioner is left out!"

His next step is to divide his class into two, and to pit one half against the other, successfully reproducing in the class room the atmosphere of the playing-fields. "Nothing in the whole of my professional career," writes this enthusiastic teacher, "has been so fruitful in joyous surprise as the immediate success of this step that I took with much diffidence and doubt." The book is a fascinating little study of the effects of liberty and responsibility, when discreetly controlled by an able teacher.

The Psychology of Musical Talent. By Professor C. E. SEASHORE. Boston: Silver, Burdett and Co. 1919. pp. xvi + 288.

This is a popular book compiled for the benefit of the music teacher, in which articles dealing with the writer's application of psychological tests to musical ability are collected. There is, in consequence, a good deal of needless repetition and of elementary explanations which are apt to irritate the musical psychologist. The author's main objects are to show the various capacities which go to make up 'musical talent,' to indicate how they may each be tested, and to insist that the musical curriculum should be flexible, the type of training being adapted to the child's individuality. He points out the value of certain tests as drill exercises, and as a means of encouraging musical activities and of obtaining more recruits for school choruses, orchestras and glee clubs. The six principal tests he advocates are those for pitch, loudness, time, consonance, memory and imagery, all save the last of which he has standardised in the form of phonographic records. These tests each take half-an-hour to perform.

The main defect of the book is that it fails to show what is the relation of successes in these (and others of his) tests to musical ability. A promising field of investigation is here open. To know the value of a test in applied psychology, it must be tried on those endowed with the greatest and with the least musical talent. Until this has been done, and until any exceptional results have been analysed and explained, it would seem useless to encourage or to discourage musical education merely on the basis of success at such tests. It is doubtful whether artistic genius will ever be hidden or revealed owing to lack or zeal of testing; in this connexion we may note that the writer lays little or no stress on musical inventiveness in his scheme of tests. His complete scheme includes tests of auditory and motor imagery, grip, ergography, precision of movement, motor reliability, simple, complex and choice reactions, rhythmic action, time-keeping, voice register, quality of voice, etc.; but we have yet to know the relative importance of these tests for different musical requirements and ends. As a pioneer worker, Dr Seashore is to be commended for having with such patience and enthusiasm essayed to lay the foundations of vocational psychology applied to art.

Functional Nerve Disease: an epitome of War Experience for the Practitioner. Edited by Dr H. CRICHTON MILLER. London: Frowde, Hodder and Stoughton, Ltd. 1920. pp. xi + 208. 8*s.* 6*d.* net.

This book is a collection of papers by various writers, many of them well-known psychologists. Its object, as explained in the preface, is to present to the medical practitioner a simple and concise picture of the functional neuroses of war-time. Each writer takes a special part of the subject, and deals with it entirely in his own fashion, apparently without correspondence with the writer of any other chapter. The inevitable consequence of such an arrangement is to bring out most prominently the wide differences of view which exist among individual psychologists at the present day, and to demonstrate more effectively than anything else could do the sharp divisions of opinion that occur between the different schools of psychological thought. Dr McDougall, in a summary which is placed at the end of the book, admits this want of co-ordination between the different writers, but he has summarised most skilfully the points upon which they agree. That such a chapter should be necessary is sufficient comment upon the difficulty that is presented by this method of preparing a book on Psychological Medicine. The reader would be well advised to treat the book as a collection of separate papers, and not to attempt to read it straight through. The abrupt changes of style and language between the different authors makes the confusion that exists in their terminology more noticeable. Most of the authors appear to accept the Freudian theory of the causation of the psycho-neuroses, but they have not been content to accept, at the same time, the Freudian nomenclature. For example Dr Rivers introduces the word 'suppression' as being different from the word 'repression' which hitherto has included the process which he describes.

In the first chapter upon Physical Aetiology, Dr Crichton Miller brings to our notice the possibility of agreement between the materialist and the psycho-therapist. The value of this is instantly apparent, and the clear way in which Dr Miller explains the case from both points of view should make the co-ordination between the two types of practitioner much more simple than it has hitherto been.

The chapter on Differential Diagnosis by Dr George Riddoch is one of the most valuable contributions to the same problem of co-ordination that have been published of recent years.

Both in this chapter, and in a later chapter upon Regression by Dr Maurice Nicoll, the theory of Hughlings Jackson is endorsed. We have here a timely reminder of a theory which both neurologists and psychologists are constantly in danger of forgetting.

Perhaps the most interesting chapters to the practitioner are those on Regression, The Mother Complex, and Psycho-analysis. These contain very much that is of value to those who would carry out psychological investigation, and if there is anyone nowadays who doubts the advisability of analysis in cases of psycho-neuroses, he should read these exceptionally sane contributions to the subject.

Some disappointment may be felt when it is found that in the chapter on Psycho-analysis no definite formula is given for the cure of this or that particular form of neurosis, but it should be more widely recognised that psycho-analysis cannot be taught from the book, but only by the practitioner submitting himself or his patient to analysis, finding out from errors where the truth lies, and working out a personal method of his own.

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PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

General Meetings.

March 13, 1920.	Human Motives in the Light of Recent Discussion, by W. McDougall.
	A Linguistic Factor in English Characterology, by Ernest Jones.
May 8, 1920.	Speech Inscriptions in Normal and Abnormal Conditions, by E. W. Scripture.
	Camouflage in Land Warfare, by Adrian Klein.

Sectional Meetings.

(a) *Education Section.*

February 12, 1920.	The Dreams of Children in Blind, Deaf and Industrial Schools, by C. W. Kimmins.
March 9, 1920.	A Statistical Survey of Arithmetical Ability, by D. J. Collar.
April 14, 1920.	The Purposeful Act as the Unit in the Educative Process, by W. H. Kilpatrick.
May 12 & 26, 1920.	Left-handedness and Mental Deficiency, by Hugh Gordon (Joint Meeting with Medical Section).

(b) *Industrial Section.*

February 25, 1920.	The Effect of Change in Hours of Work on Output, by H. M. Vernon.
March 25, 1920.	Some of the Psychological Causes of Labour Turn-over in Factories, by G. M. Broughton.

(c) *Medical Section.*

February 18, 1920.	The Revival of Emotional Memories and its Therapeutic Value, by W. Brown, C. S. Myers and W. McDougall.
April 28, 1920.	Psychological Adaptation, by Constance Long.
May 12 & 26, 1920.	Left-handedness and Mental Deficiency, by Hugh Gordon (Joint Meeting with Education Section).

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